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# The American Historical Review

THE AMERICAN HISTORICAL ASSOCIATION

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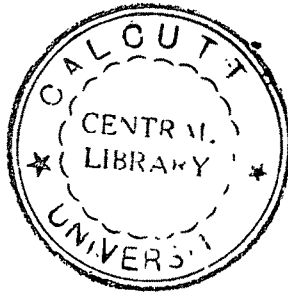
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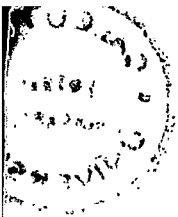
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400 A Street, S.E., Washington, D.C. 20003

1972

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EMMET LARKIN, professor of British history at the University of Chicago and author of *James Larkin, Irish Labour Leader* (London, 1965), is currently writing a history of the Roman Catholic Church in Ireland in the nineteenth century. Mr. Larkin received his Ph.D. at Columbia University, where he studied with the late John Bartlett Brebner.

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THEODORE K. RABB, an associate professor of history at Princeton University, specializes in early modern Europe. He is the author of "Sir Edwin Sandys and the Parliament of 1604" (*AHR*, 69 [1963-64]: 646-70) and *Enterprise and Empire* (Cambridge, Mass., 1967) and coeditor of the *Journal of Interdisciplinary History*. Mr. Rabb is currently writing a biography of Sir Edwin Sandys.

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# The American Historical Review

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THE AMERICAN HISTORICAL ASSOCIATION

*Cover illustration.* Elite Argentine family's summer house, built ca. late nineteenth century. (Detail from fig. 13, p. 1063.) Photograph courtesy of the Archivo General de la Nación, AG, Colección Witcomb.

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# The American Historical Review

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THE AMERICAN HISTORICAL ASSOCIATION

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EMMET LARKIN

"IF YOU KNEW," a Waterford priest wrote Tobias Kirby, the new rector of the Irish College in Rome, on January 3, 1850, "all there is to remedy, all the evil there is to check!"<sup>1</sup> "We have not had," he further explained to Kirby, referring to the decline in clerical discipline after the famine, "a Conference here since the beginning of the distress, four years now probably—& but *one* retreat all that time & everyone doing & thinking & speaking as it listeth him, & no one to prevent it." The occasion for this lament was the recent and encouraging news from Rome that Paul Cullen, Kirby's predecessor as rector of the Irish College, had just been appointed archbishop of Armagh and the accompanying rumor that the new primate had also been armed with the power of apostolic delegate by Pius IX and instructed to summon a national synod for the better government and regulation of the Irish Church. More than a quarter of a century after Cullen's arrival in Ireland, his cousin and protégé, Patrick Francis Moran, the bishop of Ossory, was able to report to Kirby in a matter-of-fact way from Kilkenny during the course of a letter that "we ended two small Missions in two of our city Churches on Sunday last, preparatory for Christmas."<sup>2</sup> "Nothing," he further explained, "could be more consoling than the great piety of our poor people. All without exception approached the Holy Sacraments." "At my Mass on Sunday in the Cathedral," Moran emphasized in conclusion, "there were about *1000 men* at Holy Communion." In the nearly thirty years that he faithfully served Rome in Ireland, Paul Cardinal Cullen not only reformed the Irish Church but, what was perhaps even more important, in the process of reforming that Church he spearheaded the consolidation of a devotional revolution. The great mass of the Irish people became practicing Catholics, which they have uniquely and essentially remained both at home and abroad down to the present day.

THE MEASURE OF CULLEN'S ACHIEVEMENT naturally depends on how much had been done to make practicing Catholics of the Irish people before

<sup>1</sup> J. P. Cooke to Kirby, Kirby Papers (hereafter K.), Archives of the Irish College, Rome.

<sup>2</sup> Dec. 19, 1877, K.

his arrival in Ireland in early 1850. What resources, in terms of plant and personnel, had been available to the Church for the encouragement and sustaining of devotional practices? And what was the character of as well as the example given by the Irish clergy to their flocks in promoting such practices? Corporately characterizing some 2,500 priests or even only some thirty bishops over a period of fifty years is obviously as hazardous as it is difficult.<sup>3</sup> And given the still raw state of the available evidence any systematic analysis of the resources of the Irish Church before the famine is as yet virtually impossible.<sup>4</sup> While the evidence is admittedly not in a condition, either quantitatively or qualitatively, to yield a consensus satisfactory to historians, it may be useful to attempt to structure a frame in which that developing body of evidence may be more intelligently researched and analyzed.

Since the quantitative problem of the number of clergy is relatively the easiest to come to terms with, perhaps it would be best to deal with it first. In 1800 there were about 1,850 priests, including some 26 bishops, in Ireland for a Catholic population estimated at 3,900,000, or roughly a ratio of one priest to 2,100 faithful. There were also in 1800 only 122 nuns in Ireland, which if reduced to a ratio divides out at the meaningless proportion of one nun to 32,000.<sup>5</sup> By 1850 the ratio between priests and people was still about one to 2,100, with over 2,500 priests available for something more than 5,000,000 Catholics. The nun population, however, had by 1850 increased thirteenfold, from 122 to over 1,500 in fifty years, and instead of one nun for 32,000 people, there was one nun for every 3,400 Catholics.<sup>6</sup> These figures, of course, in themselves are actually misleading because they mask the effects of the outstanding social tragedy in modern Irish history—the Great Famine. Between 1800 and 1840 the Catholic population had risen to 6,500,000, an increase of about 2,600,000, and probably increased another 250,000 by 1846.<sup>7</sup> By 1850, as a result of

<sup>3</sup> Emmet Larkin, "Church and State in Ireland in the Nineteenth Century," *Church History*, 31 (1962): 295-306.

<sup>4</sup> Emmet Larkin, "Economic Growth, Capital Investment, and the Roman Catholic Church in Nineteenth Century Ireland," *AHR*, 72 (1966-67): 852-84.

<sup>5</sup> Charles Vane, marquess of Londonderry, ed., *Memoirs and Correspondence of Viscount Castlereagh*, 4: 99, 172.

<sup>6</sup> *Census of Ireland, 1851* (Dublin, 1856).

<sup>7</sup> "First Report of the Commissioners of Public Instruction, Ireland," *Parliamentary Papers*, 1835, vol. 33, no. 45. The estimate of the Catholic population is based on the percentages of the various denominations given in this first religious census taken in Ireland in 1834: Population (total) 7,943,940; Catholic 6,427,712; Church of Ireland 852,064; Presbyterian 642,356; Other 21,808. According to these figures the Catholics made up nearly 81 per cent of the total population, while the combined Protestant total was about 19 per cent. If, therefore, the Catholic population in 1841 is estimated at 80 per cent of the census figure of 8,175,000 for the total population, the round number of Catholics is about 6,500,000. The Catholic populations of 3,900,000 in 1800 and 5,250,000 in 1850 are also based on taking 80 per cent of an estimated total in 1800 and a census total in 1851 respectively of 4,900,000 and 6,554,074, though both in 1800 and 1850 the percentage of Protestants to Catholics was more likely to have been somewhat higher than in 1834, the probable peak year of the Catholic population bulge in the first half of the nineteenth century. In 1861, for example, when for the first time the decennial census included figures for religious denominations, the Catholics accounted for some 78 per cent of the total population.

the famine and its aftermath, this population of nearly seven million in 1846 was reduced by some two million in four years.<sup>8</sup>

When it is realized that in 1840 there were only about 2,150 priests for a Catholic population of 6,500,000, or merely one priest for every 3,000 people, and that there were, furthermore, only about 1,000 nuns, or one for every 6,500 faithful, it becomes rather obvious that in the decade of the forties, and especially in the years before 1846, the Church in the face of incredibly adverse economic circumstances responded impressively, even if tardily, to the challenge of growing numbers by increasing the clerical population by some 400 priests and over 500 nuns—a twenty and fifty per cent increase respectively in ten years.<sup>9</sup> After 1840 and before the famine, therefore, the priests were gaining slowly and the nuns rapidly in relation to a population that was still increasing, though at a progressively declining rate. Patently, this relative increase in the clerical population meant little in practical terms before the famine, but when the population was suddenly reduced by nearly two million between 1846 and 1850 the whole clerical-lay numerical relationship was dramatically transformed, and what may have been only a short-term tendency rooted in a heroic institutional effort to increase the clerical population between 1840 and 1846, became in the next one hundred years a basic secular trend in Ireland. What emerges, then, even from this cursory analysis, is simply that before the famine any effective service on the part of the clergy was severely limited by the sheer weight of lay numbers, and that up to 1840, at least, the situation had been getting progressively worse.

But if the shortage of priests was so serious, perhaps the numerical deficiency was compensated for in some measure by the quality of their performance. Without more detailed biographical information about the nearly five thousand priests who served the Irish Church between 1800 and 1845, however, any estimate of their corporate character and conduct must remain largely impressionistic. There is, in fact, a strong bias in the available evidence in favor of extreme presentations. If, for example, one confined oneself only to reading the correspondence between Ireland and Rome in the archives of the Society for the Propagation of the Faith (*Propaganda Fide*), where nearly all the dirty Irish clerical linen was washed, the clergy might easily be characterized as drunken, disorderly, and immoral, or worse. While it is obviously impossible to present in any meaningful way more than fifty years of evidence from the *Propaganda* archives, perhaps a few examples will not only suffice to show what the nature of the evidence is, but also what the problems are in evaluating it. "I expect to leave this town tomorrow," Edward Dillon, the archbishop of Tuam, explained from Tuam in County Galway, on January 7, 1805, to John Collins, one of his priests, "and do not intend to return till Lent."

<sup>8</sup> *Census of Ireland, 1851.*

<sup>9</sup> *Census of Ireland, 1841* (Dublin, 1843).

"Previous to my departure," he warned Collins, "I cannot help reminding you of the advice I gave you when you were last at this house."

I am positively determined not to tolerate any whiskey drinking or other publick irregularities amongst my clergy. . . . Let me observe to you finally that if you wish to continue in Dunmore or to be employed in the ministry in this Diocese you must learn to sett a higher value on the sacred character with which you are invested than you have hitherto done. Particularly you must not be known to associate with such persons as Math<sup>w</sup>. Martin's or Martin's Strumpet's; much less should you church such persons. I often advised Frank Burke and his Co-ajutor he vainly tought that I would confine myself to unavailing advice, beware of meeting the fate of the former the later is tolerated merely thro necessity for a few months.<sup>10</sup>

"A Rev<sup>d</sup>. M<sup>r</sup>. Corbett, a priest of my Diocese," James O'Shaughnessy, bishop of Killaloe, reported to his agent in Rome from Ennis in County Clare, on November 27, 1815, "has been charged with adultery, and with having occasioned the separation of man and wife." "The case seemed so clear against him," O'Shaughnessy explained, "that he ought to have submitted and retired, but in place of doing so, he loudly called for a public trial."

During this trial more perjury and wickedness was practiced than in any Civil Court in the world. Some turbulent and disorderly priests made common cause with M<sup>r</sup>. Corbett, and I am informed they joined in a remonstrance to the Holy See, alledging that the sentence passed by the pious and learned D<sup>r</sup>. Wright was not founded in justice. My own humble opinion is that there was already too much of this shocking business, and that our Holy Religion would be less-wounded, and less scandal given, by not stiring the embers further.

The unfortunate woman seems within one month of her accouchment tho her husband left the country 15 months ago. If with your usual attention you would make known the circumstances to the Sacred Congregation, and put a stop to any further proceedings, it would be rendering an essential service to the Catholic Religion of this poor persecuted country.

"When anything final," O'Shaughnessy suggested, "comes to your knowledge I request to hear from you." "I am sure," he concluded encouragingly, "you will give it every necessary attention, of which I will be *mindful*."<sup>11</sup>

The third and final example of the nature of the evidence in the Propaganda archives concerning the conduct of the clergy is a long letter from James Murphy, bishop of Clogher, to Lorenzo Litta, cardinal prefect of Propaganda, dated Monaghan, April 2, 1818, thanking His Eminence for the news of the appointment of a coadjutor with the right to succeed him in Clogher, but also taking exception to the cardinal's suggestion that he should be less severe regarding several of his priests with whom he was in serious litigation at Rome. "And now permit, My Lord,"

<sup>10</sup> *Scritture riferite nei congressi, Irlanda*, 18, fol. 316.

<sup>11</sup> *Ibid.*, 20, fol. 69.

Murphy began his peroration politely, "with the most profound deference, however, for your Eminence and the Sacred Congregation, to say a few words on the treatment I conceive myself to have received from the Sacred Congregation,

Your Eminence may recollect, that in the year 1814, when heavy charges were preferred against me to His Holiness, and two appeals lodged against me, one of them by Priest Maginn, since deceased, on account of having suspended him for *Turpia in Tribunali*; the other by Priest Goodwin having published to the world, and preferred and inscribed to me a *Libellum Accusatorium*, in which he charged a highly respectable and zealous priest with having revealed the confession of his penitant, and in which he also charged two other pastors, the most respectable in the Diocese, with having cooperated in said wicked act. At that time your Eminence sent two commissions to my then metropolitan, the most Rev<sup>d</sup> Richard O'Reilly, the one to examine narrowly into the said charges and report the result to his Holiness; the other to try the appeals and pass a definitive sentence on them—Both these commissions my metropolitan executed, and after a strict scrutiny into the charges against me, he told me, he reported them unfounded and calumnious—relative to the two appeals, he pronounced definitively, as empowered, that my suspensions were just and necessary in both cases—Now what I feel for and consider *severe*, is, that I, or, indeed, any other bishop should be exposed, dragged publicly and shamefully from tribunal to tribunal and tried a second time on matters that were already definitively disposed of: for though my metropolitan erred in not depriving Priest Maginn of his parish, yet, his sentence, which bound that unfortunate man on oath never to hear the confession of a female, not only justified but even proved the necessity of my suspension—to these matters I beg leave to add, that your Eminence sent a commission to my metropolitan in the year 1816, in consequence of an appeal lodged by a Priest Duffy, a curate, and, of course, without any ordinary jurisdiction, against me, for my having interdicted him from exercising certain pastoral functions in despite of his parish priest, and for having suspended him afterwards for his contumacy, in not attending citations I sent him to appear before me, and account for his exercising all and every pastoral function in defiance of my interdict, and for his, besides, raising the people in open rebellion against their lawful pastor—Your Eminence, I say, sent such commission to my metropolitan with orders to cite the parties, and after hearing us, to report the result, together with his own opinion, to your Eminence—I, of course, obeyed the citation though labouring under infirmities and having upwards of sixty Italian miles to make, and having besides to bring necessary witnesses with me at heavy expenses, some thirty, some eighty and some an hundred miles. My metropolitan, after examining me and my witnesses in the presence of the appellant, called on him to rebut what I had proved, when he was answered by the appellant in a highly disrespectful and taunting tone, that he would not, nor would he, he said, answer a single question that he put him—my metropolitan made, of course, his report on this obvious and self evident case, and the Sacred Congregation, notwithstanding, gave it in charge a second time to the Visitor Apostolic—This I confess, I consider also *severe*.<sup>12</sup>

"It has exposed me," Murphy maintained, "to additional heavy expenses, for the appellant, Priest Duffy, nor indeed any one of the other appellants,

<sup>12</sup> *Ibid.*, 21, fols. 158-59.



though they brought on the suits never paid a single sou of the expenses attendant on the different commissions." "It has, besides," he declared further, "by putting off and prolonging the decision, given them the assurance to expose and villify me and my administration frequently, in one of the most publickly circulating journals in this kingdom, and of threatening me openly and repeatedly with civil suits; so that I may justly say with the Apostle, *Spectaculum Facti Sumus* &c—" "All this publick abuse," Murphy concluded dryly, "I bore without an answer in the hope that God will give me an account for it [in] a better world."

If, on the other hand, one turns from the Propaganda archives to a perusal of the various pious lives of the Irish clergy for the same period, the result is simply a hagiographical headache, or worse.<sup>13</sup> The truth, alas, is not even found by invoking that favorite and prudent device in such circumstances—the *via media*. What happened between 1800 and 1845 is that the character and conduct of the clergy, which certainly left a great deal to be desired at the beginning of the period, was gradually and uniformly improved. By 1830 the worst was over, since the Irish bishops with the help of Rome finally secured the upper hand over their priests.<sup>14</sup> From 1830 the improvement, though still uneven, depending as it did on the character and strength of will of each individual bishop, was at all events steady. The improvement, however, does not appear to have been simply a function of the bishops' authority in time; it seems to have been a reflection of geographical circumstances as well. The improvement was most rapid and sustained in the ecclesiastical province of Dublin, while the deportment of the clergy in Cashel, Armagh, and Tuam was less and less satisfactory from one to the next.

- On the occasion of the funeral of the archbishop of Dublin, Daniel Murray, at the end of February 1852, William Meagher delivered an oration in which he reflected on the practical improvement of the Catholic population of Dublin. He graphically described the conditions prevailing in Dublin some forty-two years earlier when Murray had been raised to the episcopal dignity.

<sup>13</sup> A critical bibliography of the numerous pious lives of Irish bishops, priests, monks, and nuns is also beyond the scope of this article. An honorable exception to this general charge of hagiography, however, must be noted in W. J. Fitz-Patrick's very fine Victorian, two-volume biography, *The Life, Times and Correspondence of the Right Rev. Dr. Doyle, Bishop of Kildare and Leighlin* (Dublin, 1880). In setting a lower limit in this biographical spectrum, the best example is perhaps Peadar MacSuibhne's more recent three-volume effort, *Paul Cullen and His Contemporaries* (Naas, 1961–65). For a partial list, at least of those biographies that range between the upper and lower limits set above, see the bibliography in T. J. Walsh, *Nano Nagle and the Presentation Sisters* (Dublin, 1959).

<sup>14</sup> The milestone in effecting the better conduct of the clergy by increasing the authority of the bishops was the simultaneous holding of diocesan synods in the four dioceses that made up the province of Dublin in the third week of July 1831. For an account of the background to the meetings, see William Meagher, *Notices of the Life and Character of His Grace, Most Rev. Daniel Murray, Late Archbishop of Dublin* (Dublin, 1853), 128–31; for the legislation of synods, see R. T. McGhee, *Diocesan Statutes of the Roman Catholic Bishops of the Province of Leinster* (London, 1837); for an excellent account of a reforming bishop, James Doyle (1819–34), see Fitz-Patrick, *Life, Times*, 1: 101–32.

The morals of the people of Dublin, Catholics among the rest, were hideously corrupted. The riches daily scattered through her streets in handfuls, to purchase the luxuries of an opulent, and profuse, and dissolute aristocracy; the easy and plentiful earnings of flourishing manufacture, and of extensive and successful commerce, were seized every hour, through a series of years, for indulgence of vilest libertinism, and wildest extravagance. Vices, too gross to be more than alluded to, stalked through the streets shamelessly—the drunkard raved without obstruction, and the blasphemer shouted his impiety, and the gambler squandered in nights of dissipation what his days of toil had accumulated. And, strange to say, and suggestive of many a sad and solemn reflection, there was in our city as large an amount of physical wretchedness, particularly among the lower ranks, then as now—as much squalid poverty—as much shivering nakedness—as much famine-stricken emaciation—as many ruined families—as many houseless orphans! Vice did more to fill the town with the agonies of human suffering than famine, and plague, and abject poverty have wrought in these latter days of woe. Flatter not yourselves, My Brethren, that these excesses and their direful effects were confined to sectarians; they were as rife, if not more so, amongst ourselves. Nor, unless by some standing social and religious miracle, could it be otherwise. Amid opportunities so numerous—examples so seductive—temptations so violent—with but a handful of clergy and a dozen small, mean, and incommodious chapels to second the proverbial faith and innate pious tendencies of the people, what wonder that the multitude was hurried away in this torrent of iniquity? And the mortifying truth is, that in Dublin, at the period alluded to, amid many Catholics there were but few practical Christians; very few whose lives supplied that substantial and only unerring proof of profitable attachment to the faith—the constant and regular frequentation of the holy sacraments. As the climax of her griefs religion had to weep for the first time, perhaps, in this land, over the faltering fidelity and submission of many a son, led astray by the phrenzy of recent revolution, and the false liberality of the day, and the desolating philosophism of France.<sup>15</sup>

There appears to be, moreover, a correlation between the conduct of the clergy in these ecclesiastical provinces of Dublin, Cashel, Armagh, and Tuam and their relative wealth as well as the extent to which each was urban and rural in terms of Catholic population. Furthermore, though the evidence is still very sketchy, the clergy appointed to the town parishes appear to have been better educated than those assigned to the country parishes.<sup>16</sup>

<sup>15</sup> Meagher, *Notices*, 11-12.

<sup>16</sup> "Report from the Lords Committees appointed a secret committee to enquire into the State of Ireland," 1825, *PP*, vol. 7, no. 521, pt. 2, Minutes of Evidence, p. 569. Mortimer O'Sullivan, a Church of Ireland clergyman, was asked, "Are you acquainted with the early habits of life, of persons who afterwards become Roman Catholic priests?—Yes. / From what class of life are they generally taken?—I think generally speaking from the lower orders. My connection with an endowed school, gave me an opportunity of knowing more particularly. / Of course you have been acquainted with some who have afterwards gone to Maynooth?—Yes. / Have you had any occasion to observe what have been the effects of a collegiate education upon those persons?—It appeared to me to leave them, with respect to their moral qualities and their political prejudices, just what they were before they had gone there. When I speak of the lower orders, I mean the poorer classes; there are some of a higher order; and that church shows great judgement in disposing of her clergy; those of better manners and better information are generally placed in the towns: and those persons who are from their habits and from their education less fitted to appear in public, are left in the country parts."

But the application of episcopal authority, the relative wealth of the Catholic communities, the extent of urbanization, and the educational level of the clergy were not the only determinants of social behavior. The moral and social values of the community and the pressure the community applied in terms of what it considered to be right or wrong also affected clerical conduct. The principal vices among the clergy were drunkenness, women, and avarice. Interestingly enough, while this seems to be the order of their importance among the bishops in their efforts to impose discipline, it does not appear to be the order of their importance either before or after the famine as far as the laity who cared were concerned. Among a land-hungry and poverty-stricken peasantry avarice was the deadliest of the deadly sins, while lust and drunkenness were viewed with a more understanding, even if disapproving, eye.<sup>17</sup> The seriousness of the problem of clerical avarice vis-à-vis the faithful, for example, was certainly reflected in early nineteenth-century Ireland in the need of the bishops of the province of Dublin to set up by statute a uniform tariff for clerical dues at their diocesan synods in the summer of 1831. The tariff, however, not only gives a comprehensive glimpse of what was thought to be a fair and proper remuneration for the various services rendered by the clergy but also details an interesting summary of the clergy's sources of income.

|   |   |    |   |
|---|---|----|---|
| 1. Baptism—shopkeepers and farmers .....  | 0 | 5  | 0 |
| Do. poor labourers .....  | 0 | 2  | 6 |
| 2. Marriages—shopkeepers and farmers .....  | 2 | 0  | 0 |
| 3. Licenses for do .....  | 0 | 10 | 0 |
| Marriages for poor labourers .....  | 1 | 0  | 0 |
| Licenses for do .....   | 0 | 5  | 0 |
| 4. Masses for dead sung, to shopkeepers and farmers—parish priest .....   | 0 | 15 | 0 |
| Every other priest .....  | 0 | 10 | 0 |
| Any other priest who does not officiate .....   | 0 | 5  | 0 |
| 5. Masses, not sung .....   | 0 | 10 | 0 |
| Masses for dead, to poor labourers .....  | 0 | 5  | 0 |
| 6. Private masses .....   | 0 | 2  | 0 |
| 7. Collections after marriages—these must be voluntary according to the stations of the parties, they may vary from £1 to £100, or more.  |   |    |   |
| 8. Dues at stations—these Dr. Murray informs us are the chief support of the priests; they cannot be called voluntary, for custom makes them compulsory; they are contributed by every person who can give anything, and vary from one shilling to five, as the Editor is informed, say the lowest average from the population of Ireland who attend stations and confessions ..... | 0 | 1  | 6 |

<sup>17</sup> The problem of exorbitant clerical dues and the resistance of the laity to them had been an issue in Ireland from at least the latter part of the eighteenth century. See Robert E. Burns, "Parson, Priests and the People: The Rise of Irish Anti-Clericalism, 1785-89," *Church History*, 31 (1962): 151-63.

9. Fortuitous emoluments—of these, at least one great source of revenue, is masses for delivering the souls from purgatory of those who are dead and buried, left either by will or given by relations of deceased, or, what is very common, given by the poor creatures themselves, for masses before they die, and to benefit their souls when they are gone,—of these it is impossible to calculate the amount.
10. The collecting of corn from the people. This is sometimes commuted for money, and is valued at 1s. 6d. per house, or more.<sup>18</sup>

More particularly, the parishioners from Kilcommen Erris, near Belmullet in County Mayo, in the diocese of Kilalla, petitioned the pope in early 1840 about the abuses to which they were subject from the local clergy. Their petition comprised some fifteen heads of complaint, and like so many other documents in the Propaganda archives it was the product of a fierce local struggle for power, with the contending parties prepared to say and to write the worst about their opponents. The crucial aspect of this document, therefore, is not indeed whether the charges made in it were necessarily true, but rather that the charges made in it are a serious comment on what the mores and values of the person or persons who wrote it actually were.

To Our Most Holy Father in God Pope Gregory the XVI Successor of Saint Peter and Vicar of Jesus Christ upon Earth. . . .

6thly that the poor uneducated peasantry of the parish generally feel not only scandalised but actually horrified at the not merely tyrannical, but unchristian like conduct of the Rev<sup>d</sup> Mr. Conway towards them during the Confessions. When a poor, but pious, humble, contrite penitant presents himself before the altar of reconciliation bemoaning his offences, and with devout compunction soliciting to be admitted to the Paschal or Christmas distribution of the Bread of Life—if he has not money to propitiate the avaricious ire of the Rev<sup>d</sup> Mr Conway, he is not quietly dismissed as being too poor and contemptible for enjoying the Celestial benefits; but he is scolded, villified and threatened “D’air Cunnial De Mur Sagart”. “By the obligation of God as a Priest,” he will have revenge, if it were to run for seven, ten, fifteen, or 21 years. . . .

10thly that old Anthony Burke who lives with his daughter and son in law in Muinnaba, and whose aged wife lived with another daughter in Claggeh—did upon the death of his wife offer 2<sup>s</sup>/6<sup>d</sup> to the Rev<sup>d</sup> Mr. Conway to have mass said for the soul of his departed wife;—but that Mr. Conway not only refused the money but in a paroxysm of violence proclaimed Burke from the Altar, did ring the bell with rage—and invoke a horrible imprecation upon him and his worldly substance, for offering him 2<sup>s</sup>/6<sup>d</sup> to say Mass for the poor woman though no priest before Mr. Conway charged more than one shilling. The result was, that the Congregation would have slaughtered him [“him” crossed out] each other were it not for some peaceable characters who mollified the rage of the exasperated people. . . .

<sup>18</sup> McGhee, *Diocesan Statutes*, xli-xlii.

12thly we have had, through the pious zeal of Rev<sup>d</sup> Neal McNulty, the walls of a good chapel 80 feet in length and 30 in breadth built for the last sixteen years, and not withstanding the number of clergymen that passed through this parish during that period, and collected great sums of money from us, for the ostensible purpose of roofing the Chapel, yet they have taken away our money and left us these sixteen years without a temple of worship to put our heads into or to screen us from the inclemency of the weather and although Dr Feeny has been appealed to against these clerical plunders, he has not ordered the money to be refunded to James O'Donel Esq<sup>r</sup> the Treasurer of our Chapel Committee.<sup>19</sup>

In order to sum up here, however, on the subject of clerical avarice, which is yet another subject, a long account of the situation in Ireland by T. Chisholme Anstey, an English Catholic, apparently to the secretary of Propaganda, Giovanni Brunelli, from London on November 17, 1843, is certainly worth our attention. In his account Anstey, who appears to have been well acquainted with the clergy and conditions in the province of Connaught, maintained,

it is well known in every part of Ireland with which he is acquainted, and to the best of his belief also in other parts thereof, that however well disposed a parish priest or curate may be to relieve his parishioners or some of them from grievous and oppressive payments of the kinds specified ["Tithes, church dues, oblates, stock fees, money for repairs of fabric"], the relatives, (often very numerous) of such ecclesiastics are certain to obstruct the concession by clamorous complaints and remonstrances against his unkindness to his own flesh and blood, who by his ill timed liberality he is defrauding of their hopes of succession to his property after his death and of occasional contributions out of the same during his lifetime, and to which succession and contributions they in the popular opinion as well as in their own have a kind of equitable claim, founded upon the consent, which his family is supposed to have given in the first instance to his being withdrawn from field labor and domestic service in order to go to the seminary; and, that the fear of such complaints, remonstrances and appeals to popular opinion hath the effect of making the priests to be watchful and austere in the exact and undeviating levy of their aforesaid dues, is apparent from the greatness of their incomes; that is to say in Connaught, which is the cheapest part of Ireland, and where money is twice as valuable as it is in London, there are very few parish priests, if any, whose incomes are less than 200 sterling per annum although not one farthing of such incomes is appropriated by either priest or proper to any other purpose than the mere support of the priest. But in most parishes the income is very much higher and ranging to £500 and upwards per annum; insomuch that it is a vulgar and proverbial saying throughout Ireland that the best or richest matches are to be had with the kindred of priests and that their farms are certain to be well stocked and furnished.<sup>20</sup>

<sup>19</sup> *Scritture riferite nei congressi, Irlanda*, 28, fols. 634-35. The petition is signed by James O'Donel as "Chairman of the Parochial Committee" and Hugh Joseph O'Donel as "Secretary." Three other petitioners signed their names, but nineteen others made their "X" mark and were signed for.

<sup>20</sup> *Ibid.*, fols. 121-45.

Anstey then went on to explain that there was a fixed tariff on burials, masses for the dead, churching women, etc., below which an offering might not fall, but might exceed it. Baptisms, he reported, were 2s. 6d., while marriages were a pound or a guinea, in addition to the money collected for the priest at the marriage feast. One such collection, Anstey noted, was £25, though no one there was above the rank of farmer. If the people involved were poor, Anstey admitted, the clergy would perform the required service gratis, but in order to test the truth of their allegations of poverty, the priests were in the habit of announcing in the chapel, church, or meeting house the names of those who had gratuitously received their services. "And, however poor the Irish peasant may be," he added, "he is rarely disposed to accept the exemption upon such humiliating conditions." The result of all of this had been, Anstey continued, that secret societies had been formed, and "by means of such associations they have from time to time endeavoured, and still are endeavouring, to compel the priests to agree to a more moderate tariff of dues and to compel the people to abstain from paying them any dues except according to the tariff appointed or proposed." Anstey then turned to the bad Irish habit of "stations," where the priest would designate the houses of various parishioners who were relatively well off as the place where he would hear confessions and say mass that week for all those in the immediate area. He complained that mass was being offered in "cabins" rather than in "chapels" because the fees were greater. The "station," moreover, he pointed out, was obliged to offer hospitality and had to cater to the priest's choice of tradesmen and victuallers. The Irish priests, Anstey further complained, not only did not keep to the rubric and practice of Rome in the Mass, but their sermons were of poor quality, and "the ignorance of the people in matters of Religion is frightful, and, in particular, that the doctrine of the Trinity is rarely known or ever heard of among them, much less the doctrine of the Real Presence and other articles of Faith." After all this and a good deal more Anstey finally concluded by advising the Propaganda that the only hope for religion in Ireland was for the pope to send a legate with power to correct the many abuses. Clerical avarice, however, legate or no, whether in terms of the clergy farming or grazing large tracts of land in their brothers's or nephews's names or in squeezing the people for alms and dues was, after drunkenness, the most difficult of the deadly sins for either the bishops or the laity to check.

BUT HOW MANY of the laity really cared? The best way to begin to answer that question is to determine how many people actually attended church. While it is certainly true that all those who attended mass religiously were not necessarily active in the concerns of their church, knowledge of

the numbers who attended is at least helpful for setting an upper limit on those who were concerned. The figures on church attendance in pre-famine Ireland indicate that only thirty-three per cent of the Catholic population went to mass.<sup>21</sup> This is all the more remarkable in that in something less than fifty years church attendance would increase to over ninety per cent, and so it has continued down to the present day.<sup>22</sup> Why attendance was relatively so low in pre-famine Ireland is obvious. There were not enough priests and there were not enough churches, or, more particularly, not enough seating space in the existing churches to accommodate those who might be inclined to attend to their religious duties. If, for example, all the priests in Ireland celebrated the two masses they were allowed on a given Sunday in 1840 there would have been 4,300 masses for 6,500,000 people, or one mass for every 1,500 people in attendance, and there were no chapels and very few churches in pre-famine Ireland that would accommodate a thousand worshippers.

This deficiency was offset to some degree before the famine by the widespread practice of "stations." Baptism and marriage were also frequently celebrated in private houses rather than in churches. These practices were generally frowned on by those who were attempting to reform both clergy and laity and increase devotional zeal. The complaints of the reformers, who were concerned about the abuses attendant on the system, had mainly to do with the exorbitant "offerings" extracted by the clergy for the administration of the sacraments and the undignified if not unholy celebration of sacred rites in profane places. James Maher, writing from Carlow to his nephew Paul Cullen in Rome in early January 1842, asked "Could not Rome do something to stimulate the zeal and watchfulness of the Bishops: the holding of Stations for Mass and Confession at private houses is the very worst system. Wretched filthy cabins have been lately honored with stations." "The people," he explained,

cannot be instructed. The Priest no matter how zealous cannot do his duty. The young clergyman is brought into contact with his female penitents. The result is confessions are often invalid or sacrilegious. It is almost impossible that the poor country people in the circumstances could disclose their sins. Struggling with their natural reluctance to avow their guilt, and fearing at the same time to be overheard by those who are pressing around the Priest, who cannot utter a word of encouragement to the sinner, except in the lowest and therefore intelligible [*sic*] whisper that can be expected.

"Could not Rome," Maher suggested again, "induce the Bishops to change the system? Stations in the chapels have been recommended in the Statutes

<sup>21</sup> David W. Miller, "Religion and Social Change in Pre-Famine Ireland," unpublished paper, p. 3. Since the writing of his paper Professor Miller has revised his estimate of church attendance by Roman Catholics upward to about 40 per cent, but he is still "prepared to state flatly that the prevalence of extraordinary religious devotion evidenced by extremely regular church attendance is a post-Famine phenomenon."

<sup>22</sup> Jean Blanchard, *The Church in Contemporary Ireland* (Dublin, 1963), 29-31.

for this province. But the recommendation has proved a dead letter." "We owe much to Rome," he assured Cullen in conclusion, "and if she would help us to this reform, we would be more deeply her debtor."<sup>23</sup> Nearly all the synods, provincial and national, between 1830 and 1875 had statutes disapproving of "stations," and even though Rome eventually added her proscription as requested by Father Maher, the practice died very hard, especially in the south and west where it still survives in some places.<sup>24</sup>

Before the famine, then, despite severe limitations in plant and personnel, there was a small but perceptible change and increase in devotional practices in Ireland. Why this was so had a great deal to do with the enthusiasm and hope generated by the moral and political reform movements of Father Mathew and Daniel O'Connell. Both the Total Abstinence Society and the Repeal Association grew up in a period heavy with gloomy forebodings of impending disaster as bad harvest succeeded bad harvest, prices for foodstuffs continued to fall rapidly, and emigration mounted. Literally millions took Father Mathew's temperance pledge as the production of Irish whisky fell from 12,296,000 gallons in 1839 to 5,546,283 gallons in 1844. As Daniel O'Connell enrolled the "nation" in his Repeal Association his "monster meetings" numbered in the hundreds of thousands of people.<sup>25</sup> Essentially these were both revival movements, which created not only an enormous enthusiasm but, because of the underlying anxieties created by population pressure and land hunger, also contributed greatly to an already heavily charged emotional atmosphere. In the early 1840s, therefore, there already were manifest signs of that devotional revolution, which Paul Cullen would proceed to help make and consolidate some ten years later when he would arrive in Ireland as archbishop of Armagh and apostolic delegate.

On the occasion of a papal jubilee in 1842, for example, when the penitents were offered special indulgences if they would but confess their sins and come to Christ, the bishop of Cork wrote to Cullen in Rome asking the rector of the Irish College to secure for him additional faculties to dispense in terms of sins especially reserved to the pope. "Sinners," John Murphy explained to Cullen in May 1842, "who have for years lived in fornication, adultery, incest & have recourse to the tribunal of penance" are coming in in droves. "I have a melancholy list of 64 couples," he added sadly,

who in obedience to my commands have separated *a thora*; for where there is abject poverty, with a numerous brood we cannot insist on separation *a mensa*. The Jubilee is open in only one Parish, how numerous will be the blacklist before the conclusion of it in the whole diocese—it would be endless to enter into the minute details of every case.<sup>26</sup>

<sup>23</sup> Jan. 2, 1842, Cullen Papers (hereafter C.), Archives of the Irish College, Rome.

<sup>24</sup> Blanchard, *Church in Contemporary Ireland*, 32.

<sup>25</sup> Sir James O'Connor, *History of Ireland, 1798-1924* (London, 1925), 1: 301; L. J. McCaffrey, *Daniel O'Connell and the Repeal Year* (Lexington, 1966).

<sup>26</sup> May 29, 1842, C.



Some nine months later James Maher again reported to his nephew in Rome from Carlow. "I forgot, tho I intended it to tell you of the wonderful success of the Missionaries in Athy." "A visit from Father Mathew," he explained, "would not have put a greater number in motion. Hundreds remained all night in the Chapel, and many remained in town away from their homes from 5 and six days waiting an opportunity of confessing." "This extraordinary movement," Maher further noted, "has confirmed an old opinion of mine that we do not always afford the people an opportunity of general confession when required. In fact we have not half Priests for the wants of the Mission, and a very considerable proportion of the Parrochi leave the confessional almost entirely to the curates." The following August Cullen's sister Margaret informed him that they had had the "Missioners" in Carlow town for the last five weeks, and that it "would be impossible for me to describe the enthusiasm of the people." If the missioners were angels from heaven, she added, they could not be more venerated. Work was at a standstill, while people followed them around all day and crowded "in *hundreds* to the Confessionals, many very many who had never before been there." The missioners preached three times a day in the chapel, which was "crowded to suffocation." "What a pity," she finally concluded, "we have not more Priests in the Parish. I fear a great deal of their labours will go for nothing. Where is the opportunity for the *bulk* of the parish to approach the Holy sacraments."<sup>27</sup>

Indeed, the "bulk" of the Irish people in the 1840s never did have the opportunity to approach the sacraments. In writing his customary annual letter in October 1851 to the cardinal prefect of Propaganda, for example, Michael Jones, a former student of the Propaganda's Urban College in Rome, complained about "an almost general neglect in giving the People the necessary knowledge of the Faith, the Commandments and the Sacraments." "The Irish People," Jones explained to Cardinal Frasoni, "are very good, but much neglected in every way by both the Civil and Ecclesiastical Government, more by the latter than the former." It was time "for the Ecclesiastical authority to put an end to the present state of things." The priests who received training at the National Seminary at Maynooth, did "not receive in the College any notion how things should be. The old system of the days of Persecution, the Catacombs, and the Caves is all that they know."<sup>28</sup> That the Irish people were receptive and might

<sup>27</sup> Feb. 21, Aug. 30, 1843, C.

<sup>28</sup> Michele Jones to Giacomo Cardinale Frasoni, Oct. 9, 1851, *Scritture riferite nei congressi, Irlanda*, 30, fols. 720-21, Archives of the Society for the Propagation of the Faith, Rome. The original letter is in Italian. See also K., Feb. 28, 1858, for a letter from John Kyne, chaplain at Alum Bagh, Lucknow, Oude, East India, to Thomas Grant, bishop of Southwark (London) and Catholic chaplain-general, regarding Kyne's work among the British troops, who were mainly Irish and presumably Catholic. "I am anxious first of all to communicate to your Lordship the pleasing fact that even here in India I can bear testimony from personal experience, to the good effect produced by the Mission given last year to the soldiers at Chatham. In fact the recruits, who arrived here last month from Chatham are the *only* persons of whom it can be

have made excellent evangelical material to work with is certainly given credence by the unusual success of the sporadic attempts made, but the resources available for a religious and moral revival on a national scale were too slender in the face of the number of people.

What achievement there was before the famine, then, was largely confined to that "respectable" class of Catholics, typified by the Cullens and the Mahers in Carlow, who were economically better off. Since this class generally survived the famine intact, while the "bulk" of the cottiers, laborers, and paupers were swept away by starvation, disease, and emigration, the Church actually had a stronger devotional nucleus relative to absolute numbers in 1850 than in 1840.<sup>29</sup> This nucleus, furthermore, would come to count for more with every succeeding year because the remaining subsistence population was gradually liquidated by a continuing emigration sparked periodically by the fear of starvation. When Paul Cullen arrived in Ireland he therefore had a potentially more favorable situation than has been generally supposed. He also patently derived very great advantage from the psychological impact the famine had on those who remained in Ireland. The growing awareness of a sense of sin already apparent in the 1840s was certainly deepened as God's wrath was made manifest in a great natural disaster that destroyed and scattered his people. Psychologically and socially, therefore, the Irish people were ready for a great evangelical revival, while economically and organizationally the Church was now correspondingly ready after the famine to meet their religious and emotional needs.

THE PROBLEM OF CHARACTERIZING the making and consolidating of this devotional revolution is somewhat simplified by the fact that the period begins with the first National Synod of Thurles in 1850 and ends with the second National Synod of Maynooth in 1875. The first Synod of Thurles was primarily concerned with the proper administration of the sacraments

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said, that they complied with the Easter obligation. With this exception, the rest of the poor soldiers were in a most pitiable state. The vast majority had never in their lives received the Holy Sacraments. And their ignorance of even the first principles of religion was truly astonishing with all my experience I was never in my life so taken by surprize. If I had heard it from others, I could not have believed it possible. Yet 'with all that they are not bad materials to work upon—I believe, notwithstanding the general impression to the contrary, that on the whole as much may be made of them as of any other class of Christians." By way of proof Kyne explained that in three months he had brought upward of a thousand to the sacraments and that every morning he had between thirty to fifty communicants, while every evening he recited the rosary and gave religious instruction.

<sup>29</sup> John Kepple to Kirby, Sept. 2, 1852, K. Writing from Ballyhea, Charleville, where he was parish priest, Kepple noted, "There is not in the County of Cork a finer country than this. The population of my parish is not very large, the poorer portion of it (as everywhere else through out Ireland) has been swept away by the Famine fever emigration &c &c. The farmers tho' not numerous are very respectable, and comfortable but in consequence of the failure of crops, and the thinness of the people our emoluments here are inconsiderable, however I don't complain."

and regulating more closely the lives of the parish clergy.<sup>30</sup> In the statutes the clergy were exhorted to administer the sacraments more often and only in church, except where it was impossible, and to encourage the laity to better lives by the clergy's own good example. The bishops were assigned the responsibility by the synod of seeing that these reforms were carried out in their respective dioceses. Twenty-five years later the Synod of Maynooth reiterated mainly what had already been decreed in 1850 and in enlarging upon the statutes further increased episcopal control and authority.<sup>31</sup> The making of the law, however, proved to be one thing, and the enforcing of it quite another. The first Synod of Thurles had made it quite clear that the Church was to be reformed from the top down and that the responsibility for enforcing that reform should fall to the bishops. As apostolic delegate, however, Cullen had very real difficulties in keeping the Irish bishops up to the mark. The problem was not only that a large number of bishops were set in their ways and naturally averse to reform, but that the bishops also had serious differences with regard to educational and political matters, and their quarrels in these areas seriously inhibited their efforts at pastoral reform.

Cullen, however, was not only a reformer but a very effective ecclesiastical politician, and with the support of Rome, especially in episcopal appointments, the Irish Church was reformed by him in his generation. His method was to deal with one principal issue at a time, while trying to contain the worst effects of the other issues. His fellow archbishops of Dublin, Tuam, and Cashel, for example, each respectively represented educational, political, and pastoral problems for Cullen. He mobilized Tuam and Cashel against the educational policies concerning the Queen's Colleges, advocated by the venerable archbishop of Dublin, Daniel Murray, who was strongly supported by a minority of the bishops. When Murray died in the winter of 1852 Cullen was translated by Rome from Armagh to Dublin and his nominee appointed to replace him in Armagh. The opposition among the bishops, without the effective leadership of Murray, was quickly isolated and eventually crushed. Cullen then turned to the problem posed by the archbishop of Tuam, John MacHale, and the involvement of the clergy in secular politics, particularly the Tenant League. Supported by Armagh and Cashel, and with Rome's aid, MacHale was also eventually isolated, and if not crushed, he was at least effectively contained. Finally Cullen tackled the problem of pastoral reform in the province of Cashel, which was most difficult because most of the bishops of that province strongly supported the custom of "stations." Cullen once again undermined the opposition to him by having only those who agreed

<sup>30</sup> *Decreta, Synodi Nationalis Totius Hiberniae Thurlesiae Habitae Anno MDCCCL* (Dublin, 1851).

<sup>31</sup> *Decreta, Synodi Plenariae Episcoporum Hiberniae, Habitae Apud Maynutium, An. 1875* (Dublin, 1877).


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## THE APPARITION AT KNOCK CO MAYO.

AS SEEN ON AUGT 21<sup>ST</sup> 1879. [ EVE OF THE OCTAVE OF THE ASSUMPTION ]

MANY MIRACULOUS CURES HAVE BEEN EFFECTED THERE SINCE THE ABOVE OCCURRENCE.

THIS VIEW WAS TAKEN ON THE SPOT BY W. COLLINS.

AND SUBMITTED TO, AND APPROVED OF BY THE SEVERAL PERSONS WHO SAW THE ABOVE.

About 7:30 on the evening of the 21st of August, 1879, the vigil of the octave day of the Feast of the Assumption at the parish church of Knock, Co. Mayo, "an apparition of Our Blessed Lady, wearing a large brilliant crown and clothes in white garments was distinctly seen by some fifteen persons at the south gable wall of the church. Our Lady is described as having her hands raised as if in prayer and her eyes turned towards heaven. On her right hand was St. Joseph, his head inclined towards her, and on her left was St. John the Evangelist, attired as a bishop, his left hand holding a book and his right hand raised as if in preaching. To the left of St. John was an altar on which stood a cross and a Lamb, about 8 weeks old. . . . The gable wall where this manifestation was seen was covered with a cloud of light and the vision lasted for fully two hours." Liam Ua Cadain, *Venerable Arch-Deacon Cavanagh, Pastor of Knock, 1867-1897* (Dublin, 1955), 69. Illustration courtesy of National Library of Ireland.

with his reform principles succeed to bishoprics in that province, and gradually but relentlessly those bishops who were reluctant to change their ways in the rest of Ireland were replaced by Cullen's more energetic and aggressive nominees. By 1875, therefore, there was hardly a bishop in Ireland, except MacHale, who did not zealously promote pastoral reform in his diocese, whatever his educational and political views were. Actually this resolution of the distribution of power in the Irish Church in favor of Cullen was not nearly as smooth or inevitable as it may appear from this oversimplified account, but what is most important to understand is that this resolution of power was absolutely necessary to the making and consolidation of the devotional revolution that took place.

In the twenty years following Cullen's arrival in Ireland the number of priests was increased by some seven hundred, or nearly twenty-five per cent, to a total of about 3,200, while the Catholic population declined from five to four million, or a ratio increase of one priest to 2,000 people to one priest to 1,250 people in 1870. The nun population increased even more rapidly over the same period. In 1850, for example, there were only some 1,500 nuns in Ireland, while in 1870 there were more than 3,700, or an absolute increase of 2,200, and a ratio increase of 1:3,300 in 1850 to 1:1,100 in 1870.<sup>32</sup> Not only were the numbers of clergy relative to the population rapidly appreciating, but it also appears that their quality was improving over the same period. The amount of dirty clerical linen washed in Rome appears to have decreased, as did the volume of litigation between the bishops and their priests. The improving quality of the clergy, moreover, is not only testified to by their really prodigious energy in building churches, schools, seminaries, convents, and parochial houses, but their conduct and learning was certainly improved by conferences, retreats, synods, and the erection of cathedral chapters, as well as by the annual or triennial visitations by their bishops depending on the size of their dioceses.

In extending their increasing zeal and piety to the laity the clergy centered their attention on the sacraments, and especially on the sacraments of penance and Holy Eucharist. Confession and communion, which usually had been associated with a practicing Catholic's Easter duty in pre-famine Ireland, now became much more frequent. To encourage the laity, missions were held in nearly every parish in Ireland in the decade of the fifties. Pastoral gains thus made were consolidated by the introduction of a whole series of devotional exercises designed not only to encourage more frequent participation in the sacraments but to instill veneration by an appreciation of their ritual beauty and intrinsic mystery. The spiritual rewards, of course, for these devotional exercises were the various indulgences, which shortened either the sinner's or the sinner's loved one's time of torment in purgatory. The new devotions were mainly of Roman

<sup>32</sup> *Census of Ireland, 1871* (Dublin, 1875).

origin and included the rosary, forty hours, perpetual adoration, novenas, blessed altars, *Via Crucis*, benediction, vespers, devotion to the Sacred Heart and to the Immaculate Conception, jubilees, tridiums, pilgrimages, shrines, processions, and retreats. These devotional exercises, moreover, were organized in order to communalize and regularize practice under a spiritual director and included sodalities, confraternities such as the various purgatorian societies, the Society of St. Vincent de Paul, and Peter's Pence as well as temperance and altar societies. These public exercises were also reinforced by the use of devotional tools and aids: beads, scapulars, medals, missals, prayer books, catechisms, holy pictures, and *Agnus Dei*, all blessed by priests who had recently acquired that privilege from Rome through the intercession of their bishops. Furthermore, this was the period when the whole world of the senses was explored in these devotional exercises, and especially in the Mass, through music, singing, candles, vestments, and incense.

THE EVIDENCE IN SUPPORT of this vast social change, of course, is too diversified and complicated to be presented coherently here. Still, in order to acquire perhaps at least the flavor of that phenomenon, if not its extent, it might be useful to follow selectively the career of the man who spearheaded that change in the initial years. "I was in Armagh," Cullen reported shortly after his arrival in Ireland to Tobias Kirby in Rome, "and saw nearly all the clergy." "The old Cathedral," he explained, obviously scandalized, "is awfully bad—the priests use only *one* tallow candle on the altars at mass in the Cathedral. Imagine what it must be elsewhere." Three months later Laurence Forde, Cullen's master of ceremonies at the Synod of Thurles, reported to Kirby that the opening day's solemnities were a grand Roman success with high mass being celebrated *alla* Palestrina, complete with assistant priest, deacon, and subdeacons. "I gave the seventh candlestick," Forde assured Kirby,

to Dr. Cullen at the Mass. I was not quite certain if it should be so, but I acted on your advice. It is not without its effect—I think it useful to go in some things even a little beyond strict practice for the sake of letting the bishops and clergy see the full solemnities of the Church.<sup>33</sup>

Some two years later, when he had been translated to Dublin, the richest and most influential diocese in Ireland, Cullen still was concerned about setting the proper tone. "We commenced the Jubilee here yesterday," he reported to Kirby in October 1852, "I sang more meo high mass attended by all the Chapter and dignitaries of Dublin." Mr. Faber, the celebrated English priest, "preached a magnificent sermon by far the most eloquent I ever heard," giving "a new tone and a Catholic one to Dublin." "We have

<sup>33</sup> May 21, Aug. 22, 1850, K.

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the *40 ore*," he noted further, referring to his pro-Cathedral, "at Marlboro St. with great pomp and magnificence. The Church is ornamented with damask, a great machine erected and what is better crowds of people are attending. Deo Gratias. The devotion will be continued through the entire three months of the jubilee." He finally assured Kirby in Italian that he would soon see "the fruits of our preaching." Two months later, on December 8, in a letter headed "Evviva Maria," Cullen again reported to Kirby that the jubilee, the forty hours, and the novena in process were all very successful. "Here," he added even more significantly, "we are trying to enrol a large missionary body before next summer to wipe out the proselytizers everywhere." "It is necessary to see," Cullen concluded prudently in Italian, "if it will be successful, and then I will write to Propaganda. The Jesuits, Dominicans, Carmelites, Vincentians, Redemptorists, secular Priests will all join together—but."<sup>34</sup>

"The Jubilee," Cullen again assured Kirby less than two weeks later, on December 20, 1852, "has succeeded beyond all hope. All the churches are crowded with people trying to go to confession. Were the priests ten times as many as they are they could not hear them all." "I have done nothing lately," he added, breaking into Italian,

but to cure invalid marriages<sup>3</sup> and remedy similar impediments. We must beg of the Pope to give a Jubilee of one month next May. It will put down all heresies—and set things right. . . . The priests are greatly fatigued with the Jubilee otherwise I wd apply at present to have it prolonged.

"Some of the people here," Cullen then noted, indicating that devotional practices were becoming contagious, "are anxious I should establish in the schools and convents the 'Child of Mary' such as they have in Waterford convent. Will you get me faculties to do this, and to establish every other sodality. I wd require to know what the indulgences are and what the rules."<sup>35</sup>

Early in the new year Cullen again wrote Kirby complaining that all was not well in the various parts of the Irish Church. "I wrote some days ago to Propaganda regarding the diocese of Ardagh," he explained in Italian in January 1853. "It seems to me," Cullen noted, "that it would be well to appoint a bishop quickly." "Father John Kilduff," he suggested, referring to a Vincentian on the Dublin mission, "a native of the diocese would be the saving of it." "He is a good preacher," Cullen continued, giving an interesting example of what he thought was important in a bishop, "a good theologian, full of zeal, and yet courageous enough. Such a man would be required to reform the diocese. He is about 33 years of age." "In the diocese of Cashel," Cullen then informed Kirby, turning to another trouble spot, "there is a parish called Doon, where I have heard

<sup>34</sup> Oct. 9, Dec. 8, 1852, K.

<sup>35</sup> Dec. 20, 1852, K.

there are seven or eight hundred apostates." "The Archbishop of Cashel, however," he reported, "does not want any noise made about it. Father Dowley, Superior of the Vincentians recently offered to give a mission but so far the offer has not been acknowledged." "The poor Archbishop," he added, "is very timid, and believes that he is always on the verge of death, even though he is in good health." "He is almost the only bishop," Cullen complained, "that has done nothing about what was prescribed in the Synod of Thurles." "Baptisms and confessions remain as they formerly were, and they also celebrate marriages in private houses. In almost all the other dioceses something at least has been done." "In this diocese of Dublin," he then explained,

all marriages and baptisms are celebrated in the churches. In the city and in the towns all the confessions are heard in the churches. In all the mountainous places where there are no churches nearby, if the distance is not too great, I told the priest to find every means of transporting the people to those distant churches—but if that were not possible to hear the confessions in private houses (except in case of illness), if the church is not more than two miles away.<sup>36</sup>

"Evviva S. Patrizio," Cullen greeted Kirby again some two months later on March 17, 1853, and continued in Italian, "I have already convened a provincial synod to be held in Dublin at Pentecost. The Bishops are not pleased." "Now it is necessary to see," Cullen explained, "quid agendum. There are things enough to be done—but it is difficult to put them in order and I must do all myself. There is no one who knows how to draw up a decree or write a line of Latin." "Monsignor Dixon," Cullen then reported, referring to the new archbishop of Armagh, "has already visited Ardagh and has promised to write in favor of Kilduff." It is Kilduff, Cullen added tenaciously in conclusion, who would be "the salvation of that unfortunate diocese."<sup>37</sup> Kilduff "will be consecrated here by me on S. Peters day." Dr. Dawson, the popular candidate among the Ardagh priests, "has written him a most foolishly impolite letter, which shows he was never fit to be a Bishop." Every attempt had been made "to get up some agitation against Kilduff by Dawsons friends—protests I believe have been sent to Rome—but the people are delighted, and the greatest part of the clergy—the appointment was absolutely necessary." "I have been told," Cullen added, referring to the archbishop of Tuam and the bishop of Clogher, "that Dr. McHale and Dr. McNally spoke against it—but this is only a report." "There is no doubt however," he assured Kirby again, "that K. [Kilduff] will be a blessing to the diocese—tho' he will have to carry his cross with the opp. of Dawson & Co." "I send you the acts and decrees of the Council in a day or two with a letter to the Pope," Cullen then noted, referring to his recently concluded provincial synod. "In Cashel," he then complained again of the archbishop, "I hear,

<sup>36</sup> Jan. 28, 1853, K.

<sup>37</sup> Mar. 17, 1853, K.

Dr. Slattery has not made a single change as yet. Marriages, baptisms, confessions still, as formerly in private houses. The same in several dioceses and I believe in Tuam." "It will be necessary," he advised darkly, "to do something in these matters." "But," he concluded characteristically, and appropriately breaking into Italian, "little by little all will be accomplished."<sup>38</sup>

This brief account of Cullen's early attempt at reforming the Irish Church in the interests of making and consolidating a devotional revolution is useful as a model, though a somewhat imperfect one, of his own continuing attempts, and those of his protégés, at reform over the next twenty-five years. In general Cullen preferred to promote men like Kilduff who were made in his own image and likeness. They were not only good preachers, adequate theologians, zealous, courageous enough, and young, but they were also generally strangers to the diocese and, therefore, they did not have any of the personal ties or loyalties that might inhibit them in their zeal for reform. If they were not recruited from the regular clergy, moreover, the new bishops were usually rectors or vice-rectors of seminaries—strict, stern, austere men who had both the experience of, and a proven talent for, efficient administration. They were also well aware that the new discipline they represented would not be popular among their priests, but if these bishops were ever to make their wills effective with their clergy, the bishops would have to depend on their patron's continued exertions on their behalf at Rome. They all tended, therefore, to be ultramontanes, because Rome was not only the theoretical but the actual source of their own and Cullen's real power in the Irish Church.

WHILE ALL OF THE FOREGOING may tell one something about what this devotional revolution consisted of and, at least partially, how it was made, the crucial question still remains—why did the Irish people respond so readily to the reform of their Church and become virtually practicing Catholics within a generation? The Great Famine was truly a gigantic psychological shock, and it certainly would be both neat and convenient to be able to assign so impressive a cause for so remarkable an effect. A guilt-ridden and frightened people turning more formally and fervently to their God in their hour of need makes more, indeed, than a good deal of superficial sense. The problem, of course, is that the devotional need appears to have been increasingly present before the famine, and only the adverse circumstances of population growth and the lack of money and personnel on the part of the Church prevented that need from being realized. The famine, therefore, was as much the occasion for as it was a cause of the devotional revolution being made and consolidated in Ireland, and one must probe more deeply if one is to understand why as well as how this remarkable historical phenomenon took place.

<sup>38</sup> [June 1853], K.

What I would like to suggest is that the devotional revolution which took place after the famine satisfied more than the negative factors of guilt and fear induced by that great catastrophe. There may indeed be something worse than the simple fear of being destroyed—the mounting terror in the growing awareness that one is being destroyed. The Irish, after all, had been gradually losing their language, their culture, and their way of life for nearly a hundred years before the famine. Education, business, politics, and communication in the written word, even more than in the spoken word, were all increasingly geared to English as the Irish were being effectively Anglicized, or, perhaps more appropriately, West Britonized. There has been so much concern, for example, in the study of Irish history in the nineteenth century with the geography of emigration that it has hardly been noticed that the Irish before the famine had nearly all become cultural emigrants, that they had in fact moved in their minds before a good many of them had actually to move in space.<sup>39</sup> In a word, then, Irishmen who were aware of being Irish were losing their identity, and this accounts in large part for their becoming practicing Catholics. The devotional revolution, I would argue further, provided the Irish with a substitute symbolic language and offered them a new cultural heritage with which they could identify and be identified and through which they could identify with one another. This is why, for example, Irish and Catholic have become almost interchangeable terms in Ireland, despite the attempts of Nationalists to make Irish rather than Catholic the inclusive term. "Take an average Irishman," the celebrated Irish Dominican preacher, Father Tom Burke, said in 1872, "—I don't care where you find him—and you will find that the very first principle in his mind is, 'I am not an Englishman, because I am a Catholic! Take an Irishman wherever he is found, all over the earth, and any casual observer will at once come to the conclusion, 'Oh; he is an Irishman, he is a Catholic!' The two go together.'"<sup>40</sup>

<sup>39</sup> Francis O'Neill to Kirby, Plymouth, July 13, 1853. K. O'Neill was a native of Waterford, but when he was ordained after his studies in the Irish College in Rome he had to go on the English mission, because there was no place available for him in the diocese of Waterford. Though the following passage is both interesting and illustrative in many ways, the important point is that the Irish speakers in Plymouth wanted to remain psychologically whole in revealing the secrets of their hearts. "Please God I hope to have a place in Waterford next year; as one can effect more where there are good hearts. Here the English Catholics never look at an Irish priest, but we have the poor Paddies who are the support of this mission and without whom the Bishop, would have to leave this place. The Irish here are about three or four hundred. Some of them cannot be got to make their confessions in English, at this the Bishop is most indignant. He says that it is pride, and also because they make more of the priest than the Sacrament. He has to keep a priest who can speak Irish. I cannot put two words of it together so in that point I am no help. Were I sure of having this good Bishop always I would not think of returning to Ireland but as this is not at all likely especially as he is an old man I shall get my Exeat. He finds many faults with the Irish but they are the ones that we cannot deny. He speaks frequently about the way the priests in Ireland demand money for the Sacraments. This is no false charge against them for all whoever had any experience of the system even in our own Diocese and also of it even since the Thurles Synod will acknowledge that too much cannot be said against it."

<sup>40</sup> Thomas H. Burke, O.P., "The Supernatural Life of the Irish People," in *Lectures on Faith and Fatherland* (London, n.d.), 117. Father Burke lectured extensively in the United States in 1872.

Finally it is necessary to observe that the making and consolidating of this devotional revolution had a wider and deeper significance than even making practicing Catholics of the Irish people in a generation. One can argue that the cause and effect relationship between what may be popularly called in the best current sociological jargon a group "identity crisis," and the resolution of it in what was fundamentally a religious revival has some very serious implications for, as well as allowing for some very interesting insights into, the history of the Irish people both at home and abroad in the nineteenth century. Daniel O'Connell, for example, if viewed as the bridge between the old and the new Ireland rather than as the divide between Old and Young Ireland, becomes not only more important but more understandable as the great transitional figure in modern Irish history.<sup>41</sup> Further, the devotional revolution and its general and particular causes are crucial to understanding the development of Irish nationalism and the cultural importance of Irish Catholicism in that development.<sup>42</sup> Moreover, the cultural revivals of Young Ireland in the middle of the century and the Gaelic movement at the end appear less ludicrous in the light of the identity crisis that had been taking place since the turn of the century, and Daniel Corkery and the other archpriests of the language movement in more recent times may indeed yet come into their own.<sup>43</sup>

<sup>41</sup> Sean O'Faolain, *King of the Beggars* (London, 1938), 367-68. In summing up the significance of O'Connell, O'Faolain wrote, "In whatever way one might try to define the ideal life of the Irish people, his image is likely to rise before the mind—always remembering that he came at the beginning and was only following his instinct in a groping use of the material to his hand. Lecky said that he studied men, not books; in studying men he found himself, and in finding himself he presented to his people a mirror of their reality. He is interesting in a hundred ways, but in no way more interesting than in this—that he was the greatest of all Irish realists who knew that if he could but once define, he would thereby create. He did define, and he did create. He thought a democracy and it rose. He defined himself, and his people became him. He imagined a future and the road appeared. He left his successors nothing to do but to follow him. They have added precision to his definition, but his definition is not altered; they have added to his methods, but his methods remain. You may break gold but it is gold still, fashion wood but it is wood still. The content of Irish life is the content of the Irish character, the dregs and the lees and the pure wine of this one man's recipe—to be purified indeed, to grow more rich in the wood with time, but never to lose the flavour of his reality, the composition of his mind."

<sup>42</sup> The author is at present engaged in writing the third volume—"The Devotional Revolution in Ireland, 1850-1878"—of his projected *History of the Roman Catholic Church in Ireland in the Nineteenth Century, 1780-1918*, in which he hopes to deal *in extenso* with this crucial relationship between nationalism and Catholicism as well as with many of the other themes only touched upon in this article.

<sup>43</sup> "The truth is, the Gaelic people of that century were not a mob, as every picture given of them, whether by historian or novelist, would lead one to think. They were mob-like in externals; and one forgives the historians if those externals threw them out, but how forgive the novelists? If not a mob what then were they? They were the residuary legatees of a civilisation that was more than a thousand years old. And this they knew; it was indeed the very pivot of all they did know, and the insult that followed on their poverty wounded them not only as human beings but as 'Children of Kings, Sons of Milesius!' ('Clanna ríghthe maca Míleadh'). With that civilisation they were still in living contact, acquainted with its history; and such of its forms as had not become quite impossible in their way of life, they still piously practised, gradually changing the old moulds into new shapes, and, whether new or old, filling them with a content that was all of the passing day and their own fields. What of art they did create in their cabins is poor and meagre if compared with what their fathers had created in the Duns of Kings and Grianans of Queens; yet the hem matches the garment and the clasp the book. Here hinted, then, what these historians scanted; and scanting the soul and the spirit of a people, what of that people have they profitably to speak? But history has belied the historians,

Last but not least in this necessarily less than complete catalog of what was significant in the devotional revolution is its importance for understanding the great Diaspora of the Irish people in the nineteenth century, as more than four million of them found new homes in a new world. Most of the two million Irish who emigrated between 1847 and 1860 were part of the pre-famine generation of nonpracticing Catholics, if indeed they were Catholics at all. They congregated in the ghettos of English, American, and Canadian cities where they acquired a fearful reputation for ignorance, drunkenness, vice, and violence. What the famine Irish actually represented, therefore, was a culture of poverty that had been in the making in Ireland since the late eighteenth century because of the pressure of population on the means of subsistence. That culture produced all the circumstances and most of the values that the British and the Americans were to find most repugnant in the Irish. The crucial point here, however, is that after the famine that culture of poverty was broken up in Ireland by emigration, and the new circumstances created by that breakup allowed for the emergence of other values.

Of the four million Irish, for example, who immigrated to the United States between 1845 and 1900, some 2,300,000 came after 1860. By 1860 there already were in Ireland 3,000 priests and 2,600 nuns for a Catholic population of 4,500,000, or one priest for every 1,500 people and one nun for every 1,700. In 1900 there were 3,700 priests and 8,000 nuns for a further reduced Catholic population of 3,300,000, or a ratio of one priest for every 900 and one nun for about every 400 people.<sup>44</sup> Besides this remarkable improvement in the clerical population vis-à-vis the Catholic population in Ireland, the Irish Church during this period exported a very large number of priests and nuns to help staff churches in the United States and the rest of the English-speaking world.<sup>45</sup> What these crude figures suggest is that the Irish were transformed as a people—men and women alike—into practicing Catholics. The succeeding waves of these recently created

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for that people, if they were but a mob, had died, and their nationality died with them: instead of which that nationality is vigorous today, not only at home, but in many lands abroad—'translated, passed from the grave.'" Daniel Corkery, *The Hidden Ireland: A Study of Gaelic Munster in the Eighteenth Century* (Dublin, 1925), 28-29.

<sup>44</sup> *Census of Ireland, 1861* (Dublin, 1863); *Census of Ireland, 1901* (Dublin, 1903).

<sup>45</sup> "Ireland has not only done a great deal during the past century for the Propagation of the Faith so that her missionaries and nuns and faithful people are now to be found foremost in everything good going on in the various missions of the old and new world but at present the whole country may be regarded as a vast recruiting field for sustaining these distant missions. We have in Kilkenny at present (to mention one instance) an American Christian Brother seeking for young men to join that order in the United States. They have at present 500 Irishmen among the Xtian Brothers of the United States and only 200 of all other nationalities. They have seven Colleges in the United States and the Superiors of them all are Irish. Nevertheless they are called the 'French Xtian Brothers.' At present they desire at least 50 more Irish postulants, as they find that none labour so zealously and efficiently in the American schools, as the Irish Brothers. The Brother has been only a few days in Ireland still he has already got twenty postulants. We have also Nuns of the Holy Cross who is seeking postulants in like manner. She told me that when she was over here two years ago she succeeded in getting twenty-five young ladies for her order in the United States, and that they all persevered. She now desires 25 more and has come over to search for them." Patrick Francis Moran to Kirby, Apr. 28, 1876, K.

devotional Catholics brought their cultural and religious needs and corresponding values with them when they emigrated, and in doing so they helped to reclaim those lapsed and nonpracticing "shanty" Irish. The newer, "lace-curtain" Irish found it progressively easier to assimilate to their new environment, because they were objectively less objectionable.

In a word, then, the Irish immigrants in this country in 1900 were a vast improvement over the generation of famine Irish who had arrived before 1860, and that improvement was not evident simply in terms of social behavior. Not only were later immigrants less drunken and less prone to violence, they also had acquired basic educational skills and were actually less poor. Average daily attendance in the Irish National System of Education increased from 100,000 in 1840 to nearly 500,000 in 1900, and this in spite of the fact that the population had been reduced by one-half over that period of time.<sup>46</sup> The literacy figures reveal that in 1861 45.8 per cent of Roman Catholics were unable to read and write, but by 1901 the figure had dropped to 16.4 per cent, a decline that was reflected in comparative literacy figures for various immigrant groups in the United States after the turn of the century.<sup>47</sup> Economic conditions in Ireland, furthermore, improved between 1840 and 1900, though the economy was certainly a very sick one, and the culture of poverty that was broken at the famine was liquidated partly by that improvement and partly by the continuing emigration, leaving the remaining population relatively less poor. No factor, then, was more important in the moral and social improvement of the Irish people either at home or abroad in the nineteenth century than the devotional revolution between 1850 and 1875; yet no aspect of recent Irish history has received less attention.

<sup>46</sup> Donald H. Akenson, *The Irish Education Experiment: The National System of Education in the Nineteenth Century* (London, 1970), 140, 346. There are no average daily attendance figures before 1852. In 1852 the number of children on the rolls was 544,604, and average daily attendance was 282,575; the number of children on the rolls in 1840 had been 232,560. I have, therefore, assuming there was improvement, calculated average daily attendance at something less than half of 232,560.

<sup>47</sup> *Ibid.*, 377. These percentages, of course, are for that portion of the population over five years of age. See also William D. P. Bliss, ed., *The New Encyclopedia of Social Reform* (New York, 1908), 598. "The total illiteracy of immigrants [to the United States] over fourteen years of age, in 1905, was 26.2 per cent. The females are, in general, more illiterate than the males." The study then cites figures for "the illiteracy of the races contributing more than 2,000 immigrants . . . for the same year." The statistics, presented under the heading "Northern and Western Europe (Chiefly Teutonic and Celtic)," are as follows: Scandinavian 0.6; Scotch 0.7; English 1.3; Bohemian and Moravian 1.7; Finnish 1.8; French 2.7; Irish 3.8; German 4.2; Dutch and Flemish 5.3; Italian (North) 14.0; Average 3.7.

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## The Vital Center, the Fair Deal, and the Quest for a Liberal Political Economy

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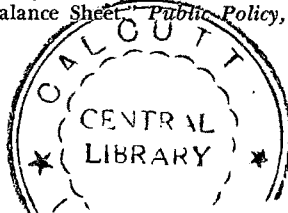
ALONZO L. HAMBY

"EVERY SEGMENT of our population and every individual has a right to expect from our Government a fair deal," declared Harry S. Truman in early 1949. In 1945 and 1946 the Truman administration had almost crumbled under the stresses of postwar reconversion; in 1947 and 1948 it had fought a frustrating, if politically rewarding, battle with the Republican Eightieth Congress. Buoyed by his remarkable victory of 1948 and given Democratic majorities in both houses of Congress, Truman hoped to achieve an impressive record of domestic reform.<sup>1</sup> The president systematized his past proposals, added some new ones, and gave his program a name that would both connect his administration with the legacy of the New Deal and give it a distinct identity. The Fair Deal, while based solidly upon the New Deal tradition, differed from its predecessor in significant aspects of mood and detail. It reflected not only Truman's own aspirations but also a style of liberalism that had begun to move beyond the New Deal during World War II and had come to maturity during the early years of the cold war—"the vital center."

THROUGHOUT THE HISTORY of the United States the main stream of reform has been within the broad Lockean-capitalist consensus to which most Americans subscribe. The Great Depression, however, had caused liberal reformers to question capitalism as never before; mass unemployment at home and the rise of an aggressive fascism out of the ruins of capitalism abroad seemed to provide proof that the old system had failed beyond repair. One logical response with appeal to many reform thinkers and leaders

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<sup>1</sup> *Public Papers of the Presidents: Harry S. Truman, 1949* (hereafter *Public Papers, 1949*) (Washington, 1964), 7. The best guide to the political history of the Truman era is Richard E. Neustadt, "Congress and the Fair Deal: A Legislative Balance Sheet," *Public Policy*, 5 (1954): 349-81.





was the movement for a popular front of all reform and radical forces, most strongly united by a determination to stop the spread of fascism but also seeking newer and better socioeconomic arrangements, even "revolutionary" ones. The New Deal itself, faced with the actual responsibility of governing, took a far more moderate course, searching for a viable middle way that would preserve capitalism; yet even the New Dealers, unable to overcome the depression, were increasingly driven to the conclusion that capitalism had become incapable of the growth needed to provide reasonably full employment.

The thirties did not exactly constitute the fabled "Red Decade" of right-wing mythologists. Most liberals who worked within the government sought American solutions to American problems and appear to have been only marginally influenced by foreign examples. Those outside the government were more likely to look toward European patterns. The most enduring appeal they found was in Scandinavian welfarism, but many were at least provisionally drawn to Soviet communism. A liberal of the thirties, quite in line with the popular-front mood, was more likely to think of himself as part of an undifferentiated Left and more prone to consider substitutes for capitalism than were earlier progressives. The failure of capitalism at home and abroad did not throw the liberals en masse into the Communist party, but it shook old assumptions to an extent that left few unaffected.<sup>2</sup>

Temporarily shattered by the Nazi-Soviet Pact of 1939, popular-front foreign policy staged a resurgence during World War II and received an aura of legitimacy from President Roosevelt's effort to forge a lasting alliance with the Soviet Union. During the war and the years immediately following, advocates of Soviet-American friendship could use the Roosevelt name and symbolism as a potent appeal. Yet at the same time World War II eroded the domestic side of popular frontism. The war eliminated the depression—as the New Deal had not—and demonstrated the potential of American industry. To a large extent, moreover, businessmen managed the economic war effort, and, while the liberals frequently criticized them on matters of detail, it was hard to refute the statistics of success. One result was a widespread repudiation of the psychology of scarcity, which had grown out of the long years of the depression. Leading progressives popularized the vision of an ever-expanding capitalist economy balanced by Keynesian fiscal methods and buffered by extensive social welfare programs. Their intellectual leader was the eminent economist Alvin H. Hansen and their political leader was Vice-President Henry A. Wallace, who demonstrated that it had become possible, even natural, to be a popular fronter

<sup>2</sup> Frank A. Warren, III, *Liberals and Communism: The "Red Decade" Revisited* (Bloomington, 1966), is a sober and intelligent analysis of popular frontism in the 1930s. William E. Leuchtenburg mentions the appeal of Scandinavia in *Franklin D. Roosevelt and the New Deal* (New York, 1963), 345.

in foreign policy and an advocate of "progressive capitalism" at home. The liberal mission was no longer to achieve a new socioeconomic system or even to prop up a "mature," worn-out economy; it was to realize capitalism's capacity for endless growth.<sup>3</sup>

The cold war completed the demise of the popular-front mood. Groups and individuals that thought of themselves as liberal came increasingly to perceive the Soviet Union as an expansionist, totalitarian force and the American Communist party as the slavish, antiliberal representative of Soviet despotism. In 1947 an influential group of liberals established Americans for Democratic Action (ADA) with the express purpose of isolating Communists and pro-Communists from the main stream of liberal politics. Key foreign policy events that followed—the Russian rejection of the Marshall Plan, the Czech coup, and the Berlin Blockade—inclined most progressives toward the ADA position. In 1948 the *New Republic*, probably the most sensitive barometer of progressive opinion, rejected the popular-front style of Henry Wallace's presidential candidacy and endorsed Truman. Wallace's weak showing on election day demonstrated a massive liberal repudiation of the Soviet Union and the Communist party.<sup>4</sup>

By 1949 the ADA was the dominant progressive organization, and signs of a transformation were appearing throughout the liberal community. The editor of the *New York Post*, T. O. Thackrey, had endorsed Wallace for president. In April 1949 the paper's owner and publisher—his wife, Dorothy Schiff Thackrey—fired him. It is significant that Mrs. Thackrey defined the break in terms of attitudes toward communism, arguing that the Communists posed "new threats to democracy" and asserting that henceforth the paper would fight with equal vigor "all totalitarianism, whether Fascist or Communist." Her new editor, James A. Wechsler, was an ADA leader and a militant anti-Communist, who quickly dismissed popular-front columnists.<sup>5</sup>

The *New Republic* continued the shift it had begun in 1948. Its publisher, Michael Straight, who had once regarded Henry Wallace as a personal hero, undertook a speaking tour on behalf of the ADA and delighted its leaders by bringing his magazine into nearly total agreement with the organization's viewpoint. The Italian-American intellectual, Max Ascoli, established the *Reporter* as a new outlet for moderate, tough-minded, anti-Communist liberalism. The Congress of Industrial Organizations expelled

<sup>3</sup> I have discussed these themes in "Sixty Million Jobs and the People's Revolution: The Liberals, the New Deal, and World War II," *Historian*, 30 (1968): 578-98; and in "The Liberals, Truman, and FDR as Symbol and Myth," *Journal of American History*, 56 (1970): 859-67. Ironically the Communist party itself contributed to the decline of domestic popular frontism by pursuing a wartime national unity line. See Irving Howe and Lewis Coser, *The American Communist Party: A Critical History* (2d ed.; New York, 1962), ch. 9.

<sup>4</sup> Alonzo L. Hamby, "Henry A. Wallace, the Liberals, and Soviet-American Relations," *Review of Politics*, 30 (1968): 153-69; "1948: The New Beginning," *New Republic*, Sept. 27, 1948, p. 32.

<sup>5</sup> *New York Post*, Apr. 6, 1949; James A. Wechsler, *The Age of Suspicion* (New York, 1953), 238-41.

popular-front unions. "We in American labor will fight totalitarianism from the right or the left," declared Philip Murray, the president of the CIO, "We regard the human welfare state as America's middle way."<sup>6</sup>

Murray's remark epitomized a new mood that conceived of liberalism as a center doctrine midway between the totalitarian poles of fascism and communism. Implicit in the new self-image was a slight moderation, a decline of utopian hopes and aspirations, a somewhat stronger suspicion of big government, and increasing doubts about the goodness of human nature. It was no coincidence that four significant books expressing this viewpoint in one manner or another appeared in 1949.

In *Target: You*, Leland Stowe, a widely read foreign correspondent, addressed himself to "Mr. American Middle Man"—the target of fascist and Communist totalitarianism, of monopolistic "Big Capitalism" and Communist Marxism—and argued that the future of American democracy depended upon the maintenance of a "strong political Center" that would counter both domestic communism and right-wing extremism by providing economic security and securing civil liberty under a rule of law.<sup>7</sup>

Max Ascoli in *The Power of Freedom* depicted the earth as caught up in a worldwide civil war with one side struggling to maintain freedom by finding the middle way between unrestrained capitalism and total socialization, the other attempting to achieve "the total subjection of men on a world-wide scale." Stressing the limitations of human nature and the unattainability of utopias, Ascoli unabashedly admitted that he was a disciple of such thinkers as Edmund Burke, Alexander Hamilton, and Alexis de Tocqueville. Yet he found no inconsistency in declaring: "I am a liberal, and I don't want to add any qualifying adjectives."<sup>8</sup>

In *Strategy for Liberals* the political journalist Irwin Ross used militant rhetoric and projected an ambitious reform program. Yet he carefully typed his ideal polity as the "Mixed Economy" and distinguished it not simply from fascism and communism but also from socialism, which, with its complete control of industry, detailed planning, massive bureaucracy, and control of communications, contained within itself "if not the seeds of decay, certainly the seeds of totalitarianism."<sup>9</sup>

Arthur M. Schlesinger, Jr. gave the new liberalism a name with the publication of *The Vital Center*. An exercise in political philosophy and an exhortation to American progressives, the volume won an impressive reception. "It seemed to me one of those books which may suddenly and

<sup>6</sup> James Loeb, Jr. to Edith Fountain, Feb. 2, 1949, Americans for Democratic Action Papers (hereafter: ADA Papers), State Historical Society of Wisconsin, Madison, Wis.; M. A. [Max Ascoli], "What We Stand For," *Reporter*, Dec. 20, 1949, p. 2; "Our Liberalism," *ibid.*, Mar. 28, 1950, p. 1; Max M. Kampelman, *The Communist Party vs. the C.I.O.* (New York, 1957), 256, and chs. 11-13; David J. Saposs, *Communism in American Unions* (New York, 1959), chs. 17-20.

<sup>7</sup> Leland Stowe, *Target: You* (New York, 1949), 164, *passim*.

<sup>8</sup> Max Ascoli, *The Power of Freedom* (New York, 1949), xiii, 8, 70-71, *passim*.

<sup>9</sup> Irwin Ross, *Strategy for Liberals* (New York, 1949), 45, *passim*.

clearly announce the spirit of an age to itself," wrote Jonathan Daniels. Deeply influenced personally and intellectually by Reinhold Niebuhr, Schlesinger castigated the popular-front liberals as sentimental believers in progress and human perfectionism who, yearning for utopias, had been seduced by the surface idealism of communism and the Soviet experiment. Awake only to the evils of fascism, they had sympathized with at least some aspects of the Soviet experience and had accepted the Communists as allies in a common struggle, not understanding that such a tactic could lead only to self-destruction. The "restoration of radical nerve" had come with the rise of a non-Communist Left in Europe and the United States, largely through the efforts of younger liberals whose impressions of the Soviet Union stemmed from the Stalinist purges of the 1930s rather than the idealism of the Russian Revolution. The new liberalism—or "radicalism" as Schlesinger preferred to call it—unconditionally rejected all varieties of totalitarianism. Applied to foreign affairs it stood for a dual policy of vigilantly containing communism and encouraging the democratic Left abroad. Believing "in the integrity of the individual, in the limited state, in due process of law, in empiricism and gradualism," it was acutely aware of the weaknesses of human nature and of the dangers of excessive concentration of power. Devoted to the furtherance of individual liberty, it stood for a mixed economy, featuring partial government planning and ownership, antitrust action to discipline private big business, and welfare programs to provide a minimum of security and subsistence to all. The conception of liberalism as a sort of centrism had its liabilities. Schlesinger found it natural to identify with "responsible conservatives" such as Charles Evans Hughes and Henry L. Stimson; liberals, he suggested, might find common cause with this group, especially on matters of civil rights and civil liberties. Doubtless he was correct, and it was tempting, after militantly rejecting the revolutionary totalitarian ideology of communism, to conceive of the liberal effort to preserve humane, democratic values as akin to an intelligent conservatism; yet even the creed of a Hughes or a Stimson provided few answers for the problems that preoccupied the liberals. Unfortunately it was but a short step from the vital center to the superficialities of the "New Conservatism" of the 1950s.<sup>10</sup>

Whatever its inner weaknesses, the vital-center approach gave the liberal movement a moral integrity and consistency that had been absent during the popular-front era. Its implications, moreover, went beyond the affairs of diplomacy or the tactical wisdom of a liberal-Communist alliance: its approach to political economy rejected what remained of domestic popular frontism and idealized the New Deal as an effort to establish a mixed economy that would preserve the essentials of capitalism while mitigating its abuses. Even the business community was recognized as a potentially

<sup>10</sup> Arthur M. Schlesinger, Jr., *The Vital Center* (Boston, 1949), 208–09, 223–24, *passim*; Jonathan Daniels, "Ready to Be Radical," *Saturday Review of Literature*, Sept. 10, 1949, pp. 11–12.

constructive, if frequently wrongheaded, force in American life. The vital-center liberals looked to Niebuhr for a sociopolitical theory and to Keynes for an approach to economics, convinced that this combination provided the best possible foundation for human freedom. In 1948 a group of Keynesians published the major liberal economic manifesto of the Truman era, *Saving American Capitalism*.<sup>11</sup> The title accurately represented the way in which vital-center liberalism was a return to the traditional American progressive impulse.

The legislative goals Truman announced for his administration, while not devised to meet the needs of an abstract theory, were well in tune with the vital-center approach: anti-inflation measures, a more progressive tax structure, repeal of the Taft-Hartley Act, a higher minimum wage, a farm program based on the concepts of abundant production and parity income, resource development and public power programs, expansion of social security, national medical insurance, federal aid to education, extensive housing legislation, and civil rights bills. The president's most controversial request was for authority to increase plant facilities in such basic industries as steel, preferably through federal financing of private enterprise but through outright government construction if necessary. Roundly condemned by right-wing opponents as "socialistic" and soon dropped by the administration, the proposal was actually intended to meet the demands of a prosperous, growing capitalist economy and emerged from the Fair Deal's search for the proper degree of government intervention to preserve the established American economic structure. "Between the reactionaries of the extreme left with their talk about revolution and class warfare, and the reactionaries of the extreme right with their hysterical cries of bankruptcy and despair, lies the way of progress," Truman declared in November 1949.<sup>12</sup>

The Fair Deal was a conscious effort to continue the purpose of the New Deal but not necessarily its methods. Not forced to meet the emergencies of economic depression, given a solid point of departure by their predecessors, and led by a president more prone than FDR to demand programmatic coherence, the Fair Dealers made a systematic effort to discover techniques that would be at once more equitable and more practical in alleviating the problems of unequal wealth and opportunity. Thinking in terms of abundance rather than scarcity, they attempted to adapt the New Deal tradition to postwar prosperity. Seeking to go beyond the New Deal while preserving its objectives, the Truman administration advocated a more sweeping and better-ordered reform agenda. Yet in the quest for political means, Truman and the vital-center liberals could only fall back upon one of the oldest dreams of American reform—the Jacksonian-Populist vision of a union of producing classes, an invincible farmer-

<sup>11</sup> Seymour Harris, ed., *Saving American Capitalism* (New York, 1948).

<sup>12</sup> *Public Papers*, 1949, 1-7, 552.

labor coalition. While superficially plausible, the Fair Deal's political strategy proved too weak to handle the burden thrust upon it.

The Fair Deal seemed to oscillate between militancy and moderation. New Dealers had frequently gloried in accusations of "liberalism" or "radicalism"; Fair Dealers tended to shrink from such labels. The New Dealers had often lusted for political combat; the Fair Dealers were generally more low keyed. Election campaigns demanded an aggressiveness that would arouse the Democratic presidential party, but the continued strength of the conservative coalition in Congress dictated accommodation in the post-election efforts to secure passage of legislative proposals. Such tactics reflected Truman's personal political experience and instincts, but they also developed naturally out of the climate of postwar America. The crisis of economic depression had produced one style of political rhetoric; the problems of prosperity and inflation brought forth another.

The Fair Deal mirrored Truman's policy preferences and approach to politics; it was no more the president's personal creation, however, than the New Deal had been Roosevelt's. Just as FDR's advisers had formulated much of the New Deal, a group of liberals developed much of the content and tactics of the Fair Deal. For the most part these were the men who had formed a liberal caucus within the administration in early 1947 shortly after the Republican triumph in the congressional elections of 1946, had worked to sway the president toward the left in his policy recommendations and campaign tactics, and had played a significant, if not an all-embracing, role in Truman's victory in 1948. Truman's special counsel, Clark M. Clifford, was perhaps the most prominent member of the group, but Clifford, although a shrewd political analyst, a persuasive advocate, and an extremely valuable administrative chief of staff, was neither the caucus's organizer nor a creative liberal thinker. Others gave the Fair Deal its substance as a program descending from the New Deal yet distinct from it.<sup>13</sup>

The founder of the liberal caucus, Oscar R. Ewing, exemplified better than any other prominent member of the Truman administration the linkage between the New Deal and the Fair Deal. Even as a young man in turn-of-the-century Indiana he had possessed a consuming interest in Democratic politics and social welfare problems. At the age of sixteen he was secretary to the state Democratic committee, and for a time he planned to become a social worker. Instead, after graduating from the Harvard Law School, he settled in New York and pursued a highly successful practice as a partner of first the elder, then the younger, Charles Evans Hughes. By the 1940s he had also become one of the most prominent Democrats in the state and was frequently mentioned as a possible candidate for high

<sup>13</sup> Cabell Phillips, *The Truman Presidency: The History of a Triumphant Succession* (New York, 1966), 162-65; and Irwin Ross, *The Loneliest Campaign: The Truman Victory of 1948* (Signet ed.; New York, 1969), 27-29, discuss the liberal caucus; both probably overemphasize the importance of Clifford.

office. During Robert E. Hannegan's tenure as chairman of the Democratic National Committee (1944-47), Ewing was vice-chairman and, after Hannegan's health collapsed, acting chairman. Appointed administrator of the Federal Security Agency<sup>14</sup> in 1947, he began a drive to revitalize the agency and secure cabinet status for it. It was he who took the initiative in mobilizing the liberals within the Truman administration for the crucial struggles of 1947 and 1948.

Ewing's advocacy of comprehensive social welfare legislation—a popular magazine described him as “Mr. Welfare State himself”—was the end result of a tradition that had begun with the social workers of the Progressive era, had found partial realization during the New Deal, and was now struggling for complete fulfillment. Ewing also represented a type of Democrat who had developed during the New Deal—the staunch, partisan regular who was nevertheless committed to social welfare liberalism and identified his party with it. The strongest fighter within the administration for expanded welfare programs, he did not shrink from debate with the opposition. “It is the fate of the American liberal to be a scrapper,” he remarked. Accepting the Sidney Hillman Award from the Amalgamated Clothing Workers Union in March 1950, he defined the key to America's future as “the protection and extension of equal opportunity for all our people—opportunity to live, to advance, to think, to achieve.” Especially in 1949 and 1950 he engaged in lusty verbal combat with his conservative opponents—“the League of Frightened Men,” he called them. Ewing demonstrated the way in which the New Deal, and indeed the whole progressive social welfare tradition, provided a solid basis for the Fair Deal, but his ideas, although they went beyond New Deal welfare programs, did not give the administration its claim to a separate identity. His style, as it turned out, was not especially productive; someone doubtless had to speak out against the bitter-end opponents of social welfare reforms, but Ewing only exposed himself to defeat by doing so. His militant advocacy of national health insurance not only failed to put the proposal over; it led to a backlash and caused Congress to reject an administration reorganization plan that would have created a cabinet-level Department of Welfare with Ewing as its first secretary. His personal defeat on this issue exemplified many of the difficulties the Fair Deal encountered when it adopted the militant tones of years past.<sup>15</sup>

WHILE EWING REPRESENTED CONTINUITY, Leon H. Keyserling and Charles F. Brannan gave the Fair Deal much of its distinctive approach. Both men

<sup>14</sup> Established during the New Deal, the Federal Security Agency became one of the major components of the present-day Department of Health, Education, and Welfare.

<sup>15</sup> New York Times, June 2, Sept. 14, 1945; Feb. 3, Mar. 17, Apr. 13, 1946; Dec. 3, 1947; Jan. 28, 1948; Jan. 10, Feb. 19, May 20, Aug. 17, 1949; Ewing, speeches in the *Congressional Record*, 81st Cong., 2d sess., 1950, app., pp. A1844-45, A4071-73; *Current Biography*, 1948, 193-96; Monte M. Poen, “The Truman Administration and National Health Insurance” (Ph.D. dissertation, University of Missouri, 1967), 140-42, 145-50, 155-56, 176, 188-89; Arthur M. Schlesinger, Jr., *The Coming of the New Deal* (Boston, 1958), 554.

served their political apprenticeships during the New Deal, but both formulated important criticisms of it and sought new techniques to achieve the objectives of liberal reform.

Keyserling, educated at Columbia University—where he was influenced by Rexford G. Tugwell—and the Harvard Law School, had gone to Washington in the early days of the Roosevelt administration to work for Jerome Frank in the Agricultural Adjustment Administration. He attracted the attention of Senator Robert F. Wagner, who made him an administrative assistant; during the next several years he was a central figure in the drafting of some of the most important legislation of the 1930s, including the National Labor Relations Act. Subsequently he was general counsel of the U.S. Housing Authority, later the National Housing Agency. In 1944 he took second prize in a widely publicized contest on the achievement of postwar prosperity with an essay urging an expansion of the economy to provide jobs for all. In 1945 he was active in the struggle for full-employment legislation. With Senator Wagner's backing he was a natural choice for the new Council of Economic Advisers, established by the Employment Act of 1946.<sup>16</sup>

During 1947 and 1948 Keyserling was a valuable member of the administration liberal caucus. At the same time he was gaining a public reputation as the most imaginative and articulate economist in the government. When the bland and moderate Edwin Nourse resigned as chairman of the Council of Economic Advisers in October 1949, Keyserling was automatically the liberals' candidate for the post, and the ADA spearheaded an intensive lobbying campaign in his behalf. After a long delay, in the spring of 1950 the president gave Keyserling the appointment.<sup>17</sup>

Although he had won formal appointment as the chief economic spokesman of the administration and had long been valued by the most able members of the president's staff, Keyserling appeared rather insecure. Academic economists were cool toward him because he lacked the appropriate pedigree of university degrees, though in mobilizing support for his promotion, the ADA found most liberal economists willing to support him, not as the best man for the job, but simply as the best possible alternative. Within the administration he had to live down his reputation as an Ivy League liberal ideologue; he seized opportunities to remind listeners that he had been born in South Carolina and could produce a letter of commendation from Robert A. Taft. Perhaps such difficulties were responsible for his enormous vanity and stuffy manner. Yet his brilliant mind transformed Truman's style and aspirations into an economic program.<sup>18</sup>

<sup>16</sup> *Current Biography*, 1948, 352-55; author's interview with Keyserling, Sept. 20, 1967; J. Joseph Huthmacher, *Senator Robert F. Wagner and the Rise of Urban Liberalism* (New York, 1968), 147, 163-64, 190, 192, 211, 224-25, 296-97.

<sup>17</sup> Charles La Follette to Harry S. Truman, Oct. 20, 1949, and La Follette to William Green, Oct. 27, 1949, both in ADA Papers; "Toward Full Employment," *New Republic*, May 22, 1950, p. 8.

<sup>18</sup> Robert S. Allen and William V. Shannon, *The Truman Merry-Go-Round* (New York, 1950), 79-81.



In line with the mood of the Fair Deal, Keyserling assiduously avoided labels more specific than "forward-looking" or "the middle way" for his ideas. He noted on the first page of his essay in *Saving American Capitalism* that he rejected "classification within any 'school of thought' or endorsement of any 'general theme' or 'purpose' which this collection may be deemed by some to represent." Adamantly refusing to be typed as a Keynesian, he frequently criticized New Deal economics: "Neither those 'liberals' who betray nostalgia for the New Deal of the thirties which accomplished much but not nearly enough, nor those 'conservatives' who would incarnate the brutal and reckless economic philosophy of the twenties should be allowed to say the last word."<sup>19</sup>

On the surface his ideas and advice seemed an odd mixture of liberalism and conservatism. Writing to Clark Clifford in December 1948 with suggestions for the State of the Union message, he sounded like a conservative: "I am particularly concerned about the discussion of the economic program, which seems to imply that the Government is going to do the whole job. . . . The first responsibility for employment and production rests with business." Yet a few days later he was advocating more ambitious public-housing schedules than those proposed by the National Housing Authority. He sounded like a conservative when he emphatically disclaimed responsibility for the controversial proposal to expand basic industrial plants. He sounded like a New Dealer when he urged delegates to the convention of the Meat Cutters Union to push for higher wages and when he declared that "accruals of fat earnings" justified such demands. Shortly thereafter, however, he was reassuring business that "nobody in Washington has ever taken the position that the American economy could expand without profits."<sup>20</sup>

Keyserling's critique of New Deal economics had several themes. First of all, the New Deal had failed to grasp the virtual impossibility of the task it had undertaken: the lifting of the nation out of the depression. Those who argued that the New Deal would have been successful with a more massive spending effort were probably wrong. Government alone simply could not solve great economic crises, and if the New Deal could not be blamed for its failure, the New Dealers could be blamed for not learning the lessons of that failure.

The New Dealers also had become too dogmatic in their adoption of the antitrust persuasion. "Today some industries which are organized on a large, integrated basis are charging prices under the limit of what the traffic will bear," Keyserling wrote in 1948. By contrast, home building, the

<sup>19</sup> Keyserling, "Deficiencies of Past Programs and Nature of New Needs," in Harris, *Saving American Capitalism*, 81-94.

<sup>20</sup> Leon H. Keyserling to Clifford, Dec. 20, 1948, Jan. 3, 1949, both in Clark M. Clifford Papers, Harry S. Truman Library, Independence, Mo.; *New York Times*, Jan. 19, Feb. 28, Apr. 21, 1949.

most fragmented industry in the country, "has been notoriously inefficient, highly resistant to technological change, and periodically prices its product out of the market." The antitrust laws should be used to prosecute monopolistic wrongdoing, but "we cannot re-create the pre-Civil War pattern." The liberals needed instead to ask if there were not instances in which monopolistic concentration might be "used to stabilize rather than to exploit the economy." Conversely, they needed to undertake a more searching analysis of the problems that competition presented to economic stability. Some degree of economic coordination, as voluntary as possible, would always be desirable and, during times of economic difficulty, essential.

The adoption of Keynesianism by the New Dealers had not provided American liberalism with an economic panacea, Keyserling argued. Keynesianism might be useful during a depression, but it raised more problems than it solved by its remedies for inflation. Higher taxes and interest rates bore most heavily upon the lower and lower-middle classes. Cutbacks in government spending meant the sacrifice of "national objectives which we should not forgo merely because we are prosperous."

Finally, he charged, the New Dealers had lost faith in the potential of capitalism. Considering the system pathologically unstable, even if for some reason worth saving, they awaited the inevitable onset of a major depression armed with vast government programs, which probably would be no more successful than the New Deal itself. They had failed to address themselves to the potential of the American economy; they had not formulated theories for the maintenance of prosperity. "The people of America need to be electrified by our limitless possibilities, not frightened into action by prophets of disaster."

American capitalism, as Keyserling envisioned it, had virtually unlimited opportunities for growth; an ever-expanding economy could produce undreamed-of abundance and material gain for all classes. The liberals should concentrate not on reslicing the economic pie but rather on enlarging it. Business could expect higher profits, labor better wages, farmers larger incomes, and, above all, those at the bottom of the economic scale could experience a truly decent life. The federal government should publicize these possibilities; it should provide education and guidance to the private forces whose responsible cooperation would be imperative. Keyserling recommended the initiation of a "National Prosperity Budget" in which the government would lay down targets for employment and production, indicate priority needs, and sketch out price and wage recommendations. It would be purely advisory, depending upon the cooperation of the private sectors for implementation.

The government would not be passive. It would continue to police the economy against monopolistic abuses, dictate minimum wages, use Keynesian fiscal and monetary techniques, and even impose selective controls if conditions demanded. It would provide important programs and services

—such as low-cost housing, social insurance, education, and resource development—that fell outside the realm of private enterprise. Washington, however, could not keep the economy growing by itself. Expansion demanded voluntary cooperation: “The widening of this area of voluntary cooperation, through common study of common problems, is the only way that our highly industrialized and integrated economy can steer between the danger of periodic collapse and the danger of excessive governmental centralization of power.”

To those who feared that expansion meant boom-and-bust inflation, Keyserling replied that the growth years 1927–29 had constituted an era of remarkable price stability. Economic policy should concentrate less on prices as such and more on the relationship between wages, prices, and profits; it should work for the optimum balance between consumer purchasing power and corporate income in order to maintain full employment and expansion. The New Dealers, he believed, had turned too frequently toward controls to fight inflation after World War II. Selective controls might be necessary at times, but the way to deal with inflation was to enlarge productive capacity to meet demand. Keyserling did not shrink from stimulative government spending in times of prosperity, and although he would not admit it, he was willing to trade a mild inflation for growth. Such an alternative was greatly preferable to the achievement of price stability via a “downward ‘correction’” or recession. Higher unemployment and lower production might keep prices stable but hardly contributed to the overall health of the economy. “The idea that we can protect production and employment by reducing them ‘a little bit,’” he warned, “is about as safe as the ancient remedy of blood-letting.”<sup>21</sup>

During the first half of 1949 Keyserling transformed his vision of abundance to solid figures. Assuming an annual growth rate of three per cent and constant dollar values, the gross national product could rise from \$262 billion in 1948 to \$350 billion in 1958, national income from \$226 billion to \$300 billion. In 1948 almost two-thirds of all American families had lived on incomes of less than \$4,000 a year; by 1958, \$4,000 could be the minimum for all families. It would require only about half of the GNP increase to attain this goal, leaving a substantial sum for government programs and the enhancement of private incomes at other levels. Poverty thus could be eliminated without a redistribution of wealth. Progressive reform did not necessarily mean social conflict; rather it required intelligent cooperation.<sup>22</sup>

<sup>21</sup> Keyserling, “Deficiencies of Past Programs,” 81–94; Keyserling, “For a National Prosperity Budget,” *New York Times Magazine*, Jan. 9, 1949, pp. 42–43, 45; Keyserling, “The Middle Way for America,” *Progressive*, May 1949, pp. 5–9; Keyserling, “A Policy for Full Employment,” *New Republic*, Oct. 24, 1949, pp. 13–15.

<sup>22</sup> Keyserling, address in San Francisco, Sept. 18, 1949, Clifford Papers; [Keyserling] “Memorandum Relating to \$4000 Minimum Standard of Living,” Sept. 30, 1949, George Elsey Papers, Truman Library; Keyserling, “Planning for a \$300 Billion Economy,” *New York Times Magazine*, June 18, 1950, pp. 9, 24–27.

Truman adopted Keyserling's figures and rhetoric. Speaking to a Kansas City audience in the fall of 1949, he acclaimed the nation's history of economic growth and increasingly higher standards of living and declared his determination to continue the process. He talked of the \$300 billion national income and the \$4,000 family minimum. "That is not a pipe dream," he asserted. "It can be done."<sup>23</sup>

Keyserling had not discovered the idea of economic growth, although his ego seemed at times to tempt him to imply that he had. The growth levels of World War II had awakened many economic thinkers—Alvin Hansen and Henry Wallace, for example. Nor was Keyserling fair in his assertions that the New Deal Keynesians really accepted the business cycle and that their remedies could not be put into effect until a depression had already hit the economy. The Keynesians sought at the least to smooth out the business cycle so that depressions would be eliminated altogether, at best to maintain a constant growth without even periodic recessions.

If some of Keyserling's polemics rested on artificial assertions, his broad conception of economic expansion was nevertheless inspiring and enormously constructive. The major difficulty in the program he advocated was his reliance on voluntarism, his faith in education, his belief that group conflict could be mitigated by alluring vistas that promised gains for all. In accord with the general approach of the Fair Deal, Keyserling sought to base his economics on a politics of consensus, which neither he nor other Fair Dealers were ever able to achieve. His dream of an ever-prosperous society based on voluntary cooperation was almost as utopian as the scheme of a nineteenth-century anarchist.<sup>24</sup> Fortunately there were surer, if less perfect, ways of promoting economic growth. Keyserling defined important goals for the administration and captured the ear of a president who respected his intellectual ability and liked him as an individual.

CHARLES F. BRANNAN, who was as much a product of the New Deal as Keyserling, had begun his career in Colorado politics as a disciple of the old progressive, Edward Costigan, and an associate of Oscar L. Chapman, a dedicated liberal whom Truman appointed secretary of the interior in 1949. During the Roosevelt era Brannan had worked as an attorney for the Resettlement Administration and had been a regional director of the Farm Security Administration. Long close to the neo-populist National Farmers Union, he was a personal friend of its president, James G. Patton. Moving to Washington as assistant secretary of agriculture in 1944, Brannan quickly established himself as a loyal and capable lieutenant. In 1948 he took command of the department with the blessings of the outgoing

<sup>23</sup> *Public Papers, 1949*, 494.

<sup>24</sup> Richard Lester, "Truman Economics—1950 Model," *New Republic*, Jan. 28, 1950, pp. 11-13.

secretary, the moderate Clinton Anderson, and the enthusiastic endorsement of the Farmers Union, which had bitterly fought Anderson. No man, not even the elder or the younger Henry Wallace, had entered the office of secretary of agriculture with clearer credentials as an aggressive liberal. Like Keyserling, Brannan used the concept of abundance as an intellectual foundation. The politicoeconomic strategy that he formulated constituted the Fair Deal's clearest break with the New Deal.

In the fall of 1948 Brannan's astute advice on political strategy and his vigorous campaigning won the attention of Truman and brought him into the White House inner circle. Almost alone Brannan grasped that Midwestern farmers were apprehensive about the future of price supports and that the Republican Eightieth Congress, by failing to enlarge government storage facilities, had practically guaranteed that grain prices would decline during the presidential campaign. Truman and his liberal advisers quickly adopted Brannan's counsel of attacking the GOP as the party of opposition to price supports, and the secretary himself carried the message into farm areas with a tirelessness that shamed other cabinet members. Truman's unexpected success in the rural Midwest made Brannan one of the major figures of the administration. It also suggested new political strategies to liberals both inside and outside the government.<sup>25</sup>

Many progressives believed that the farm results represented a new trend in liberal politics. To the influential columnist Samuel Grafton, 1948 had been "a year of deep and quiet decision" for farmers; the election indicated that they had overcome their conservative biases in favor of their practical need for government support and would turn increasingly to the Democratic party.<sup>26</sup> If such were the case, then the task of the liberals was to encourage and consolidate this trend. The ultimate result would be a new Democratic party with a more solidly liberal base than ever before, a liberalism that would fuse the outlook and voting power of labor with an apparently reborn Midwestern agrarian insurgency. The liberal cause would be greatly strengthened and the conservative forces proportionately weakened. Within the Republican party the number of Midwestern reactionaries would decline; within the Democratic party the Southern conservatives would have less leverage.

The first imperative was to establish lines of communication between the farmers and the liberal-labor forces. The ADA began the process by calling a conference of about thirty farm and labor leaders, which met in Chicago at the end of February 1949. The farm leaders included James Patton from the Farmers Union, Murray Lincoln and some other progressive dissenters from the Farm Bureau, Jerry Voorhis and others from the cooperative movement, and several local Grange officials. Among the labor delegates were representatives of the Railway Trainmen, the Textile

<sup>25</sup> Allen J. Matusow, *Farm Policies and Politics in the Truman Years* (Cambridge, Mass., 1967), 170-85; Allen and Shannon, *Truman Merry-Go-Round*, 114-16.

<sup>26</sup> Samuel Grafton in the *Chicago Sun-Times*, Jan. 26, 1949.

Workers, the International Ladies Garment Workers Union, and the United Auto Workers. The meetings amounted simply to an exchange of views; the conference made no effort to hammer out a legislative program or draft a call to action. Yet James Loeb, the ADA's executive secretary, found the sessions "an exciting experience." The discussions were friendly despite some disagreements, and many of the participants favored more conferences at the state and local levels. "The farm and labor groups are moving, slowly but definitely, in the direction of mutual understanding," declared Loeb. "The encouragement of this process can have a lasting effect on the future history of America."<sup>27</sup>

The administration took the next step in April with the introduction in Congress of a new farm program, which had been drawn up under Brannan's direction. The Brannan Plan was difficult and complex in detail, but essentially it was an effort to maintain farm income at the record high level of the war and immediate postwar periods while letting market prices fall to a natural supply-demand level. Brannan thus proposed to continue the New Deal policy of subsidizing the farmers, but he broke dramatically with the New Deal technique of restricting production and marketing in order to achieve artificially high prices.

Many agrarian progressives, including Henry A. Wallace himself, had long been troubled by the price-support mechanisms and had sought methods of unleashing the productive capacity of the farms. Brannan seemed to show the way. He proposed the maintenance of farm income through direct payments to farmers rather than through crop restriction. In order to encourage and protect the family farm, moreover, he recommended supporting a maximum of about \$26,100 worth of production per farm. To the consumer he promised milk at fifteen cents a quart, to the dairy farmer a sustained high income. To the Democratic party he offered an apparently ingenious device that would unite the interests of farmers and workers.<sup>28</sup>

Liberals generally were enthusiastic over both the principles and the politics of the Brannan proposals. "The new plan lets growers grow and eaters eat, and that is good," commented Samuel Grafton. "If Brannan is right, the political miracle of 1948 will become a habit as farmers, labor and consumers find common political goals," wrote agricultural columnist Angus MacDonald. James Patton called the Brannan Plan "a milestone in the history of American agriculture," and the *Nation* asserted that the average consumer should devote all his spare time to support of the program.<sup>29</sup>

<sup>27</sup> James Loeb to George Jacobson, Feb. 5, 1949; Loeb to James Patton, Feb. 7, 1949; Loeb to Hubert Humphrey, Feb. 17, 1949; Loeb to Joseph P. Lash, Apr. 15, 1949; Loeb, radio speech, Mar. 8, 1949, all in ADA Papers.

<sup>28</sup> Matusow, *Farm Policies and Politics*, 115-19, 194-200.

<sup>29</sup> Grafton in the *Chicago Sun-Times*, Apr. 20, 1949; St. Louis *Post-Dispatch*, Apr. 8, 1949; Angus McDonald, "The Fair Deal's Farm Program," *New Republic*, May 2, 1949, pp. 11-13; editorial paragraph, *Nation*, Apr. 16, 1949, pp. 429-30; "Patton Praises Principles of Brannan Plan," *National Union Farmer*, Apr. 1949, p. 8.

The plan immediately ran into the opposition of the conservatives who dominated Congress. Republicans feared that the political coalition Brannan was trying to build would entrench the Democrats in power. Large producers, most effectively represented by the powerful Farm Bureau Federation, regarded the plan as discriminatory, and many Democrats with ties to the Farm Bureau refused to support it, among them Senate majority leader Scott Lucas and Clinton Anderson, now the freshman senator from New Mexico. By June it was obvious to most political analysts that the Brannan Plan had no chance of passage in 1949. The administration and most liberals nevertheless remained optimistic. The issue seemed good, the alignment of interests logical and compelling: enough political education and campaigning could revive the scheme and revolutionize American politics.<sup>30</sup>

Both the CIO and the Farmers Union undertook campaigns to spread the message of farmer-labor unity. An article in the *National Union Farmer* typified the effort:

Workers today are in a tough spot, just like farmers. Production has been steadily declining, and that means fewer jobs and lower wages. And that means smaller markets for farm products. This worries everybody but Big Business, but these advocates of scarcity still rule the roost. Monopoly wants less production, less employment, lower wages, fewer family farmers, less collective bargaining, lower farm prices and less competition except for jobs. . . . There is little basic difference between the labor fight against the Taft-Hartley law, and our fight against attempts to tax cooperatives out of existence. . . . Labor's strong objections to 40¢ an hour as a minimum is no different than our equally strong objections to 60% of parity.<sup>31</sup>

Brannan campaigned extensively for his program. "Farm income equals jobs for millions of American workers," he told a labor gathering in a typical effort. "Together, let workers and farmers unite in achieving a full employment, full production economy." The administration sponsored regional farmer-labor conferences around the country. The one attracting the most attention was held in June at Des Moines, Iowa, and featured prominent labor leaders, important Democratic congressmen, and Vice-President Alben Barkley. Other such grass-roots meetings were organized as far east as upstate New York, and the Democratic National Committee prepared a pamphlet on the Brannan Plan for mass distribution. On Labor Day the president devoted two major appearances, one in Pittsburgh and the other in Des Moines, to the Brannan Plan and to farmer-labor unity. "Those who are trying to set these two great groups against each other just

<sup>30</sup> Matusow, *Farm Policies and Politics*, 199-201, 204-19; *Public Papers*, 1949, 292-93.

<sup>31</sup> "Workers Back Brannan Plan," *National Union Farmer*, June 1949, p. 3; "Labor Needs Taft-Hartley Repeal, 75¢ Minimum Wage," *ibid.*, 7; "Hear Labor's View of Brannan Plan. ABC Network, 20th," *ibid.*; "Urge Closer Farm Labor Cooperation," *ibid.*, Aug. 1949, p. 3.

have axes of their own to grind," he warned his Pittsburgh audience. "Price supports must . . . give consumers the benefit of our abundant farm production," he told his Des Moines listeners.<sup>32</sup>

Many liberals and Democratic politicians remained convinced that they had an overwhelming political strategy. "In 1950 and '52, the Brannan Plan will be the great issue in the doubtful states," wrote journalist A. G. Mezerik. "After that, Congress will enact a new farm bill—one which is based on low prices for consumers and a high standard of living for family farmers." In early 1950 the Brannan Plan seemed to be gaining popular support. Liberals inside and outside the administration continued to hope for vindication at the polls in November. They could not, of course, foresee the Korean War and the ways in which it would change the shape of American politics.<sup>33</sup>

Even without the Korean War, however, even without the disruptive impact of McCarthyism, it is doubtful that the Brannan Plan would have worked the miracles expected of it. The liberals inside and outside the administration who had created or worked for it assumed that urban and rural groups could be united simply on grounds of mutual self-interest. They failed to understand that these groups were not deeply concerned with *mutual* self-interest; both sides had practiced with some success methods that had taken care of their own self-interest. The rhetoric about urban-rural interdependence was extremely superficial, talked but not deeply felt. Most farm and labor leaders, even those progressive in their outlook, hardly had a basis for communication. The ADA conference of February 1949 included some of the best-informed figures from the unions and the farms. Yet one of the labor leaders had to ask for an explanation "in simple language" of the concept of parity. One of the farm leaders then admitted that he had no idea what the dues check-off was or how it worked. The farm leaders also frankly commented that their constituents were strongly against such things as a minimum wage applied to farm workers, the extension of social security to cover farm labor and farmers in general, and especially the re-establishment of any sort of price controls. The situation at Des Moines seems to have been much the same. Even some of the Farmers Union officials at the conference were annoyed by the presence of the labor people. "Some farmers wondered if they weren't being sucked in to help the forces of labor fight the Taft-Hartley Act," reported journalist Lauren Soth. Such ideas, of course, were not entirely fanciful. Most of the observers at Des Moines sensed the artificiality of the

<sup>32</sup> Brannan, "The Challenge of Our Era," address delivered to Labor's League for Political Education, in Chicago, Sept. 5, 1949, Clifford Papers; Paul E. Fitzpatrick to Maurice Tobin, May 3, 1950, and Fitzpatrick to Brannan, May 8, 1950, Harry S. Truman Papers, OF 300, Truman Library; Matusow, *Farm Policies and Politics*, 199-200; *Public Papers, 1949*, 460, 467-68.

<sup>33</sup> A. G. Mezerik, "The Brannan Plan," *New Republic*, Nov. 28, 1949, pp. 11-13; Matusow, *Farm Policies and Politics*, 218-21.



whole affair, but they continued to hope that further contacts would consummate the union of city and country.<sup>34</sup>

The farm leaders harbored a provincial suspicion of labor, while the reverse was true in the cities. "While labor has given general support to the Brannan plan, I have had the suggestion made, almost ironically, that labor might be given a guaranteed income if such were to be granted to farmers," remarked Jim Loeb in November 1949. Many liberals felt that, as proposed by the administration, the Brannan Plan was too generous. The Chicago *Sun-Times* and the *Nation* agreed that the principles and machinery of the Brannan system were excellent, but both dissented from Brannan's proposal to support farm income at record heights. "The country as a whole should not undertake to support farm income at a higher level than is fair and just," warned the *Sun-Times*, adding that it would always be easier to raise supports than to lower them. Chester Bowles went a step further when he proposed that the whole matter of agricultural subsidies should be tied to urban employment with no supports at all during periods of full employment. Such ideas were hardly the cement of a new urban-rural coalition.<sup>35</sup>

Many urban liberals found the plan itself difficult to grasp and could not work up much enthusiasm about it. "Most of us do not understand it completely," admitted Jim Loeb a month and a half after its introduction. A group of ADA leaders had a cordial meeting with Brannan in June 1949 and pledged their support. Actually, however, the ADA did little to promote the program. In the spring of 1950 a Philadelphia liberal wrote to the organization asking for information on the issue, but Violet Gunther, the legislative director, replied that the ADA had published nothing other than an endorsement in the platform, nor could she think of any group other than the Farmers Union that might have something available. The *Nation* and the *New Republic* gave only occasional mention to the plan. Most liberals could heartily endorse and even get excited about Brannan's political objectives, but understanding and identifying with the scheme itself was quite a different matter.<sup>36</sup>

For a time in early 1950 declining farm prices seemed to generate a surge of support for the Brannan Plan. At the beginning of June, Albert Loveland, the undersecretary of agriculture, won the Iowa Democratic senatorial primary on a pro-Brannan platform and thereby encouraged the administration to believe that the Midwest was moving in its direction. Just a few weeks later, however, the Korean War began, creating situations and pressures that doomed most of the Fair Deal.<sup>37</sup>

<sup>34</sup> Loeb, radio speech, Mar. 8, 1949, ADA Papers; Milburn P. Akers in the Chicago *Sun-Times*, June 15, 1949; Lauren Soth, "Democrats in Des Moines," *Reporter*, July 19, 1949, pp. 4-7.

<sup>35</sup> Loeb to J. M. Kaplan, Nov. 1, 1949, ADA Papers; Chicago *Sun-Times*, July 12, 1949; "New Farm Plan Needed," *Nation*, June 11, 1949, pp. 649-50; New York *Times*, Apr. 4, 1950.

<sup>36</sup> Loeb to David Williams, May 20, 1949; Loeb to Brannan, June 15, 1949; Violet Gunther to Wilbur Hitchcock, May 10, 1950, all in ADA Papers.

<sup>37</sup> Matusow, *Farm Policies and Politics*, 220-21.

Even if the Brannan Plan had become law, it is far from certain that it would have created the dream farmer-labor-liberal coalition. Most leading agricultural economists, including those of a progressive outlook, were convinced that the proposal would be unworkable and prohibitively expensive. Some liberal economists condemned its failure to give the rural poor at least as much aid as the middle-class family farm.<sup>38</sup> Even assuming that the economists were wrong, there is no guarantee that a smoothly functioning Brannan program could have performed the neat trick of uniting the very different cultures of urban liberalism and rural insurgency; such a feat probably would have required more than mutual economic benefits. The down-to-earth, church-social ethos of the Farmers Union would not automatically homogenize with the sophisticated, intellectual progressivism of the city liberals or the wage-and-hour, union-shop, reformism of labor.

DURING 1949 AND EARLY 1950 the Truman administration managed a record of substantial legislative accomplishment, but it consisted almost entirely of additions to such New Deal programs as the minimum wage, social security, and public power. The Housing Act of 1949, with its provisions for large-scale public housing, appeared to be a breakthrough, but weak administration, local opposition, and inadequate financing subsequently vitiated hopes that it would help the poor. Acting on his executive authority, Truman took an important step by forcing the army to agree to a policy of desegregation. The heart of the Fair Deal, however—repeal of the Taft-Hartley Act, civil rights legislation, aid to education, national medical insurance, and the Brannan Plan—failed in Congress. Given the power of the well-entrenched conservative coalition and a widespread mood of public apathy about big new reforms, Truman could only enlarge upon the record of his predecessor.<sup>39</sup>

Democratic strategists hoped for a mandate in the congressional elections of 1950. In the spring Truman made a successful whistle-stop tour of the West and Midwest, rousing party enthusiasm and apparently demonstrating a solid personal popularity. Loveland's victory provided further encouragement, and in California the aggressive Fair Dealer Helen Gahagan Douglas won the Democratic nomination for the Senate by a thumping margin. Two incumbent Fair Deal supporters—Frank Graham of North Carolina and Claude Pepper of Florida—lost their senatorial primaries, but, as Southerners who had run afoul of the race issue, they did not seem

<sup>38</sup> *Ibid.*, 202-04.

<sup>39</sup> Neustadt, "Congress and the Fair Deal"; Richard O. Davies, *Housing Reform during the Truman Administration* (Columbia, Mo., 1966), 101-42; Richard M. Dalfiume, *Desegregation of the U.S. Armed Forces: Fighting on Two Fronts, 1939-1953* (Columbia, Mo., 1969), ch. 9. On the public apathy toward reform, see Mildred Strunk, ed., "The Quarter's Polls," *Public Opinion Quarterly*, 13 (1949): 154-76, 346-71, 537-61, 709-32.

to be indicators of national trends.<sup>40</sup> Nevertheless, the hope of cutting into the strength of the conservative opposition ran counter to the historical pattern of mid-term elections. The beginning of the Korean War at the end of June destroyed any chances of success.

The most immediate impact of Korea was to refuel an anti-Communist extremism that might otherwise have sputtered out. Senator Joseph R. McCarthy had begun his rise to prominence in February 1950, but he had failed to prove any of his multiple allegations and seemed definitively discredited by the investigations of a special Senate committee headed by Millard Tydings. McCarthy, it is true, was a talented demagogue who should have been taken more seriously by the liberals and the Truman administration in early 1950, but it seems probable that his appeal would have waned more quickly if the cold war with communism had not suddenly become hot. As it was, many of his Senate colleagues rushed to emulate him. In September 1950 Congress passed the McCarran Internal Security Act; only a handful of congressional liberals dared dissent from the overwhelming vote in favor. Truman's subsequent veto was intelligent and courageous, but was issued more for the history books than with any real hope of success. In the subsequent campaign, liberal Democrats, whether they had voted for the McCarran Act or not, found themselves facing charges of softness toward communism.<sup>41</sup>

The war hurt the administration in other ways. It touched off a brief but serious inflation, which caused widespread consumer irritation. By stimulating demand for agricultural products it brought most farm prices up to parity levels and thereby undercut whatever attractiveness the Brannan Plan had developed in rural areas. Finally it removed the Democratic party's most effective spokesman—the president—from active participation in the campaign. Forced to play the role of war leader, Truman allowed himself only one major partisan speech, delivered in St. Louis on the eve of the balloting.

The Fair Deal might have been a winning issue in a nation oriented toward domestic concerns and recovering from an economic recession; it had much less appeal in a country obsessed with Communist aggression and experiencing an inflationary war boom. The reaction against the administration was especially strong in the Midwest. Indiana's Democratic aspirant for the Senate asked Oscar Ewing to stay out of the state. In Iowa, Loveland desperately attempted to reverse his identification with the Brannan Plan. In Missouri the managers of senatorial candidate Thomas

<sup>40</sup> "Exit Senator Pepper," *Nation*, May 13, 1950, pp. 436-37; Carleton Kent, "Harry Goes A-Hunting," *ibid.*, 466-67; "Election Overture," *ibid.*, 514-15; *Chicago Sun-Times*, May 15, 1950; *St. Louis Post-Dispatch*, May 3, June 26, 27, 1950; ADA press release, June 18, 1950, Stephen J. Spingarn Papers, Truman Library.

<sup>41</sup> On the rise of McCarthyism, see Phillips, *Truman Presidency*, 372-93; Alan D. Harper, *The Politics of Loyalty: The White House and the Communist Issue, 1946-1952* (Westport, Conn., 1969), chs. 6-7, apps. 3-4; Robert Griffith, *The Politics of Fear: Joseph R. McCarthy and the Senate* (Lexington, Ky., 1970), chs. 3-5.

C. Hennings, Jr. privately asked White House aides to make Truman's St. Louis speech a foreign policy address that would skip lightly over Fair Deal issues. A few days before the election the columnist Stewart Alsop returned from a Midwestern trip convinced that the region had never been more conservative. Nevertheless, Truman's political advisers, and probably Truman himself, felt that the Fair Deal still had appeal. Given the basic strength of the economy and the victories in Korea that followed the Inchon landing, the White House believed that the Democrats could easily rebut generalized charges of fumbling or softness toward communism. In mid-October the Democratic National Committee and many local leaders were so confident of success that their main concern was simply to get out the vote.<sup>42</sup>

The November results, however, showed a Democratic loss of twenty-eight seats in the House of Representatives and five seats in the Senate. Truman seized every opportunity to remind all who would listen that the numbers were small by traditional mid-term standards. Liberal political analysts, including Kenneth Hechler, a White House staffer, and Gus Tyler of the International Ladies Garment Workers Union, subjected the returns to close scrutiny and all but pronounced a Democratic victory. All the same, most of the Democrats who went under had been staunch Fair Dealers. Republican candidates, including John Marshall Butler in Maryland, Richard M. Nixon in California, Everett McKinley Dirksen in Illinois, and Robert A. Taft in Ohio, scored some of the most spectacular GOP victories by blending right-wing conservatism with McCarthyism. The Midwestern losses were especially disappointing. Hechler argued that the corn-belt vote primarily reflected urban defections and that the Democrats had done comparatively well among farmers. Perhaps so, but for all practical purposes the results put an end to the Brannan strategy of constructing a farmer-labor coalition. Truman was probably more accurate than Hechler when, with characteristic overstatement, he privately expressed his disappointment: "The main trouble with the farmers is that they hate labor so badly that they will not vote for their own interests."<sup>43</sup>

Thereafter, with the Chinese intervention transforming the Korean

<sup>42</sup> Matusow, *Farm Policies and Politics*, 220-21; Stewart Alsop in the St. Louis *Post-Dispatch*, Nov. 4, 1950; George Elsey, memorandum to Charles Murphy, Oct. 30, 1950, Elsey Papers; Kenneth Hechler, memorandums, Aug. 30, Oct. 24, 27, 1950, *ibid.*; David Lloyd, memorandum to Charles Murphy, Sept. 28, 1950, David Lloyd Papers, Truman Library; *Public Papers of the Presidents: Harry S. Truman, 1950* (Washington, 1965), 697-703.

<sup>43</sup> Hechler, memorandum on the 1950 elections, Nov. 15, 1950, Charles Murphy Papers, Truman Library; Matusow, *Farm Policies and Politics*, 220-21; *Public Papers, 1950*, 713; T. R. B., "Washington Wire," *New Republic*, Nov. 20, 1950, pp. 3-4; Harold Ickes, "Fear Rides Herd," *ibid.*, 17; Gus Tyler, "The Mid-Term Paradox," *ibid.*, Nov. 27, 1950, pp. 14-15; Thomas L. Stokes in the *Washington Evening Star*, Nov. 13, 1950; W. McNeil Lowry, "Frustration in the Corn Belt," *Progressive*, Dec. 1950, pp. 14-16; James Patton, "'The People Have Spoken,'" *National Union Farmer*, Nov. 1950, pp. 1, 2; Elmer Davis, news commentary, Nov. 8, 1950, Elmer Davis Papers, Library of Congress; Truman to Aubrey Williams, Nov. 18, 1950, Aubrey Williams Papers, Franklin D. Roosevelt Library, Hyde Park, N.Y.

War into a more serious conflict and with the dismissal of General Douglas MacArthur in April 1951, Truman faced a tough attack from a Republican opposition determined to capitalize upon the frustrations of Korea. Finding it necessary to place party unity above all else, he quietly shelved most of his domestic legislative program and sought to bring the conservative wing of his party behind his military and defense policies. He secretly asked Richard B. Russell of Georgia, the kingpin of the Southern conservatives, to assume the Democratic leadership in the Senate. Russell, content with the substance of power, declined and gave his nod to Ernest W. McFarland of Arizona, an amiable tool of the Southern bloc; Truman made no effort to prevent McFarland's selection as Senate majority leader. The president's State of the Union message was devoted almost entirely to foreign policy and defense mobilization and mentioned social welfare programs only as an afterthought. Subsequently Truman told a press conference that while he supported the Fair Deal as much as ever, "first things come first, and our defense programs must have top priority."<sup>44</sup>

Truman's success in achieving a minimum degree of party unity became apparent in the weeks of investigation and accusation that followed General MacArthur's return to America. Russell, playing the role of parliamentarian-statesman to the hilt and cashing in on his great prestige with senators of both parties, chaired the Senate committee that looked into the MacArthur incident, and he saw to it that the administration was able to deliver a thorough rebuttal to the general. The Northern liberal, Brien McMahon of Connecticut, relentlessly grilled hostile witnesses. The Western representative of oil and gas interests, Robert S. Kerr of Oklahoma, lashed out at MacArthur himself with a vehemence and effectiveness that no other Democrat could match. The tandem efforts of Russell, McMahon, and Kerr demonstrated the new party solidarity, but in terms of the Fair Deal the price was high.

In July 1951 the Federal Power Commission renounced the authority to regulate "independent" (non-pipeline-owning) natural gas producers. The ruling amounted to an administrative enactment of a bill, sponsored by Kerr, which Truman had vetoed a year earlier; Truman's close friend and most recent appointee to the Federal Power Commission, Mon Wallgren, cast the deciding vote. Although he talked like a militant liberal in a private conversation with ADA leaders, the president stalled throughout 1951 on repeated demands for the establishment of a Korean War Fair Employment Practices Committee. In December the administration established an ineffective Committee on Government Contract Compliance. Other domestic programs were soft-pedaled to near-invisibility.<sup>45</sup>

Yet even the Korean War was not entirely inimical to reform. Its exi-

<sup>44</sup> Rowland Evans and Robert Novak, *Lyndon B. Johnson: The Exercise of Power* (New York, 1966), 41-43; *Public Papers of the Presidents: Harry S. Truman, 1951* (Washington, 1965), 6-13, 18, 22; Poen, "The Truman Administration and National Health Insurance," 214-19; memorandums in "Taft-Hartley-1952" file, David Stowe Papers, Truman Library.

<sup>45</sup> Douglass Cater, "A Parliamentarian, A Hatchet Man, an Inquisitor," *Reporter*, June 12,

gencies forced the army to transform its policy of integration into practice.<sup>46</sup> Korea also provided a test for one of the basic underpinnings of the Fair Deal—Leon Keyserling's philosophy of economic expansion. Truman did not in the end fully embrace Keyserling's policies, but in the main he followed the guidance of his chief economic adviser. The Korean War years demonstrated the extent to which Keyserling's economics diverged from conventional New Deal–World War II Keynesianism and revealed both the strengths and weaknesses of his approach.

From the outbreak of the fighting, most liberals favored either immediate strong economic controls akin to those that had held down inflation in World War II or at least the establishment of stand-by machinery that could impose them rapidly. Truman disliked such measures on the basis of both principle and politics. He and his diplomatic advisers also wanted to signal the Soviet Union that the United States regarded the North Korean attack as a limited challenge meriting a limited response. Keyserling's expansionary economics provided an attractive alternative to the liberal clamor for controls. Convinced that extensive controls would put the economy in a strait jacket and retard the expansion necessary to meet both consumer and defense needs and assuming a North Korean defeat in a few months, the administration decided to accept a short-term, war-scared inflation (probably unavoidable in any case) and concentrate on economic growth, which would be underwritten in large measure by tax incentives for business. An expanding economy would be the best long-term answer to inflation: growth policies could fit a small war into the economy, avoid the social and political strains accompanying wartime controls, and reduce inflationary pressures to a level at which fiscal and monetary policies could contain them. Liberals outside the administration watched with alarm as prices went up, but Truman and Keyserling continued to gamble on a quick end to the war and the development of an economy capable of producing both guns and butter.<sup>47</sup>

Their plan might have worked fairly well had the United States not

1951, pp. 31–32; Willard Shelton, "Presidential Appointments," *Nation*, Feb. 16, 1952, p. 149; "Costly Alliance," *Progressive*, Dec. 1951, p. 3; St. Louis *Post-Dispatch*, July 20, 22, 1951; Francis Biddle to Truman, May 22, 1951, ADA Papers; Biddle to Emil Rieve, June 14, 1951, *ibid.*; John Tucker to Biddle, Aug. 16, 1951, *ibid.*; Violet Gunther, memorandum to Reginald Zalles [early Nov. 1951], *ibid.*; Herbert Garfinkle, *When Negroes March* (New York, 1969), 176–77. Although there can be no doubt that the administration stalled on the civil rights order for political reasons, it is also true that the twilight nature of the Korean War placed formidable legal difficulties in the way of a strong Fair Employment Practices Committee. See Fred Lawton, memorandum for the record, Feb. 1, 1951, Fred Lawton Papers, Truman Library; Charles Murphy, memorandum to Truman, Dec. 1, 1951, Truman Papers, OF 40; "FEPC for Defense," *New Republic*, Dec. 17, 1951, p. 6.

<sup>46</sup> Dalhume, *Desegregation of the U.S. Armed Forces*, ch. 10.

<sup>47</sup> For the administration position on controls, see Edward S. Flash, Jr., *Economic Advice and Presidential Leadership: The Council of Economic Advisers* (New York, 1965), 43–44; *Public Papers, 1950*, 561–64, 568–69, 589–90; Richard Neustadt, memorandum to Kenneth Hechler, Apr. 28, 1952, Elsey Papers; and author's interview with Keyserling. For liberal criticism, see, e.g., Chester Bowles to Burnet R. Maybank in the *Congressional Record*, 81st Cong., 2d sess., Aug. 4, 1950, pp. 11825–26; ADA mobilization program, *ibid.*, 11826–27; Hubert Humphrey in *ibid.*, app., pp. A5647–49; St. Louis *Post-Dispatch*, July 28, 1950; "Washington Fiddles," *Nation*, Aug. 26, 1950, pp. 178–79; "Galloping Inflation," *New Republic*, Oct. 2, 1950, pp. 5–6.

overreached itself militarily in Korea. The Chinese intervention of November 1950 wrecked hopes of a quick recovery, set off another round of scare buying, and intensified war demands upon the economy. The administration quickly threw up a price-wage control structure, but by the end of February 1951, eight months after the beginning of the Korean conflict, the consumer price index had risen eight per cent (an annual rate of twelve per cent). Keyserling agreed that the new situation necessitated controls, but he accepted them with reluctance and sought to keep them as simple as possible, even at the risk of benefiting profiteers. "We'll never be able to out-control the Russians," he told a Senate committee, "but we can out-produce them." Speaking to an ADA economic conference, he asserted that many liberals, in their opposition to tax breaks for large business and in their demands for stronger controls, were confusing the Korean War with World War II and "engaging merely in hackneyed slogans out of the past."<sup>48</sup>

Most liberals disagreed with Keyserling's emphases. As production was his first imperative, an end to the wage-price spiral was theirs. "Unless we are willing seriously to endanger the basis of existence of the American middle class, we must stop prices from rising," wrote Hans Landsberg in the *Reporter*. The liberals assumed that economic expansion was possible within a framework of rigid, tightly administered controls. Chester Bowles observed that the controlled economy of World War II had turned out a twofold increase in industrial production. John Kenneth Galbraith rejected the idea that Keyserling's expansionary policies could outrun the inflationary pressures they themselves created. The bulk of liberals regarded the administration approach as dangerous, the product of political expediency rather than sound economic analysis.<sup>49</sup>

Neither Keyserling nor the more conventional liberals won a complete victory. Truman, who understood all too well the political dangers of a prolonged inflation, made substantial concessions to the controllers, led by Michael V. DiSalle, head of the Office of Price Stabilization. In the interest of fairness Truman approved a more complex system of price controls than Keyserling thought desirable, giving DiSalle considerable leeway to roll back some prices while approving advances in other areas. By March 1951 inflation was under control; during the final ten months of the year the cost-of-living index increased by less than two and one-half per cent. The waves of scare buying that followed the North Korean attack and

<sup>48</sup> Flash, *Economic Advice and Presidential Leadership*, 69-76; *New York Times*, Nov. 30, Dec. 11, 12, 1950, May 10, 1951; Keyserling, speech to ADA conference, Washington, D.C., May 18, 1951, ADA Papers; Harold Enarson, memorandum to David Stowe, Apr. 11, 1951, Harold Enarson Papers, Truman Library.

<sup>49</sup> Landsberg, "Who Pays for Mobilization?" *Reporter*, Mar. 20, 1951, pp. 6-8; Galbraith, "The Taxonomy of Inflation Control," *ibid.*, July 10, 1951, pp. 9-11; Bowles, public letter to Paul H. Douglas, May 31, 1951, copy attached to Bowles to Charles Murphy, June 6, 1951, Murphy Files, Truman Papers.

the Chinese intervention had subsided. Higher taxes and restraints on credit were beginning to affect consumer buying. The Federal Reserve System, despite opposition from the administration, initiated a stringent monetary policy. Tax breaks for businesses expanding plant facilities presaged increased productive capacity. All these factors, along with the government's stabilization program, discouraged an inflationary psychology.<sup>50</sup>

At the time, however, it appeared to most economic observers that the lull was only temporary. Many of the administration's liberal critics refused even to admit the existence of a lull and called for tougher controls as if prices were still skyrocketing. More moderate analysts feared that the impact of large government defense orders would set off another inflationary spiral in the fall. Influenced by such expectations, Truman ostentatiously mounted an anti-inflation crusade, demanding that Congress not only extend his control authority, due to expire on June 30, but actually strengthen it. In fact the Defense Production Act of 1951 weakened the president's powers considerably. Truman signed it reluctantly, comparing it to "a bulldozer, crashing aimlessly through existing pricing formulas, leaving havoc in its wake." A subsequent tax bill failed to meet administration revenue requests and increased the danger of serious inflation.<sup>51</sup>

Yet price stability persisted through 1952, in large measure because defense production, hampered by multiple shortages and bottlenecks, lagged far behind its timetable. In late 1951 these problems and the fear of renewed inflation led Truman to decide in favor of a "stretch-out" of defense production schedules; in doing so he overrode Keyserling's urgings for an all-out effort to break the bottlenecks and concentrate relentlessly upon expansion. Given the serious problems in defense industry, the stretch-out decision may have seemed necessary to Truman, but it also carried the dividend of economic stability.

The president had steered a course between the orthodox liberal obsession with inflation and Keyserling's easy disregard of its perils; perhaps as a result the economy failed to expand at the rate Keyserling had hoped. On balance, however, Truman's approach to the political economy of the Korean War was closer to Keyserling's, and the conflict produced a dramatic economic growth. Before the war the peak gross national product had been \$285 billion in 1948; by the end of 1952 the GNP (measured in constant dollar values) had reached a rate of \$350 billion. The production index of durable manufactured goods had averaged 237 in 1950; by the last quarter of 1952 it had reached 313. The expansion, even if less than

<sup>50</sup> Arthur Viner, "What Happened to Inflation?" *Reporter*, Apr. 15, 1952, pp. 32-34; *Statistical Abstract of the United States, 1953* (Washington, 1953), 307.

<sup>51</sup> Examples of continued liberal concern about inflation are Thomas L. Stokes in the *Washington Evening Star*, Apr. 4, 1951; ADA press release, Apr. 16, 1951, ADA Papers; "Are We Licking Inflation?" *New Republic*, Apr. 30, 1951, p. 6; and *St. Louis Post-Dispatch*, June 12, 1951. For Truman's course, see *Public Papers, 1951*, 244-53, 320-21, 333-38, 364, 369, 435-37, 478-83, 590.



Keyserling had wanted, was breathtaking. Moreover, aside from the probably unavoidable inflation that accompanied the early months of the war, this remarkable growth had occurred in a climate of economic stability. Using a somewhat more orthodox approach than Keyserling preferred, the administration had achieved one of the central goals of the Fair Deal.<sup>52</sup>

In its effort to carry on with the reforming impulse of the New Deal the Truman administration faced nearly insuperable obstacles. A loosely knit but nonetheless effective conservative coalition had controlled Congress since 1939, successfully defying Franklin Roosevelt long before it had to deal with Truman. Postwar prosperity muted economic liberalism and encouraged a mood of apathy toward new reform breakthroughs, although Truman's victory in 1948 indicated that most of the elements of the old Roosevelt coalition were determined to preserve the gains of the New Deal.<sup>53</sup> The cold war probably made it more difficult to focus public attention upon reform and dealt severe blows to civil liberties. It did, however, give impetus to the movement for Negro equality.

The Fair Deal attempted to adapt liberalism to the new conditions. Under the intellectual leadership of Leon Keyserling it formulated policies that sought to transcend the conflicts of the New Deal era by encouraging an economic growth that could provide abundance for all Americans. With Charles Brannan pointing the way, the Truman administration tried to translate abundance into a political coalition that could provide the votes for its social welfare policies. The political strategy, ambitious but unrealistic, collapsed under the weight of the Korean War. Keyserling's economics, on the other hand, received a lift from Korea; in a period of adversity the Fair Deal was able to achieve at least one of its objectives.

<sup>52</sup> *Statistical Abstract*, 1953, 786; Flash, *Economic Advice and Presidential Leadership*, 85-99; *Public Papers of the Presidents: Harry S. Truman, 1952-53* (Washington, 1966), 1179.

<sup>53</sup> Samuel Lubell, *The Future of American Politics* (3d ed. rev.; New York, 1965), remains the most influential analysis of the deadlock of the Roosevelt coalition.

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# Marx and the Agrarian Question

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OSCAR J. HAMMEN

THE FIRST SUCCESSFUL REVOLUTION avowedly inspired and guided by the ideology and historical interpretation of Karl Marx and Friedrich Engels took place in Russia in 1917. Since this event occurred in a mainly agricultural state with a relatively small industrial establishment, it is often listed as one of the ironies of history. This has led some scholars to the conclusion that Lenin, the director of the Bolshevik forces, gave an un-Marxian twist to Marx, deviating from the theories of the master in the process.

The theories of Marx and Engels certainly stressed the primary revolutionary role of an industrial proletariat in the more advanced capitalistic states. Marx and Engels in their critiques also depended heavily on the historical experience and the contemporary condition of England in demonstrating the inherent contradictions and class conflicts that resulted from changing modes of production.<sup>1</sup> With the expansion of industry and the application of capital, associated as they were with the presence of an ever-growing and more numerous proletariat, the capitalist system produced its own hangmen. The above line of thought also reflected an application of Marxian dialectics, with an emphasis on the clash of opposites, class conflict, and the advance of civilization proceeding from this logic.

It must be noted, though, that Marx and Engels were so certain that the course of history pointed toward the ultimate triumph of communism that they were ready to support any revolutionary movement that promised to eliminate a variety of roadblocks (political, economic, and social institutions, and even "reactionary nations") that needed to be removed to clear the way for the last climactic confrontation between proletariat and bourgeoisie. A pattern that such upheavals were likely to follow was always sought by Marx in the great French Revolution—a revolution that had

This article represents an expanded version of a paper that was read on December 30, 1970, at the annual meeting of the American Historical Association in Boston.

<sup>1</sup> Beginning with Engels's "Umrisse zu einer Kritik der Nationalökonomie," in the *Deutsch-Französische Jahrbücher* (Paris, 1844), followed by *Die Lage der Arbeitenden Klasse in England* (Leipzig, 1845), and culminating in Marx's *Kapital* (Hamburg, 1867).

followed an "ascending line," as the "more progressive parties" pushed their predecessors aside and sent them to the guillotine.<sup>2</sup> A personal factor also entered the picture, because Marx and Engels were anything but mere theorists content to watch history follow its own fumbling but destined path in the ripeness of time. They expected to witness the predicted revolutions and to participate directly in them.<sup>3</sup> This helps to account for their unmatched display of "tactical shuffling," as Joseph A. Schumpeter once called it,<sup>4</sup> their readiness to identify themselves with the grievances of diverse groups—worker, agrarian, national, political, economic, and even religious—that promised to advance the "movement" (*Bewegung*, a favorite expression of Marx and Engels).

Scholars who are interested primarily in the theoretical side of Marx and Engels are apt to frown on any undue, or perhaps merely proportionate, emphasis on Marx as an active promoter of revolutions, with all the tactical adjustments to diverse national settings and local circumstances that this entailed. Such a stress on tactics appears to be a derogation from the role that theory occupied in the thoughts of Marx and Engels. Actually it underlines the importance of the theoretical assumptions. Marx and Engels could recommend and practice all manner of deviations to promote the destruction of an existing system precisely because they were completely confident that their theoretical conclusions were infallible, that they moved with the stream of history. The communists who comprehended the "movement" remained in a position to guide events in the destined historical direction. This confidence persisted even though Marx ultimately shed some of the "comfortable delusions and the almost childish enthusiasm" with which Marx and Engels had greeted the 1848 revolutions. Marx, moreover, had come to recognize the "role that stupidity plays in revolutions and how the rascals [*Lumpen*] exploit it."<sup>5</sup>

<sup>2</sup> Marx, "Der achtzehnte Brumaire des Louis Bonaparte," *Karl Marx/Friedrich Engels: Werke* (hereafter *Werke*) (Berlin, 1956-68), 8: 135.

<sup>3</sup> Their commitment to revolutionary involvement in the "movement" is illustrated by the expectations expressed in section 4 of the *Communist Manifesto* and in their participation in the revolutions of 1848-49, notably in Germany. In the last "red" number of the *NRZ* (*Neue Rheinische Zeitung. Organ der Demokratie*), May 19, 1849, they promised their followers that they would return when the action started again. Their hopes for an early return to the continent following another eruption of the "revolutionary volcano" persisted into 1850 and are well documented. The Marx-Engels correspondence (*Briefwechsel*) in the years that followed demonstrates the same expectations. See the following representative letters: Engels to Marx, Nov. 27, 1851; Marx to Engels, Mar. 5, 1856; Marx to Engels, Sept. 26, 1856; Engels to Marx, Feb. 11, 1858; Marx to Engels, Feb. 14, 1858; Engels to Marx, Apr. 11, 1859; Engels to Marx, Apr. 10, 1866; Engels to Marx, Nov. 29, 1868. The *Briefwechsel*, aside from special editions, appears in the four volumes in part 3 of *MEGA, Karl Marx/Friedrich Engels. Historisch-Kritische Gesamtausgabe. Werke/Schriften/Briefe* (Frankfurt, Berlin, Moscow, 1927-35). References in this article will be to the *MEGA* publication as follows: *Briefwechsel*, 1, 2, 3, or 4. The *Briefwechsel* also appears in volumes 27-35 of the *Werke*. Engels's testimonial at the graveside of Marx in 1883 stresses the fact that the "scientist and theorist in Marx represented only half of the man." See Engels, "Das Begräbnis von Karl Marx," in *Werke*, 19: 335-37.

<sup>4</sup> Joseph A. Schumpeter, *Capitalism, Socialism and Democracy* (3d ed.; New York, 1950), 319.

<sup>5</sup> Marx to Engels, Feb. 13, 1863, in *Briefwechsel*, 3: 126-27.

Scholars by and large have neglected the systematic study of the direct participation of Marx and Engels in an actual revolution<sup>6</sup> together with their capacity for tactical adjustments—a capacity that was demonstrated repeatedly and that would give the two men a rôle in a great variety of revolutionary movements without ever causing them to lose sight of their ultimate goals. Yet any appraisal of Marx and Engels and what they represented would appear to be unrealistic and lopsided in the absence of the above considerations. Marxism represents an amalgam of theory and practice.

Marx and Engels, in any event, did not consistently abide by their own apparent theoretical conclusions that pointed to the proletariat in the most advanced industrial lands for the initiative in the communist movement. They occasionally indicated that the virgin soil of a belatedly emerging proletariat constituted a richer seed bed. In the *Communist Manifesto* of 1848 they predicted the early success of a bourgeois revolution in Germany as the “immediate prelude” to a succeeding proletarian revolution. They justified this prediction by asserting that the German upheaval would occur under “generally more advanced conditions of European civilization and a far more developed proletariat” than had existed during comparable English and French revolutions in the seventeenth and eighteenth centuries.<sup>7</sup> With the added maturity of twenty years Marx in 1868 again reflected on the advantages of a late start. Beyond that he then gave the Germans credit for a special national trait: They “had on their shoulders heads that could generalize.”<sup>8</sup> The Germans were the “most theoretical people in Europe and had preserved a theoretical frame of mind [*Sinn*],” Engels wrote concurringly. The lack of similar “frame of mind” among the English and French accounted for their relative insensitivity to Marxian views.<sup>9</sup>

<sup>6</sup> This neglect is most apparent in connection with the role of Marx and Engels in the revolutions of 1848–49, the only revolutions in which they were involved directly and to which they continued to refer as a cardinal event in their lives. Among the more comprehensive but also belated studies, the centenary account by Auguste Cornu, *Karl Marx et la Révolution de 1848* (Paris, 1948), is relatively elemental and thin (merely seventy-four pages). Gerhard Becker's *Karl Marx und Friedrich Engels in Köln 1848–1849* (Berlin, 1963) is scholarly, reasonably comprehensive and commendable, though colored by an obvious communist bias. The only relatively thorough study in the English language is by the author: Oscar J. Hammen, *The Red '48ers: Karl Marx and Friedrich Engels* (New York, 1969).

<sup>7</sup> “Manifest der Kommunistischen Partei,” pt. 4, in *Werke*, 4: 493.

<sup>8</sup> Marx to Engels, Sept. 26, 1868, in *Briefwechsel*, 4: 102.

<sup>9</sup> Engels, “Vorbemerkung zu ‘Der deutsche Bauernkrieg’ (Ausgabe 1870 und 1875),” in *Werke*, 7: 541. Engels spoke later of “two essential advantages” the German workers enjoyed in contrast to other workers. First, as members of the “most theoretical people in Europe,” they had preserved a “theoretical frame of mind.” Without it “scientific socialism” would never have become so much a part of their “flesh and blood.” The absence of this “inestimable advantage” was a major cause of the “indifference toward theory” that accounted for the fact that the English workers “moved so slowly from the spot” as it also accounted for the confusion and mischief that resulted from the French and Belgian dedication to “Proudhonism” and the “caricatured form” of the latter that had been transmitted to the Spaniards and Italians by Bakunin. See Engels to Bebel, Dec. 11, 1884, in *Karl Marx and Friedrich Engels, Correspondence*, (hereafter *Correspondence*) (New York, 1934), 431–32. August Bebel was a noted German Social Democratic leader.

Marx and Engels also declared that the misery of the workingman in the industrially backward countries and regions was more pronounced than in the advanced states, thereby suggesting a perhaps added responsiveness to radical promptings. Engels informed a German audience that the Germans experienced in a more acute form the same economic crises that struck the English.<sup>10</sup> Workers in backward areas also were drawn into the "movement" by the real industrial proletariat elsewhere, Marx asserted. The industrially advanced states produced the same internal "contradictions" (conflict of classes) in the less industrialized countries because of the competition resulting from the expansion of international trade—"the latent proletariat in Germany," for example, was "created by the competition of English industry."<sup>11</sup> By 1847, therefore, Germany had already experienced to a degree the "modern conflict between the bourgeoisie and the working class, and the battle resulting from it, because of her industrial development and German dependence on the world market."<sup>12</sup> The great economic crises, or depressions, likewise affected the backward countries with an equal if not greater force. "It is known," Marx stated in 1850, "that no European land is struck so directly, with the same scope and intensity, as Germany by the effects of an English crisis."<sup>13</sup> It must be noted here that Marx and Engels counted much on the revolutionary impulse resulting from an economic crisis, especially after the experiences of 1848-49. Such crises revealed the contradictions in a capitalist society in a most glaring manner, while simultaneously sharpening the misery of the proletariat and increasing its alleged readiness to revolt against the prevailing system.

It should be evident from the preceding that Marx and Engels were not unduly hemmed in by the apparent implications of their theories that seemed to suggest that the communist, the proletarian, victory was destined to be won first in the most advanced industrialized states. In actual practice Marx and Engels saw certain possibilities in the rela-

<sup>10</sup> Engels, "Zwei Reden in Elberfeld II," in *Werke*, 2: 551-52.

<sup>11</sup> Marx and Engels, "Die deutsche Ideologie," in *ibid.*, 3: 61, 73.

<sup>12</sup> Marx, "Die moralisierende Kritik und die kritisierende Moral," *Deutsche-Brüsseler-Zeitung (DBZ)*, Nov. 18, 1847, in *ibid.*, 4: 351.

<sup>13</sup> Marx and Engels, *Neue Rheinische Zeitung. Politisch-ökonomische Revue* (hereafter *Revue*) (Hamburg, 1850), in *Werke*, 7: 293-94. After many additional years of practical observation Engels noted some added Marxian gains that resulted from a belated industrial development. A great advantage of the Germans could be ascribed to the fact that the "industrial revolution is only just in full swing, while in France and England, so far as the main point is concerned, it is closed." In the latter countries population shifts took place only very slowly; the great mass of the people grew up "in the conditions in which they have later to live, are accustomed to them; even the fluctuations and crises have become something they take practically for granted." Moreover, they were burdened with the memory of past "unsuccessful attempts." Germany, in contrast, lagged behind, and consequently the social upheaval was all the more fundamental. Germany had a "perfectly fresh and intact proletariat, undemoralized by defeats and finally—thanks to Marx—with an insight into the causes of economic and political development such as none of our predecessors possessed." Letter to Bebel, Dec. 11, 1884, in *Correspondence*, 431-32.

tively backward states. The Germans, as noted above, showed a greater philosophical capacity, the ability to "generalize," plus a more retarded development that added to their miseries. The French, on the other hand, possessed a marked revolutionary tradition.

The less industrialized states lacked the vast and numerous industrial proletariat that could be organized to serve as the shock troops of revolution all the more readily because of their concentration in the large cities, often the capital cities. But this deficiency could be offset through tactical alliances with other potentially revolutionary elements. In the large agricultural states (most of the European states) it might be possible to turn to the agrarian question covering a host of grievances and expectations experienced by the rural people, in the hope that agrarian discontent, guided by the ideologically more sophisticated proletariat, could be mobilized for revolution. Marx and Engels always believed that the proletariat, as the class that would ultimately triumph, would play the leading role in such combinations—if not at once, then later.

In writing on Marx and the agrarian question here the greater stress will be on the earlier period extending into the 1850s. This was the time when tactical considerations were more pronounced because of the presence of actual revolutions, which were followed by reflections on the miscarriage of those revolutions and the anticipation of early revolutions to follow. When immediate revolutionary prospects were dim Marx concentrated more strongly on historical and theoretical investigations, so as to make them available as guidelines for the "deluge" to come. In any event, it is impossible within the scope of an article to provide the same coverage for the later and longer period. Enough will be added, however, to suggest that Marx and Engels adhered to the same tactical pattern in the times that followed—despite repeated disappointments resulting from the peasant's tendency to act in a conservative and even "reactionary" manner.

From a theoretical point of view Marx and Engels always felt that the peasant, farm tenant, and agricultural worker were subjected to the same nightmare of exploitation by the bourgeois, capitalist society as were the proletariat, even if in a less apparent, direct, and massive manner. Engels, who devoted a chapter in his first major work, *The Condition of the Working Class in England*, to "The Agricultural Proletariat," explained that as the machine had given birth to an industrial proletariat, so it had also produced an agricultural proletariat, which was employed on big land units owned or rented by large-scale farmers who utilized the latest machines. The lot of day laborers on the land appeared especially miserable; they would become "Chartists and socialists, that is, conscious proletarians." As for the tenant farmer who cultivated a few acres of leased land (in Wales and Ireland), he faced the same ruin that hounded the petty bourgeoisie in the cities. The resulting agrarian dis-

content manifested itself initially in the form of violence and incendiarism.<sup>14</sup> "The abolition of capital . . . is the property question in the sense of the English agricultural day laborer just as it is of the factory worker," Marx asserted.<sup>15</sup> There was little need of speaking of the once proud English "yeomanry"—as a nearly extinct species.

But the small peasant, wherever he existed, was likewise the victim of an inescapable capitalist exploitation and of the forces of competition. France, where two-thirds of the population belonged to the ranks of the peasantry, furnished the classical model to illustrate the situation. But peasants in the Prussian Rhine Province and certain other areas in Germany, as well as in other countries, faced the same problem. Speaking of France, Marx in 1848 insisted that the "rule of the feudal lords has been supplanted by that of the capitalist; the feudal obligations of the peasant have been converted into the burden of the bourgeois mortgage." Agriculture could not flourish under existing civilized conditions in which the peasant was sinking into an increased poverty.<sup>16</sup>

The continued "parceling" or subdivision of peasant holdings, accompanied by the rising price of land, brought a proportionate increase in debt in the form of the mortgage. At the same time the productivity of the land declined; it was impossible to use machinery and other modern devices to improve the yield of such small farms. Each generation started with the burden of a greater debt. One mortgage followed the other until the peasants had to turn to the usurer with his exorbitant interest rates, all in the name of private property. "Only the fall of the capitalist can help the peasant," Marx claimed. "Only an anti-capitalist, a proletarian government can end his economic misery, his social degradation."<sup>17</sup> Marx also expected to cover some aspects of the agrarian question in a series of articles on "Wage Labor and Capital" that began appearing in the *Neue Rheinische Zeitung* (NRZ) in 1849. There he promised to examine the subject under three subdivisions, item two being the "inescapable fall of the middle bourgeois classes and the peasant class under the present system."<sup>18</sup> But Marx typically never got beyond the first item in the projected series. Since the small peasant proprietor, theoretically considered, could never prosper or even survive indefinitely under the capitalist system, it was evident that he might be persuaded to support a proletarian revolution, once the communists succeeded in clarifying his thoughts on the subject and in making him conscious of his

<sup>14</sup> See Engels, "Das Ackerbauproletariat," *Die Lage der arbeitenden Klasse in England. Nach eigener Anschauung und authentischen Quellen*, chap. 4 (Leipzig, 1845), in *Werke*, 2: 473-85.

<sup>15</sup> Marx, "Moralisierende Kritik," in *ibid.*, 4: 341.

<sup>16</sup> Marx, "Thiers' Rede über eine allgemeine Hypothekenbank mit Zwangkurs," NRZ, Oct. 14, 1848, in *ibid.*, 5: 424-25.

<sup>17</sup> Marx, "Die Klassenkämpfe in Frankreich 1848 bis 1850," *Revue*, in *Werke*, 7: 83-84.

<sup>18</sup> Marx, "Lohnarbeit und Kapital," NRZ, Apr. 5, 1849, in *ibid.*, 6: 398.

hopeless position. But this appeared a hopeless task, because the peasant clung to his parcel of land and hoped for better times and perhaps more land. The more practical approach to the creation of an alliance for revolutionary purposes demanded that the communists see the world through the eyes of the peasants. The communists had to win the peasants by tactically identifying themselves with the various agrarian demands, hopes, and grievances. Marx and Engels early recognized the value of going to the land.

The first illustration of this fact spelled out in a formal manner appeared in the "Circular against Kriege," drafted in 1846 by a forerunner of the Communist League, the Communist Correspondence Committee, with its headquarters in Brussels. Hermann Kriege, whom Engels originally sent to Marx as a promising recruit,<sup>19</sup> displayed inadmissible deviations from the correct views after he settled in New York and published a newspaper. The "Circular," after citing many other errors in Kriege's views, attacked his "economics" for supporting a program advocated by "Young America" and favoring the distribution of 160 acres of land and no more to all farmers.

Had Kriege supported the free-soil movement under specific conditions, had he presented it as a movement . . . that necessarily had to lead to communism, had he shown how the communist tendencies in America originally had to present themselves in this agrarian manner that seemingly contradicted communism as such, there would be no cause for complaint,

the "Circular" explained.<sup>20</sup> The talent for tactical elasticity displayed here—a reflection of confidence in the dialectical process—remained a pronounced characteristic of Marx and Engels. They advised another German follower thus: "In a party one must support everything that helps, without having any boring scruples there."<sup>21</sup> In 1848 Marx declared that in the struggle against the existing government (in this case Prussia) they "allied themselves even with [their] enemies." They had accepted the miserable make-up of the opposition for what it was and had allowed their own views to recede into the background in an election that had just been held.<sup>22</sup> In a slightly lighter vein Marx in 1852 wrote that it was permissible to ally oneself with the devil in order to achieve a specific goal in politics—but one had to be sure that one was getting the best of the devil and not the reverse.<sup>23</sup> That Marx and Engels continued to show the same readiness to make tactical adjust-

<sup>19</sup> Engels to Marx, Feb. 22–Mar. 7, 1845, in *Briefwechsel*, 1: 13–14. Engels described Kriege as a "stupendous agitator."

<sup>20</sup> Marx, Engels, *et al.*, "Zirkular gegen Kriege," May 11, 1846, in *Werke*, 4: 8–10.

<sup>21</sup> Marx, Engels, *et al.*, "Brief des Brüsseler kommunistischen Korrespondenz-Komitees an G. A. Köttingen," June 15, 1846, in *ibid.*, 22.

<sup>22</sup> Marx, "Stein," *NRZ*, Feb. 18, 1849, in *ibid.*, 6: 298.

<sup>23</sup> Marx, "Kossuth, Mazzini und Louis-Napoleon," in *ibid.*, 8: 392. The original appeared in the form of a letter to the editor of the New York *Daily Tribune*, Dec. 1, 1852.



ments to existing circumstances is borne out in their correspondence (*Briefwechsel*).

In preparation for the expected revolutions that came in 1848 Marx and Engels posed as democrats. The "democratic party" of which they spoke included the broad masses: workers, the petty bourgeoisie, and the most numerous class, the peasantry. Marx, in addressing the Germans, presented himself also as the champion of the people (*Volk*), though he once suggested that the word "proletariat" be used in place of that "broad, indefinite expression."<sup>24</sup> Engels expressed it more bluntly: "*Democracy, that today means communism*. Any other democracy can exist only in the heads of theoretical visionaries." The party of the "Terror" of 1793 in France had found its support in the "rebellious proletariat."<sup>25</sup> "The industrial proletariat of the cities has become the crown of all modern democracy," Engels stated elsewhere; "the petty bourgeoisie and even more the peasants depend completely on its initiative." The small peasants were the least capable of taking the "revolutionary initiative."<sup>26</sup> Marx and Engels apparently felt sure that the communists would fill the gap.

The "Principles of Communism," Engels's draft of a communist "catechism" or "confession of faith" whose basic points thereafter reappeared in the *Manifesto* of 1848, touched up or toned down and supplemented by Marx, stated that the revolution would result in a "*democratic constitution*." That meant the "direct or indirect political rule of the proletariat." The rule would be indirect in France and Germany, where the proletariat was still the minority and where the majority was made up of small peasants and petty bourgeoisie. But the last two, who were "just in the process of a transition into the proletariat" and whose "political interests depended more and more on the proletariat," soon would have to yield to the demands of the workers. "Democracy would be wholly useless for the proletariat unless it immediately serves as a means for the implementation of measures attacking private property and securing the position of the proletariat."<sup>27</sup> The *Manifesto* later was less explicit.

In the last part of the *Communist Manifesto* Marx outlined the tactics of the party in the coming revolutions. The tactics varied greatly because they were attuned to the actual conditions and possibilities, as Marx and Engels saw them, in each land. In Poland they supported the party that insisted on an "agrarian revolution" as a prerequisite for national liberation. In the United States they endorsed the position of the existing worker party toward the "agrarian reformers." In Germany they expected the triumph of a liberal bourgeois revolution that was merely "the im-

<sup>24</sup> Marx, "Der Kommunismus des 'Rheinischen Beobachters,'" *DBZ*, Sept. 12, 1847, in *Werke*, 4: 193.

<sup>25</sup> Engels, "Das Fest der Nationen in London," in *ibid.*, 2: 612-13.

<sup>26</sup> Engels, "Die Kommunisten und Karl Heinzen," *DBZ*, Oct. 3, 1847, in *ibid.*, 4: 313.

<sup>27</sup> Engels, "Grundsätze des Kommunismus," in *ibid.*, 372-73.

mediate prelude to a proletarian revolution." In general communists everywhere backed every "revolutionary movement against the existing social and political conditions."<sup>28</sup>

In the revolutions of 1848, the only revolutions in which Marx and Engels ever participated directly (they felt that their "time" had come), they avowedly were interested mainly in Germany. They therefore composed and printed in short form for easy distribution a special set of "Demands of the Communist Party in Germany." The "Demands," drafted in Paris on the eve of the return of the two men to Germany in 1848, illustrate the fact that they hoped to mobilize agrarian support for their revolutionary purposes. Four of the seventeen brief demands were distinctly agrarian. All feudal obligations that hitherto had oppressed the country population were to be abolished without compensation. All princely and feudal property in land, mines, and the like were to become state property. The state would introduce large-scale farming along scientific lines on such lands, for the benefit of everyone (*Gemeinheit*). All peasant mortgages were to be transferred to the state, to which the peasants would pay interest in the form of a tax. In areas where tenant farming prevailed, rent and other obligations were to be paid to the state, again in the guise of taxes. "Consumption" on the part of landowners who did not till the land themselves was denounced as a "pure abuse." The above measures, according to the "Demands," were aimed at reducing the "public and other burdens of the peasants and tenant farmers without hurting production and without reducing the sums needed by the state." If the "Demands" as a whole were realized, "the millions who previously had been exploited by a small number would gain their rights and the power that rightfully was theirs as the producers of all wealth."<sup>29</sup> It must be pointed out that there were no similar demands for the confiscation by the state of rental properties, mortgages, and business establishments in the cities—aside from the banks.

When Marx and Engels returned to Germany in 1848 to operate out of Cologne in their native Prussian Rhineland they were supremely confident. Yet they initially had only a few followers already in Germany. They were able to bring with them several hundred additional Germans who were living in exile. These more or less informed and enthusiastic followers were dispatched to different areas in Germany to get something started. Since the proletariat was to provide the correct initiative for the peasants and the broad democratic masses in general, Marx and Engels first tried to get control of the countless worker societies that sprang up everywhere. The attempt failed. Even in Cologne Andreas Gottschalk, supposedly a communist but one who soon showed undesirable devia-

<sup>28</sup> "Manifest der Kommunistischen Partei," in *ibid.*, 491-92.

<sup>29</sup> Marx and Engels, "Forderungen der Kommunistischen Partei in Deutschland," in *ibid.*, 5: 3-5.

tions in tactics, initially became the adored leader of the Worker Society that soon counted over five thousand members. But Marx and Engels were not discouraged. Judging from the precedents set by the great French Revolution Marx and Engels assumed that the high revolutionary tide of March 1848 would be followed by a succession of waves toward the left. Events would open the field for those who acted with decision and comprehended the forces of history.

The key necessity was a daily newspaper. Throughout Marx's and Engels's lives the control over a party "organ" remained a paramount consideration—as a vehicle through which they could reach the wider public, denounce existing governments and the halfheartedness or errors of other parties, and offer daily tactical guidelines for their followers in shifting situations. Their *Neue Rheinische Zeitung* began appearing on June 1, 1848, under the editorial "dictatorship" of Marx and with a staff composed mainly of communists. As the subtitle of the paper, "Organ der Demokratie," suggested, the *NRZ* was professedly democratic. The communist direction of the "Organ" was never specifically avowed, nor was it allowed to surface clearly in the columns.<sup>30</sup>

Beyond that, Marx was personally active in the Cologne Democratic Society. The horizons became broader when Gottschalk was arrested early in July. The Marxian influence then became pronounced in the Cologne Worker Society and in its newspaper, the *ZAV*, which appeared twice a week and on one occasion was rated an "essential power" because it circulated between 1,400 and 1,800 copies.<sup>31</sup> Two communist associates of Marx and Engels, Joseph Moll, who replaced Gottschalk as president, and Karl Schapper, whose abilities as popular leaders had been demonstrated earlier in London, thereafter dominated the proceedings of the Worker Society.

The two societies, together with another related group, sponsored a Democratic Congress in Cologne on August 13 and 14. They invited all similar societies in the Rhine Province and Westphalia to send delegates in order to coordinate policy and create the framework of a wider party. The influence of Marx was certainly in the picture when the Congress recommended a direct, oral propaganda campaign in the rural areas. Emissaries were to go to the peasants, and popular meetings were to be held. The countryside, it was argued, was more open to "democracy" than were the cities. The peasants, reacting against taxes, feudal burdens, and bureaucratic supervision, had become essentially radical.

<sup>30</sup> Engels stated in retrospect that the *NRZ* could not inscribe its "proletarian character . . . on [its] banner." Had they done so, their paper would have been a mere "small, obscure local newspaper," preaching to a "small sect" instead of to a "large active party." Since they were dealing with "despicable" opponents the tone of the paper was anything but "solemn, earnest or inspired." See Engels, "Marx und die 'Neue Rheinische Zeitung' 1848-1849," in *ibid.*, 21: 18-20.

<sup>31</sup> *Zeitung des Arbeiter-Vereines zu Köln. Freiheit, Brüderlichkeit, Arbeit* (Cologne, 1848-49).

They could be won completely for "democracy" if they saw that the "democrats" took up their cause.<sup>32</sup>

In a subsequent committee meeting of the Worker Society the subject "How is the proletariat on the land to be helped?" appeared as the first item on the agenda. The greatest evil existed among them, and it multiplied in geometric proportions.<sup>33</sup> The following session then offered solutions to the problem in terms that resembled the "Demands of the Communist Party." The *ZAV* of September 7 reported that members of the Worker Society had assumed the duty of going into the country to make contact with peasants and factory workers there and to create and maintain regular ties with societies in rural villages. The paper thereafter urged city workers to establish similar links "everywhere with their brothers on the land."<sup>34</sup> The *ZAV* then announced the first brilliant results of the new tactic. Several members had gone to Worringen, a short distance north of Cologne, to discuss political and social conditions with the rural population. They discovered that the "peasants understood very well where the shoe pinched them"; they had the "courage and power" to improve their oppressed lot. The "revolutionary strength of Germany" lay in the peasant and worker classes. If they united they would soon be freed of feudal burdens and the pressures of the usurer and capital. A society of forty members was formed in Worringen on the same day. During this period the *ZAV*, apparently in concert with the *NRZ*, also gave much space in its limited columns to reports of agrarian unrest and protest in scattered areas of Germany.

The success at Worringen was exploited to the utmost. A big popular meeting held there on September 17, 1848, allegedly attended by about ten thousand people, was often referred to as the first communist rally in Germany. Every effort had been made to get a big peasant and worker turnout. It was reported, erroneously, that even Marx appeared on the scene to promote the movement. The local officials charged that a handbill (*Zettel*) with the seventeen communist "Demands" was being distributed. Karl Schapper presided at the rally of September 17, and Friedrich Engels acted as secretary. The "blood-red flag" stood unfurled on one side; the popular German national colors of 1848-49, the black, red, and gold, on the other. The crowd unanimously acclaimed the "democratic-social red republic," and speakers from every locality were recognized, including "Henri [*sic*] Brisbane," the "recognized editor of the democratic-socialist New-York Tribune." The "greatest calm, order and harmony" characterized the affair,<sup>35</sup> a statement that was included in

<sup>32</sup> *NRZ*, Sept. 13, 1848. Curiously, the report was brief and a month late. It was printed only after questions were raised regarding this silence on the part of the "Organ of Democracy."

<sup>33</sup> "Comité-Sitzung vom 21. August 1848," *ZAV*, Aug. 27, 1848.

<sup>34</sup> *Ibid.*, Aug. 31, Sept. 21, 1848.

<sup>35</sup> *Ibid.*, Sept. 21, Oct. 5, 1848.

many reports of popular meetings. But several police spies were detected and unceremoniously pitched into the Rhine, then mercifully pulled out before they drowned.

At the time of the Worringen triumph the committee of the Worker Society in Cologne proposed a second great worker-peasant rally to be held at "Wesslingen" on September 30. The selection of Wesslingen offered a special fillip because the worker emissaries had organized a society there in the face of well-publicized, hostile interference from local officials. Many people, especially in the countryside, wanted to hear the "human doctrine of democracy and socialism," Moll observed. Even if the army opposed them the soldiers (mostly peasant lads) had a "human heart and a social mind." The workers should appeal to them also. Moll, however, warned the committee that the war still had to be restricted to the "mouth and pen."<sup>36</sup> Notices in the *NRZ* and the *ZAV* in the first weeks of September also advertised the appearance of a new radical newspaper in Cologne, the *New Cologne Journal for Townsmen, Peasants and Soldiers*, edited by Fritz Beust and Fritz Anneke. The paper reproduced the tone set by the *NRZ*, though on a more plebian scale.<sup>37</sup>

Moll's warning regarding premature action reflected fully the tactics of Marx himself. An uprising in Cologne and in the Rhineland, unless preceded or accompanied by major upheavals elsewhere, was suicidal and would deprive the movement of its base of operations on the Rhine. Yet, in anticipation of decisive news from Berlin, where a major ministerial and revolutionary crisis was expected, Cologne constructed barricades during the night of September 25-26. They were left standing without defenders when the wires from Berlin did not carry encouraging news.<sup>38</sup> The Prussian authorities nevertheless imposed martial law on Cologne for several days—the "rule of the saber." This involved a temporary suspension of the *NRZ*, *ZAV*, and several other newspapers. A second Democratic Congress in Cologne, scheduled to meet when the crisis came, never held any formal sessions. The popular rally that was to have taken place in Wesslingen about a week later was also canceled. The program in the countryside was interrupted.

During the same weeks of September Marx likewise had expected a revolutionary involvement of the peasantry when the populace in Frankfurt a.M. turned to rioting and the barricades. This had occurred when

<sup>36</sup> "Comité-Sitzung vom 21. September 1848," *ZAV*, Oct. 5, 1848. See also *ZAV*, Sept. 19; *NRZ*, Sept. 12. Even with the help of German scholars in the Rhineland I have been unable to locate "Wesslingen," which apparently lies to the south of Cologne. It has been suggested that "Wesseling" was meant, though the spelling "error," repeated several times, appears a little gross and unusual for the *ZAV*. Marx's *NRZ* (Sept. 12) called the locality "Wesselingen" instead of "Wesslingen."

<sup>37</sup> The German title was *Neue Kölnische Zeitung. Für Bürger, Bauern und Soldaten*. In the last number of the *NRZ*, May 19, 1849, Marx in a sense designated the *NKZ* as the heir of the *NRZ*. The former, accordingly, soon adopted the subtitle of the *NRZ*, "Organ der Demokratie."

<sup>38</sup> See Marx's account, "Die 'Kölnische Revolution,'" *NRZ*, Oct. 13, 1848.

the Frankfurt Parliament reluctantly sanctioned the Armistice of Malmö, which seemed to represent a Prussian betrayal of ardent German national claims to Schleswig-Holstein in favor of Denmark. When the conservative Prussian delegate, Prince Felix von Licknowsky, was torn to pieces by the "enraged mass" in Frankfurt, the *NRZ* gave credit to the peasants who had hurried to the city to do their part for this *Lynch-justiz*, as a "respectable expression of the popular will."<sup>39</sup>

The *NRZ* undoubtedly lapsed into some wishful reporting regarding the actual or destined role of the peasantry. News of peasant revolts (as had occurred early in 1848), in any event, might provide the signal for action among peasants who were already restive. The "honor of Germany" has been defended by workers and peasants from the area, the paper reported. Peasants from countless localities were hurrying to the aid of the barricade fighters in Frankfurt. The revolutionary mood in the Odenwald, Nassau, and Electoral Hesse would block the arrival of additional troops. If the revolt held its own for only a day, the entire surrounding countryside would be in arms; the soldiers would be too weak to suppress the movement. And who would swear by the peasants in the Rhineland, the *NRZ* asked rhetorically; the peasants easily could block the movement of troops (probably Prussian) on the Rhine.<sup>40</sup>

After reports from Frankfurt confirmed the failure of the insurrection the *NRZ* predicted that the revolt was not crushed. The "raging peasants" would not simply put down their arms. The storm that had gathered could be diverted against six to eight princely residences and hundreds of manors. The peasant war that had flared briefly earlier in 1848 was far from being finished.<sup>41</sup> There were only scattered and elusive reports to confirm this expectation.

The revolutionary insurrection in Vienna shortly thereafter produced similar "specters" of peasant involvement. The *NRZ* apparently assumed that the Austrian peasants, freed from *Robot* earlier in the revolution, would rush to defend their gains, lest they be rescinded if Vienna fell. So the paper reported that the country people were streaming in from all directions, even from distant Styria and Tyrol. Peasants from Upper Austria gave assurances that the countryside was ready to respond to a call from Vienna. At a late point in the game 156 Tyrolese sharpshooters, all "intrepid mountaineers," reportedly fought their way into the city to aid the besieged.<sup>42</sup> But whatever peasants arrived, or failed to appear because they did not get the "call," the Croat army of General Jellačić and the mixed units of Prince Windischgrätz successfully invested and stormed Vienna.

<sup>39</sup> "Beilage," *ibid.*, Sept. 13, 1848. In subsequent days the paper offered a somewhat different account of the event.

<sup>40</sup> *Ibid.*, Sept. 21, 1848.

<sup>41</sup> *NRZ*, Sept. 21, 1848.

<sup>42</sup> *Ibid.*, Oct. 12, 14; "Zweite Ausgabe," *ibid.*, Oct. 29, 1848.

During 1848 Marx also repeatedly turned to the question of the liberation of Prussian peasants from feudal burdens and prerogatives. This took the form of denunciations of the machinations of reactionary forces as well as of the liberal elements in the Prussian constituent assembly for failing to act decisively in dealing with agrarian questions.<sup>43</sup> When the Prussian king finally dissolved the assembly and granted his own constitution on December 5, 1848, Marx announced that the action of the king did not free the peasant without compensation. It was the duty of democratic and "rustic" societies to make this clear to the peasants in the Rhineland and Westphalia. If that were done soon, the counterrevolution would face a "phalanx."<sup>44</sup>

AS THE REACTION gained ground in 1848-49 without provoking a corresponding intensification of the revolutionary spirit, Marx repeatedly looked to Paris for another "crowing of the Gallic cock" to herald the dawn of a new and more radical round of revolutions. In this connection Marx expected the peasants in France, that vast majority which had rejected the revolution in the April elections of 1848 and again in the selection of Louis Napoleon as president of the Second Republic, to enter the picture in a positive sense. Marx's imagination was engaged by the revolutionary possibilities of the "billion" (the *Milliarde*, somewhat less than a billion francs) that had been paid in 1825 to the émigrés of the great French Revolution to compensate them for their lost properties. Since a billion was an almost astronomical figure in that age, a demand for the "return of the billion" was sufficiently lofty to become the rallying cry of a new revolution.

After first noting that the Parisian masses had demanded the repayment of the billion (plus three per cent interest since 1825) in the spring months of 1848, the *NRZ* commented favorably on the "precious tactic" adopted by the "Mountain," the leftist opposition to Louis Napoleon, when it sought to gain a peasant following by taking up the same cry. The call for the "repayment of the billion" represented a step on the tail of the "reactionary snake."<sup>45</sup>

On March 11 Marx wrote a leading article on "Die Milliarde" and its revolutionary impact on the peasants. Petitions demanding a repayment of the "billion," plus the interest, were drafted nearly everywhere in France and would soon adorn the walls in all communes, according to Marx. The subject had become the "flesh and blood" of the peasants, as they realized now that their formal ownership of land made them vassals of the capitalist. The "billion" was the first revolutionary measure

<sup>43</sup> *NRZ*, June 25, July 30, Sept. 14, 1848, in *Werke*, 5: 106-07, 309-14, 402.

<sup>44</sup> *NRZ*, Dec. 17, 1848.

<sup>45</sup> *Ibid.*, Feb. 14, 23, Mar. 6, 1849.

that "hurled the peasants into the revolution." With this "dowry" the "democrats" would win the coming elections.<sup>46</sup>

After recognizing the supposed revolutionary effect of the "billion" on the French peasants, Marx began the search for a "Prussian billion." To keep the figures within reasonably credible limits he always converted the Prussian *Thaler* (worth about four francs) into francs. Prussia also had her "robber barons" who now put on a "bourgeois" coat and were joining the "Society to Protect the Property of All Classes." Even the free landowning peasants in the Rhine Province were being threatened with a new scheme of taxation under which they would pay four or five times as much as would be collected in the eastern provinces.<sup>47</sup>

This was followed by another article, "Also a Billion," that concentrated on the Prussian province of Silesia, where the feudal nobility were then requesting compensation for the rights and benefits they had enjoyed hitherto at the expense of the agrarian population. The "little man" was to be robbed again! But the mood of the peasants had changed, as was evident in thousands of rural villages where peasants were starting to figure up how much they had been robbed during the preceding thirty years when the abolition of serfdom during the Prussian reform era had not been carried out conclusively. The *NRZ*, not surprisingly, asserted that a "billion" (francs, not *Thaler*) was in the picture. The next revolution would bring the "practical realization" of this sum.<sup>48</sup> Compensation to rectify the inequitable tax burden of many years was also involved. The peasants knew enough about "natural history" to know how to "bleed the bloodsuckers."<sup>49</sup>

A series of articles on the "Silesian Billion" (*Die schlesische Milliarde*), by Wilhelm Wolff (*Lupus*), apparently with some editorial emendations by Marx, appeared in the following issues. Wolff, the son of a Silesian serf, was in a position to add a few vivid details from his own personal recollections.<sup>50</sup> He gave a detailed account of the various forms of "robbery" perpetrated against the agrarian population, together with an estimate of the sums involved. The total exceeded a "billion." Other "billions" apparently could be certified also, because the situation in Silesia was typical of conditions everywhere in Prussia (except in the Rhine Province) and in Germany at large. The peasants had the right to demand a return of the "billions." But this would not happen under the existing governments. Only complete destruction of the established system would help. Mean-

<sup>46</sup> Marx, "Die Milliarde," *NRZ*, Mar. 16, 1849, in *Werke*, 6: 353-56.

<sup>47</sup> "Die Preussische Milliarde," *NRZ*, Mar. 17, 1849.

<sup>48</sup> "Auch eine Milliarde," *ibid.*, Mar. 22, 1849.

<sup>49</sup> *NRZ*, Mar. 24, 1849.

<sup>50</sup> Wolff, a member of the recently dissolved Communist League, was on the editorial staff of the *NRZ*. Energetic and fearless, Wolff was an excellent disciple with enough intelligence to grasp the essential views and tactics of Marx without confusing the issue with mental reservations. Marx ultimately dedicated the first volume of *Kapital* to this resolute follower.



while the *NRZ* did not lose sight of the French "billion" and its expected revolutionary implications.<sup>51</sup>

The agitation involving the "billions" ultimately did not lead to marked revolutionary revival, either in France or in the German world. Reprints of the "Silesian Billion" apparently were distributed among the peasants, and the *NRZ* had the satisfaction of reporting that a Count Renard (who had been mentioned as one of the beneficiaries of the "robbery") indignantly informed the lower house of the Prussian Parliament that the "tale of the Silesian billion has affected the rural population of Silesia in an unhealthy manner."<sup>52</sup>

Simultaneously the *NRZ* noted that a petition (which would amass seventy thousand signatures) circulated by the agricultural proletariat in Mecklenburg was asking for a "safe and free existence." Hunger and bitterness were intense; guns and scythes were available in no small numbers. Although Prussian troops had entered the country the Junkers were in danger. A report from Silesia spoke of the formation of a society of farm workers. The report advised the "social-democrats" to intercede "in an active and inciting manner."<sup>53</sup>

The next, and last, revolutionary storm in 1849, however, did not open with an agrarian overture. The storm resulted from an outburst of German national frustration produced by Frederick William IV's rejection of the imperial crown of a liberal, united Germany offered by the popular Frankfurt Parliament. If agrarian distress and the lure of the "billion" played a role, it remained subsidiary. The May 1849 crisis led to the expulsion of Marx from Prussian soil. He left Germany behind within the next weeks and went to Paris, still hoping that an expected eruption in France would reverse the triumph of reaction and the return to order. As late as August 1849, just before he went to London, Marx still wrote hopefully of the anger that prevailed among French peasants.<sup>54</sup>

If Marx and Engels frequently groaned over the "stupidity" of the French as well as other peasants (the "barbarian race"), they never dismissed the peasants and the agrarian question as a significant factor in any revolutionary equation wherever the "barbarians" represented the majority. After the failures and disappointments of 1848-49, Engels wrote about the "Peasant War in Germany" in 1852, which reminded the Germans that they also had a revolutionary tradition.<sup>55</sup> Marx published "The Class Struggles in France," generally regarded as a classic example of the application of his theory of history to recent revolutionary events. Such critical examinations also served as guidelines for the future. Marx

<sup>51</sup> *NRZ*, Mar. 27, 29, Apr. 5, 12, 13, 14, 17, 22, 24, 25, 1849.

<sup>52</sup> "From Berlin, April 19," *ibid.*, Apr. 22, 1849.

<sup>53</sup> "Aus Mecklenburg," "Aus Schlesien," *ibid.*, Mar. 28, 1849.

<sup>54</sup> Marx to Engels, Aug. 17, 1849, in *Briefwechsel*, 1: 111-13.

<sup>55</sup> Engels, "Der deutsche Bauernkrieg," *Revue*, in *Werke*, 7: 329.

asserted that the French proletariat could not have moved forward until the mass of the nation, "the peasants and the petty bourgeoisie," rebelled against the "rule of capital," and, out of necessity, turned to the proletariat as their "advance guard," or leaders (*Vorkämpfer*).<sup>56</sup> France had approached that point in 1849 when a "significant part of the peasants and the provinces was revolutionized" in response to the promise of the "red party" ("the coalesced democratic party") that offered them the repayment of the "billion," the regulation of mortgages, and the abolition of usury. Only the fall of capitalism and the creation of an anticapitalist, a proletarian, government—"the social-democratic, the red republic, that is the dictatorship of [the peasant's] ally"—could end the "peasant's misery and social degradation."<sup>57</sup> Engels followed a similar line whenever he touched on the revolutionary potential of the German peasant and the agrarian question generally in his "German Imperial Constitutional Campaign" and the series of journalistic articles on the German revolution that appeared under Marx's name in an American newspaper.<sup>58</sup>

The tactical guidelines issued by the revived and again secret Communist League spoke of the nearness of another revolution and restated the need for ties with the agrarian population. The "Address of the Central Authority to the League, March, 1850" stressed the tactical exploitation of the supposed hopes and grievances of the agrarian proletariat, the farm workers. The communists would join forces with the petty-bourgeois democratic party that was destined to make the next revolution. That party was very powerful because it included most of the bourgeois city dwellers and counted on the help of the peasants as well as the "land proletariat, so long as the latter has not yet found a support among the independent proletariat of the cities." The revolution would lead to the creation of a democratic republic in which the democrats would exercise power "for a moment." They would try to call a halt to

<sup>56</sup> Marx, "Die Klassenkämpfe in Frankreich 1848 bis 1850," in *ibid.*, 21. In a later appraisal of French developments Marx again asserted that peasant and proletarian had one thing in common: both were dehumanized and exploited by the same capitalists. Because of their peculiar circumstances, however, the peasants represented no more than the "simple addition of equal quantities," like a "sack of potatoes." They were mere "troglodytes." "They therefore find their natural allies and leaders in the *city proletariat*, whose aim is the overthrow of the bourgeois order." "Der achtzehnte Brumaire des Louis Bonaparte," in *ibid.*, 8: 198-202.

<sup>57</sup> Marx, "Klassenkämpfe," in *ibid.*, 7: 83-84. Late in life Engels reaffirmed the correctness of Marx's theoretical and tactical position. If history proved they were wrong their miscalculation lay in their failure to see that the economic developments in 1848-49 were not yet ripe for the elimination of capitalist production. The latter had still possessed a capacity for vast expansion. Quite in line with their earlier views, Engels expressed the conviction that the German Social Democrats (the big "Marxian" party) would soon win the support of the greater part of the "middle stratum of society"—"the petty bourgeoisie as well as the peasants." See Engels, "Einleitung zu 'Die Klassenkämpfe in Frankreich 1848 bis 1850' (Ausgabe 1895)," in *ibid.*, 516-17, 526.

<sup>58</sup> Engels, "Die deutsche Reichsverfassungskampagne," *Revue*, in *ibid.*, 111-97. The newspaper articles appeared in the New York *Herald Tribune* between October 25, 1851, and October 23, 1852, and were published separately later under the title *Germany: Revolution and Counter Revolution*, originally under Marx's name.

the revolution, but the communists had to insist on making the "revolution permanent"; the communists had to promote added turmoils designed to force the democrats to carry out their "present terroristic phrases." The communists were to take a direct hand in perpetrating "excesses"—such as a "popular revenge against hated individuals and public buildings."

The first direct clash between workers and bourgeois democrats would involve the question of the abolition of feudalism.

As in the first French Revolution, the petty bourgeoisie will want to give the feudal lands to the peasants in the form of free property; that means preserving the land proletariat and the creation of a petty-bourgeois peasant class that will travel the same road toward impoverishment and indebtedness to which the French peasant is still committed.

This the workers had to prevent, in line with their own interests and with those of the land proletariat. Feudal lands were to become state property, to be converted into "agricultural colonies" cultivated by the "associated land proletariat."<sup>59</sup> If the emphasis in this case was on the land proletariat, the ensuing directive focused more on the peasant.

In a report on the situation in Germany, "The Address of the Central Authority to the League of June, 1850" noted the fact that league members had gained a direct influence over some peasant and farm worker societies, in some cases getting them completely in their hands. Similar societies elsewhere ("Saxon, Franconian, Hessian and in Nassau") were also mainly under the guidance of the league. The "Address" then stressed the value of gaining influence everywhere over worker, gymnastic (*Turnverein*), peasant, farm worker, and other organizations. The report directed league members to organize persons who were useful and reliable in a revolutionary sense but who had not yet grasped the "communist consequences of the present movement" as "second class" league members, to be led by the real *Bund* members. In this way they could exert a strong influence, especially over peasant and gymnastic societies.<sup>60</sup> In speculating on the chances of revolution following another economic crisis Marx wrote in 1856 that the "whole thing in Germany" depended on the possibility of backing the proletarian revolution "by some second edition of the Peasant's war." Then the matter would be superb.<sup>61</sup>

Marx and Engels thereafter never lost sight of the fact that the plight of the agrarian population might serve as the basis of an alliance between

<sup>59</sup> Marx and Engels, "Ansprache der Zentralbehörde an den Bund vom März 1850," in *Werke*, 7: 244-54.

<sup>60</sup> Marx and Engels, "Ansprache der Zentralbehörde an den Bund vom Juni 1850," in *ibid.*, 306-12.

<sup>61</sup> Marx to Engels, Apr. 16, 1856, in *Briefwechsel*, 2: 131-32. The quoted phrases were written thus in English. After he learned English Marx frequently used English words, phrases, and occasionally sentences in letters otherwise written in his native German.

the proletariat and the peasant. If less was written on the subject it was because times were relatively unpropitious in an immediate revolutionary sense. During such periods Marx and Engels concentrated more on theory and the preparation of "thick books" as their only "resource."<sup>62</sup>

Interest in tactical ties between the proletariat and the agrarian population nevertheless persisted to the end. Engels, with Marx's encouragement, published new editions of his "German Peasant War" that appeared in 1870 and 1875. In a new introduction Engels stated that the workers in Germany still fell far short of a majority and therefore needed allies. He dismissed the petty bourgeoisie as highly unreliable and denounced the *Lumpenproletariat* as being utterly impossible. The peasant, agrarian masses, in contrast, offered the best hope. Tenant farmers (*Pächter*) could expect salvation only from workers in the form of lower rents. As for the small peasant, he could find relief from the burden of the mortgage and the usurer solely from the proletariat. The most numerous and natural ally of the proletariat was the agricultural day laborer who existed in vast numbers in all of north and east Germany where large estates prevailed. The most urgent task of German workers was to pull these agrarian classes into the "movement." Engels then called attention to the decisions of the Basel Congress of the International Workingmen's Association (IWA) in 1869. It had decreed that the soil was to become "collective national property." So as not to alarm the small peasants, perhaps, Engels explained that this formula was designed primarily for areas where large landownership predominated, as it did in much of Germany and England.<sup>63</sup>

Marx privately informed Engels that the decision of the Basel Congress had been inspired by the General Council of the IWA in London, a body in which Marx's influence was pronounced. The council had suggested the creation of a "Land and Labor League," whereby the "worker party" would sever all ties with the bourgeoisie. Engels replied that the decision had "worked wonders."

<sup>62</sup> The expressions were used by Engels. See Engels to Marx, Feb. 13, 1851, in *ibid.*, 1: 150. Marx's concentration on the "thick books" of *Kapital* in the 1850s and 1860s illustrates the point. Since such "critiques" emphasized theory and historical analysis *Kapital* said little on current tactics, or even tactics in general. It speaks of modern industry annihilating the independent peasant and replacing him with wage labor, thereby engendering the same class antagonisms and desires for social change found in the cities. The farm laborer was similarly enslaved, exploited, and impoverished but possessed a lesser capacity for resistance because of his dispersed situation. Capitalism thereby prepared the way for the "higher synthesis of the future," a union of "agriculture and industry." (*Capital. A Critique of Political Economy* [Modern Library ed.; New York, n.d.] 554-55, 654.) Marx noted that rural violence in England, with its "swing riots," came to the surface almost concurrently with the widely noted uprising in Lyons early in the 1830s (pp. 653-54, 742). *Kapital* touches on such diverse items as the operations of the *corvée* in the Danubian principalities (p. 260), the thirteen-to-fourteen-hour day among Scotland's agricultural workers (p. 278), the status of agricultural "gangs" (pp. 435-36), the degradation of the peasant into a "serf" (p. 745), and the "swamp of pauperism" in which one foot of the farm worker was stuck (p. 705).

<sup>63</sup> Engels, "Vorbemerkung zu 'Der deutsche Bauernkrieg' (Ausg. 1870 u. 1875)," in *Werke*, 7: 535-37, 542.

People forget that, aside from the large estates, there are various types of peasants: (1) the tenant farmer who does not care whether the soil belongs to the state or to the large property owner; (2) the private owner—first, the large peasant against whose reactionary existence it is possible to incite the day laborers and hired hands; second, the medium peasant who will also be reactionary and who is not very numerous; and third, the debt-ridden small peasant who can be reached through the mortgage. Moreover, one can say that the proletariat for the time being is not interested in questioning the right of small landownership.<sup>64</sup>

With this approach Marx and Engels apparently felt that the support of the majority of the rural population could be enlisted even in relatively normal times. They always believed that most could be accomplished within the context of an economic crisis or when the nation was subjected to extraordinary strains, as in time of war.

Late in Engels's life, when many socialist or social-democratic parties with a "Marxian tendency" suddenly and everywhere turned to the countryside, Engels again stated his conviction that "in order to conquer political power, the party first must go from the city to the land [that the party] has to become a power in rural areas."<sup>65</sup> In line with the Marxian view Engels declared that the "development of capitalism irredeemably destroys the landownership of the small peasant."<sup>66</sup> It was the duty of the party to persuade the peasant of this fact. To reassure the peasants Engels stated, "We can only promise that we will not forcibly interfere with the property relationship against their will." Everything "permissible" would be done to make the transition to an "association" (*Genossenschaft*) easier for the peasant. It would not be desirable to wait until "capitalist developments everywhere arrived at their final consequences, until the last craftsman and the last small peasant became the victim of capitalistic exploitation on a large scale."<sup>67</sup> In contrast to the outlook for the small peasants it was possible to offer the land proletariat prospects as bright as those that "beckoned to the industrial worker." Opportunities along this line were excellent in the eastern Prussian areas (*ostelbische*). That was the "decisive field of battle"; a victory there would mean the end of *Junker* rule. Looking at Europe as a whole Engels declared, "From Ireland to Sicily, from Andalusia to Russia and Bulgaria the peasant is a very substantial factor of the population, of production and of political power."<sup>68</sup>

<sup>64</sup> Marx to Engels, Oct. 30, Engels to Marx, Nov. 1, 1869, in *Briefwechsel*, 4: 232, 233.

<sup>65</sup> Engels, "Die Bauernfrage in Frankreich und Deutschland," in *Werke*, 22: 485-86. This article, originally published in two numbers of *Die Neue Zeit*, 1 (1894-95), was translated into Russian in 1903 by Lenin.

<sup>66</sup> "Brief an die Redaktion des 'Vorwärts,'" Nov. 12, 1894, in *Werke*, 22: 480.

<sup>67</sup> "Bauernfrage," 501. See also Engels, "Zur Kritik des sozialdemokratischen Programmentwurfs, 1891," in *ibid.*, 24, where Engels stated that the petty bourgeoisie and the small peasants had no choice but to stand by their "exploiters" or to join the ranks of the workers.

<sup>68</sup> "Bauernfrage," 501.

With reference to Italy Marx early pointed to the agrarian population as a significant force to be enrolled in the battle of national liberation from Austrian rule—an important consideration because Austria, as a bastion of the old order, opposed the course of revolution everywhere. He denounced Mazzini's policy as being fundamentally wrong because it neglected the peasants, for many centuries the "oppressed portion of Italy." The country population there was as "systematically enervated and stupified" as that of Ireland.<sup>69</sup> It took courage to declare that the first step toward Italian independence demanded the "complete emancipation of the peasants and the conversion of their métayer system [*Halbpachtssysteme*] into free bourgeois property."<sup>70</sup> Years later, after Italy was independent and already had a Marxian party, Engels advised the Italian Marxist Filippo Turati that the "Socialist party" in Italy was too young and weak to gain an "immediate victory." It had to turn to the petty bourgeoisie, a class that faced ruin and that would supply the fighters and leaders for a revolutionary movement. The peasantry would follow them, as "strong and indispensable allies." It was the duty of the Socialists to regard "every revolutionary or progressive movement as a step further in the attainment of their own end." "The victory of the revolutionary movement . . . cannot but strengthen us and place us under more favorable conditions."<sup>71</sup>

<sup>69</sup> The Marx-Engels position in the Irish question requires a special treatise. In general, Marx, through the IWA and otherwise, supported the cause of Irish emancipation as a vital step toward the proletarian triumph of the English worker himself. The English "agricultural oligarchy" as long as it held its "entrenched outpost" in Ireland remained unassailable in England proper. It was easier to attack in Ireland because the issue there was not merely economic but also "national." (Marx to Ludwig Kugelmann, Mar. 28, 1870, in *Letters to Kugelmann* [New York, 1934], 107.) Since the competition of cheap Irish labor in England also depressed the status of the English worker and created divisions in the ranks of labor, the liberation of Ireland would help to unite the English workers against their real foes. (Marx to Meyer and Vogt, Apr. 9, 1870, in *Correspondence*, 288-90.) Engels, after a trip to Ireland, however, found that the Irish became corruptible as soon as they ceased to be peasants and joined the bourgeoisie—this was the case with most peasant nations, but it was especially true of the Irish. The literary representatives of the Irish peasants were thoroughly bourgeois. As a result the Irish regarded the worker movement as "sheer heresy." The Irish peasant was not permitted to know that the "socialist worker" was his "only ally in Europe." (Engels to Marx, Sept. 27, Dec. 4, 1869, in *Briefwechsel*, 4: 228, 254.)

<sup>70</sup> Marx to Engels, Sept. 13, 1851, in *Briefwechsel*, 1: 259-60; Marx to Joseph Weydemeyer, Sept. 11, 1851, in *Correspondence*, 45. The editorial note in the latter (p. 46) incorrectly speaks of "Georg" Weydemeyer.

<sup>71</sup> Engels to Turati, Jan. 26, 1894, in *Correspondence*, 520-22. Marx in the IWA similarly pointed out that workers in different lands had not reached the same degree of development. Hence the "real movement" also expressed itself in differing "theoretical forms" until it ultimately arrived at a "common theoretical program." (Marx to Engels, Mar. 5, 1869, in *Briefwechsel*, 4: 164.) In speaking of different "theoretical forms" here, did Marx set a precedent for later followers who presented temporary tactical programs as theoretically binding? Engels, with reference to party tactics in America, stated that it was "far more important that the movement should spread" than to "start and proceed from the beginning on theoretically correct lines." Their theory was not a "dogma but the exposition of a process of evolution, and the process involves successive phases." (Engels to Florence Kelley Wischniewsky, Dec. 28, 1886, in *Correspondence*, 453.)

THE ATTITUDE OF Marx and Engels toward the Eastern, largely agrarian areas of Europe demands brief but special consideration. Since revolutions in general acted as the "locomotives of history" Marx and Engels also early identified themselves with the cause of agrarian discontent in the overwhelmingly agricultural lands, notably in Poland but gradually in Russia, too. When a struggle for national liberation at the cost of "reactionary" states seemed to call for tapping the revolutionary potential in agrarian discontent, Marx and Engels advocated the cause of agrarian democracy. The liberation of Poland, aside from its direct and local revolutionary benefits, was a necessity also for Germany and the West. The creation of a free Poland would push the frontiers of Russia eastward, eliminating her reactionary pressure on Germany and her capacity to intervene against revolutions everywhere. In 1848-49 and on other occasions Marx and Engels regarded a war against Russia to liberate Poland a revolutionary necessity for Germany. Such a gigantic undertaking would increase domestic strains and would reveal the incompetence and treachery of existing governments and dynasties, thereby hastening a revolutionary drift to the left. A war of such colossal proportions, as Marx and Engels repeatedly stated, could only be conducted and won along revolutionary lines.

The big agricultural lands between the Baltic and the Black Seas, according to Engels, could achieve independence and escape "patriarchal-feudal barbarism" only through an agrarian revolution that transformed the peasants into free landowners. The fight for the national liberation of Poland coincided with the "struggle for *agrarian democracy*." "The merit of the Poles lay in the fact that they first recognized and announced that the liberation of all Slavic nations could be achieved only through agrarian democracy."<sup>72</sup> Aside from identifying themselves with the cause of Polish agrarian democracy Marx and Engels never indicated what specific role the communists, if any were present, would play in the liberation of such nations via "agrarian democracy." But one can assume that they would not have remained mere spectators. Marx and Engels always insisted that the peasants lacked the ability to assume the revolutionary initiative and would have to look to the proletariat, or possibly the petty bourgeoisie, once Polish peasants gained the freedom French peasants enjoyed, a freedom that subjected them to exploitation by the usurer and by capital in general. In any event, revolutions were likely to progress on an "ascending scale," as the great French Revolution had demonstrated.

Engels developed some doubts regarding the revolutionary utility of the cause of Poland during the next years. The more he studied history, the more it became clear to him that the Poles were a "finished nation," to be used as a "means" only so long as Russia was not projected

<sup>72</sup> "Die Polendebatte in Frankfurt," *NRZ*, Aug. 31, Sept. 3, 1848, in *Werke*, 5: 333, 357.

into an "agrarian revolution." From that moment on, Poland had "absolutely no reasons to exist any longer." Marx, however, displayed a wider perspective. The study of history caused him to come out "decisively direct" in favor of Poland. It was a historic fact, Marx wrote, that the "intensity and the capacity to survive of all revolutions" could be measured quite accurately by their attitude toward Poland. Poland was their "external thermometer."<sup>73</sup> He and Engels, accordingly, greeted the Polish uprising of 1863 with subdued optimism, as an event that might have repercussions in Russia proper.<sup>74</sup>

Marx and Engels, according to the *Briefwechsel* and other sources, did not neglect the revolutionary possibilities, agrarian and otherwise, in Russia. In expectation of a new revolution in Germany in 1851 Engels stated that the overall European picture would remain bleak unless a peasant revolution occurred in Russia.<sup>75</sup> By 1858 Marx found comfort in the assumption that in Russia "*the revolution has started*"; the Crimean War had hastened this turn of events.<sup>76</sup> In the next year the "movement" continued to progress more in Russia than in all of Europe. The nobility was turning against the tsar; peasants were opposed to the nobility; the Poles refused to be Russian. In the next revolution Russia would "obligingly revolutionize" in concert with the rest of Europe. Alexander II had "spoiled" his chances with the peasants, with the result that the "social" movement was being inaugurated both in the East and the West.<sup>77</sup>

Marx thereafter experienced a somewhat ironic pleasure in knowing that the early Russian translation of his *Kapital* enjoyed a wider sale than the original German edition had. A variety of considerations in the 1870s caused Marx to concentrate increasingly on studies of the Russian language and Russian affairs, to the point where he professed a familiarity with many of the native sources. So in 1876 when Marx, as he so often did, asked his partner to assume a major burden, Engels complained somewhat whimsically, "You can lie in a warm bed, study Russian agrarian questions in particular and ground-rent in general and nothing interrupts you."<sup>78</sup> Russia's experience with temporary military reverses at the hands of the Turks in 1877 was seen by Marx as a "new turning point" in

<sup>73</sup> Engels to Marx, May 23, 1851; Marx to Engels, Dec. 2, 1856, in *Briefwechsel*, 1: 157, 204-08. Marx and Engels made a point of participating in most Polish happenings—such as anniversaries of the 1830 and 1846 (Cracow) revolts as well as Polish rallies in general. This served as a common ground on which various democratic, radical, liberal, and even conservative elements could meet.

<sup>74</sup> Marx to Engels, Feb. 13, Engels to Marx, Feb. 17, 1863, in *ibid.*, 3: 126-27, 128-29.

<sup>75</sup> Engels to Marx, May 23, 1851, in *ibid.*, 2: 204-05.

<sup>76</sup> Marx to Engels, Oct. 8, 1858, in *ibid.*, 2: 341-43. Marx, with perhaps the French Revolutionary parallel in mind, stated that he regarded the assembling of the "Notables" in St. Petersburg as signaling the start. On the Continent revolution was "imminent" and would assume at once a socialist character.

<sup>77</sup> Marx to Engels, Dec. 13, 1859, Jan. 11, 1860, in *ibid.*, 2: 448-49, 452-53.

<sup>78</sup> Engels to Marx, May 28, 1876, in *ibid.*, 4: 436-38.



European history. Russia was on the eve of an "upheaval." "All sections of Russian society are in complete disintegration economically, morally and intellectually," Marx asserted. "This time the revolution will begin in the East, hitherto the unbroken bulwark and reserve army of counter-revolution."<sup>79</sup>

In view of the revolutionary prospects in Russia Marx appeared reluctant to discourage certain otherwise sympathetic Russian ideologists who hoped that the Russians might find a different "path of development," growing out of a transformation of the "village commune" into a more advanced form and thereby escaping the tortures of the customary capitalist developments. Marx here declared "straight to the point" that his "historical sketch of the genesis of capitalism in Western Europe" need not have universal application. He cited an example from Roman history to illustrate the point. But he then concluded that, if Russia proceeded along existing capitalist lines, she would "experience its pitiless laws like other profane peoples."<sup>80</sup> As in parts of the *Manifesto*, which also was professedly outspoken, Marx appears to evade the essential question. In a preface to the Russian edition of the *Communist Manifesto* of 1882 he added, "If the Russian revolution becomes the signal for the workers' revolution in the West, so that the one supplements the other, then the present form of land-ownership in Russia may be the starting point of a historical development."<sup>81</sup> The statement is guarded with an "if" and a "may."

After Marx's death Engels was a little more explicit. He was pleased to find that there was a "party among the youth of Russia that frankly and without ambiguity accepts the great economic and historic theories of Marx." While professing a limited knowledge of Russian affairs Engels stated that the revolution "may break out there any day." The "historic theory of Marx" was the "fundamental condition of all *reasoned* and consistent revolutionary tactics; to discover these tactics one has only to apply the theory to the economic and political conditions of the country in question." Russia was a "charged mine" that needed only a "fuse to be laid to it." It did not matter how the revolution started or who lit the fuse. In a country "where all these contradictions are violently held together by an unexampled despotism . . . there, when 1789 has once

<sup>79</sup> Marx to Sorge, Sept. 27, 1877, in *Correspondence*, 348-49.

<sup>80</sup> Marx to the editor of the *Otycestvenniye Zapiski*, end of 1877, in *ibid.*, 552-55.

<sup>81</sup> Quoted in *Correspondence*, 355n.; see also *Werke*, 4: 576. A few years later, in taking note of other special developments in Russia, Marx wrote that the introduction of railroads in countries with limited capitalist developments "accelerated the social and political disintegration," by the creation of a "capitalistic superstructure in dimensions altogether disproportionate to the bulk of the social body, carrying on the great work of production in traditional modes." Marx to Danielson, Apr. 10, 1879, in *Correspondence*, 359. As noted earlier in other connections, Marx here again asserts that the pangs of the Industrial Revolution were experienced more acutely in the less advanced states.

been launched, 1793 will not be long in following."<sup>82</sup> The agrarian question was one of the sources of such contradictions.

In 1893 Engels returned to the question of whether the "Russian commune might not be the starting point of a new social development." Had the West been able to upset the "capitalistic régime some ten or twenty years ago," he asserted, "there might have been time yet for Russia to cut short the tendency of her own evolution toward capitalism." As it was, Russia, "being the *last* country seized upon by the capitalist *grande industrie*, and at the same time the country with by far the *largest peasant population*," she experienced the acute "*bouleversement* caused by this economic change." Meanwhile the commune was fading away. In a later letter to Danielson, Engels added,

No more in Russia than anywhere else would it have been possible to develop a higher social form out of primitive agrarian communism unless—that higher form was *already in existence* in another country, so as to serve as a model. That higher form being, wherever it is historically possible, the necessary consequence of the capitalistic form of production and of the social dualistic antagonism created by it, it could not be developed directly out of the agrarian commune, unless in imitation of an example already in existence somewhere else.<sup>83</sup>

That may have been the more explicit expression of Marx's own position.

THE LIVES AND DEEDS of Marx and Engels indicate that they were not unduly hobbled by the apparent implications of their own theories that stressed the revolutionary role of the advanced industrial states. Marx and Engels were generally ready to side with any groups against an existing government. In predominantly agricultural countries this meant an appeal to the rural masses. As champions of democracy, that broad, popular aspiration that sought salvation in the rule of the people, Marx and Engels were confident that the communists who represented the proletariat and comprehended the laws of historical development would capture the initiative in any such democratic front. That possibly was enhanced by the characteristics with which Marx and Engels endowed their followers—energy, decisiveness, a readiness to make tactical adjustments to the requirements of the hour, and a certain "communist pride of infallibility" (*Kommunistenstolz der Unfehlbarkeit*) that Marx once ascribed to the communists.<sup>84</sup>

The question of whether Lenin operated along truly Marxian lines in 1917 cannot be answered solely on the basis of an appeal to theory.

<sup>82</sup> Engels to Vera Zasulich, Apr. 23, 1885, in *Correspondence*, 436-38.

<sup>83</sup> Engels to Danielson, Feb. 24, Oct. 17, 1893, in *ibid.*, 508-10, 515.

<sup>84</sup> See Marx to Engels, Aug. 25, 1851, in *Briefwechsel*, 1: 244-51.

One must take into account the actions, tactics, and expectations of Marx and Engels in reasonably comparable circumstances. If Lenin appealed to the broad rural masses he certainly did not noticeably deviate from the tactics repeatedly advocated by the master.

As for the position of Russia in 1917, Was the latter so much more retarded industrially than the Germany of 1848 where Marx and Engels had expected an early communist triumph? Was Russia not also affected by the special miseries that afflicted a backward country because of the competition of the industrially advanced states and the belated arrival of capitalist developments? Did the Russians not experience the suffering, confusion, demoralization, and decline of confidence in the ruling classes that accompany a protracted, disappointing war—all major considerations that Marx and Engels counted on, as when they advocated war in 1848–49 or when they speculated on the revolutionary consequences of war on other occasions? Did the revolution in Russia not occur in a “more advanced stage of civilization” than was the case with earlier revolutions? The Russians perhaps also possessed “minds” with a greater capacity for “generalizing.”

It appears, then, that if he had neglected to see the opportunity to act and if he had not acted as he had—with energy, decision, and a total disregard of “boring scruples”—Lenin would have failed Marx. Perhaps the same can be said for communist leaders in other lands (like China) where the purely “theoretical” circumstances favoring a communist seizure of power were even less auspicious. Marx and Engels, of course, at no point exactly advocated the distribution of land to the peasants, as Lenin did in 1917. But in certain revolutionary situations, as when they supported national liberation movements against undesirable governments (notably in Poland but also in Ireland and to a certain extent in Italy), they advocated an “agrarian democracy” that apparently was based on the acquisition of the land by the peasants. In Germany Marx and Engels were ready to give assurances to the small peasants that the revolution would not lead to the liquidation of small private ownership. The gap that separated them from Lenin’s tactics in 1917 was not too formidable. Since Marx and Engels continually stressed the importance of getting a revolution started and the need to consolidate a revolutionary position, it might even be presumptuous to assume that, given the same historical circumstances, they would have hesitated to take the step that Lenin took. They certainly always paid tribute to the virtue of “audacity.”

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## Parliament and Society in Early Stuart England: The Legacy of Wallace Notestein

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WALLACE NOTESTEIN. *The House of Commons 1604-1610*. New Haven: Yale University Press. 1971. Pp. xiii, 598. \$18.50.

ROBERT ZALLER. *The Parliament of 1621: A Study in Constitutional Conflict*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xi, 242. \$9.00.

ROBERT E. RUIGH. *The Parliament of 1624: Politics and Foreign Policy*. (Harvard Historical Studies, vol. 87.) Cambridge, Mass.: Harvard University Press. 1971. Pp. x, 434. \$15.00.

ALMOST FIFTY YEARS have passed since the publication of *The Winning of the Initiative by the House of Commons*, the famous essay by Wallace Notestein that opened a new era in studies of parliament before the Civil War.<sup>1</sup> More than twenty years elapsed before much further progress was made, but in the last two decades the monographs have begun to appear that have made the century from 1560 to 1660 the most thoroughly studied in the history of any representative assembly. This outburst of scholarship has, of course, been fueled by interest in the English Civil War, one of whose sides coalesced around parliament. But the immediate inspiration has clearly come from the two pioneers in the field, Notestein and Sir John Neale. They and their students have ransacked archives public and private to extract the last ounce of information about nearly two dozen sessions of the Commons (thirteen under Elizabeth I and nine under James I), and their followers have delved equally deeply into the further assemblies of Charles I and the Interregnum. Both Notestein and Neale, blessed with long life, have lived to see their efforts bear manifold fruit, and now the appearance of Notestein's last work, published posthumously, suggests that it may be time to review the accomplishments of at least the Stuart half of this mountain of scholarship.<sup>2</sup>

From one perspective the importance and uniqueness of developments in the House of Commons during the early seventeenth century have

<sup>1</sup> Wallace Notestein, *The Winning of the Initiative by the House of Commons* (London, 1924).

<sup>2</sup> The chief works of the two pioneers are J. E. Neale's *The Elizabethan House of Commons* (London, 1949) and *Elizabeth and Her Parliaments* (London, 1953, 1957), and the edition by Notestein, Frances Relf, and Hartley Simpson of *Common Debates 1621* (New Haven, 1935).

never been so widely appreciated. The studies sponsored by the International Commission for the History of Representative and Parliamentary Institutions in the last few years have only served to underline the pathbreaking achievements of the few hundred parliamentarians who created the first effective government centered around an elected assembly. And yet within recent Tudor-Stuart historiography this all seems to be taken for granted and attention is focused elsewhere. A century ago S. R. Gardiner, writing the ten-volume work that has remained the standard political history of the years 1603-42, saw the conflict between king and Commons as the essential encounter that gave shape to the period.<sup>3</sup> Gardiner built his narrative around parliamentary sessions and presented the Civil War as the result of the irreconcilable claims of an arrogant monarch and his ambitious subjects. It was within this framework that Notestein wrote his essay on the winning of the initiative. But in the last twenty years or so the gentry controversy has upstaged Parliament, which now appears as no more than the instrument of much more powerful forces.

When Lawrence Stone recently summed up the causes of the English Revolution he placed his emphasis on three elements—the Crown's failure to gain an army or a bureaucracy; the relative rise of the gentry in wealth, status, education, administrative experience, group identity, "and also [in terms] of political self-confidence in the House of Commons"; and the spread of Puritanism.<sup>4</sup> Although Parliament appears low in these rankings, few contemporary Stuart historians would argue with Stone's assessment. Yet perhaps this shift only signifies that we are unable to link ostensible and underlying causes. The appearance of these three books provides a good opportunity to re-examine some of the issues and to place Notestein's legacy in the context of current studies of Stuart England.

THE CENTRAL PROBLEM is the extent to which the stated objectives of historical actors are regarded as sufficient indicators of their motives. Bernard Bailyn has recently stimulated a major re-examination of the American Revolution by suggesting that Locke really did matter to the colonists, that ideological disagreements did impel sober citizens to take up arms. Is a similar reassessment due for seventeenth-century England?

The differences between the two cases are vast, especially since the English of Stuart times had neither theory nor philosophy to guide them. There was no Locke, no Bill of Rights, not even (and this was a major problem) an accepted definition of royal prerogative or the privileges of subjects. In that sense, therefore, one can hardly speak of ideological

<sup>3</sup> S. R. Gardiner, *History of England from the Accession of James I to the Outbreak of Civil War, 1603-1642* (London, 1883-84).

<sup>4</sup> Lawrence Stone, "The English Revolution," in Robert Forster and Jack P. Greene, eds., *Preconditions of Revolution in Early Modern Europe* (Baltimore, 1970), 55-108, especially 96.

origins of the English Civil War. Nonetheless there was undeniably constant recourse to abstract notions of justice, equity, and right. Above all there was a very powerful consciousness of the supremacy of law; and in the revival of Magna Carta—however distorted and misapplied—there was an attempt to set a standard against which political acts could be measured. The question is: how much weight can one give such arguments? Were they genuinely believed, or were they merely cloaks, possibly unconscious, for the assertion of more material aims?

The three books under consideration come down quite decisively on the side of the parliamentarians' sincerity. "A moderate given neither to overstatement nor to enthusiasm," who "had a knack for drawing men along with him in a common pursuit of reason and of English liberty" is the way Notestein sums up the personality and aims of Sir Edwin Sandys, the most prominent exponent of opposition to the Crown from 1604 to 1621. Neither Zaller nor Ruigh would seem to disagree—in Zaller's eyes Sandys exercised "good sense and moderation," and to Ruigh he was one of the moderates.<sup>5</sup> Even Coke, bitter, vengeful, and belligerent though he was, emerges as a devoted adherent of "the reign of the common law," a pursuit that "redeemed an often sordid career." It is perhaps an exaggeration to regard him in 1621 as setting out "to rectify what he considered a dangerous imbalance in the constitution"—hardly a concept Coke would have understood—but it would be difficult to deny that he "always sought . . . to create a bulwark that could protect the subject and his precious legal rights from the inroads of government."<sup>6</sup>

Moderation and legalism are scarcely the terms by which one expects the forerunners of revolutionaries to be described. Rousseau, the social critics of the nineteenth century, even Locke never evoke such restrained epithets. True, the moderation was sometimes abandoned, but only over narrow issues—the punishment of Edward Floyd, who had libeled the king's daughter, occasioned by far the most angry and hysterical outburst in the Commons during James I's reign. And as one reads the debates one becomes convinced that the arguments and proposals that were aired represented exactly the concerns of these serious and determined country gentlemen. Their hearts were on their sleeves, their worries were laid bare, and yet one finds no hint of revolutionary rhetoric.

What was it, then, that stirred the M.P.'s? In the 1604–11 sessions, according to Notestein, there were three basic issues. In descending order of importance they were: those royal prerogatives, such as wardship and purveyance, which were sources of revenue for the king; the union with Scotland; and the regulation of foreign trade. In 1621, as Zaller

<sup>5</sup> Notestein, *Commons 1604–1610*, 7; Zaller, *Parliament of 1621*, 48; Ruigh, *Parliament of 1624*, 244.

<sup>6</sup> Zaller, *Parliament of 1621*, 53, 52.

points out, the problems shifted: to the conduct of foreign policy and of royal ministers, to religion, and to the trade depression. Finally, in 1624, Ruigh sees foreign policy and, once again, ministerial behavior as the only matters of major concern, though it could be argued that this is to miss the considerable significance of the Monopolies Act, which, remarkably enough, is not once mentioned in the book.

The problems that the Jacobean Parliaments struggled with thus centered on the royal government's activities—its financial devices, its foreign policy, the actions of its servants. Religion and trade were recurrent and important, but clearly subsidiary issues. And the standards used for judgment were always custom and law. As Notestein observes, "those who would maintain the rights of Parliament found it prudent to go back and recall the fourteenth century."<sup>7</sup> Although it is true that a conscientious interpretation of the precedents would only have reinforced the king's rights, in one vital regard these gentry were right. For what they wanted was a return to that system of centrifugal authority which, in previous ages, had sustained their power and kept them immune from the interference of a distant central government. That invulnerability was fast crumbling in the face of exactions like wardship and purveyance, of Privy Council interventionism, and of the spreading tentacles of royal courts. Behind the attacks on specific grievances always lay the fundamental worry that the government was becoming too powerful, that it was moving into areas outside its proper sphere, that individual rights and property were threatened, and that the servants of the Crown were acting in haughty indifference to tradition.

The only defense, so it seemed, was an independent parliament, and thus the M.P.'s fought for their privileges with unyielding determination. It was only because they got their way on perhaps the most fundamental right of all, freedom of speech, that the 1624 session became, in the phrase Ruigh quotes, a "*Parlementum Foelix*." This is why the battles over seeming trivia were in fact so crucial to the Commons. If they failed, who would protect the ordinary property owner? Certainly not the courts—they had proved all too fragile. The privileges and independence of Parliament thus came to be synonymous with the independence of all Englishmen in their localities against the encroachments of royal power. Of the three authors, Notestein, the master, perceives this connection best, and he emphasizes the development of new procedures such as the Committee of the Whole House and the changing role of the Speaker as the means whereby the M.P.'s fortified themselves against the Crown. Yet it was in 1621 that the decisive breakthrough came, with the revival of the power of impeachment. Zaller rightly stresses the importance of the proceedings against Francis Bacon, but he does not seem to appreciate

<sup>7</sup> Notestein, *Commons 1604-1610*, 391.

fully that now the Commons grasped a weapon more dangerous than the one used hitherto, the withholding of taxes. This doubling of their armory changed the parliamentarians' relationship with the Crown decisively, and both Cranfield and Charles I were later to regret bitterly their encouragement of the Commons in 1621 and 1624 for immediate ends.<sup>8</sup> Ruigh sees that the winning of the right to discuss foreign policy was a vital advance for the subject, but he, too, fails to set the events of the Parliament of 1624 into their long-term context.

It is at this point that one must return to the basic question: what did these lengthy and often tedious debates have to do with revolution? Few historians study the first half of the seventeenth century without looking ahead to the 1640s; and yet here, in the institution that became the arena for civil war, the future is played down almost into nonexistence. Only when the Puritans come on the scene—usually in support of ambitious but abortive schemes for reform—does one get a sense of a real threat to the established order. Nobody but a Puritan could have made so ominous and enthusiastic a demand as that contained in a manuscript quoted by Notestein, in which M. P.'s were asked to be ready "in behalf of the freedom of the gospel to hazard their estates rather than leave their posterity to perpetual thralldom."<sup>9</sup> This is the stuff of which revolutionaries are made, but somehow the Puritans remain in uncomfortable relationship with the other members of the Commons. What emerges are two distinct sets of aims, held by different members of Parliament. Both groups resisted the king, but the more numerous and visible of the two was essentially conservative, thus creating the paradox that lies at the heart of the problem of linking parliamentary history with the Civil War.

On the one hand stand the gentry, wedded to legalism and moderation, anxious to hold back the incursions of central authority. Alongside them, and mixed in with them, are the Puritans, determined to refashion society. In all three of these books—indeed in all three of the Parliaments—the chief actors are the gentry, and the Puritans are little more than peripheral. Is this, perhaps, why so little hint of the 1640s appears? Partly, but the problem goes much deeper. For after all, a huge amount of research has been devoted in recent years to the gentry, primarily with the object of explaining why this conservative class, at the head of a deferential society, launched a revolution. Social, economic, and intellectual analysis has been devoted to just this end, and yet somehow these studies and the studies of Parliament seem to pass each other like ships in a fog—beyond an occasional booming echo, there is no attempt to offer mutual assistance. Since the social history is currently far more common it is not difficult to see how the impression arises these days that

<sup>8</sup> Zaller, *Parliament of 1621*, 24; Ruigh, *Parliament of 1624*, 343–44.

<sup>9</sup> Notestein, *Commons 1604–1610*, 41.



English historians are slighting events in Parliament when they try to explain the Civil War.<sup>10</sup>

This difficulty partakes of a much larger problem, and one that faces historians in all fields—how to connect the growing body of research into social history with the extensive long-standing findings of political history. One body of pioneers in social history, the Sixth Section of the *Ecole Pratique des Hautes Etudes* in Paris, evades the issue by ignoring political history. But in studies of seventeenth-century England the dilemma is nakedly before us. One set of historians clings to the view of the revolution as a political struggle of independent-minded or Puritan gentry versus an arbitrary or overambitious king; the other group stresses the rise of the gentry as a class that had come to terms with changing conditions in the economy, the social structure, land use, and so on, and now made its lunge for political power. The two views are not incompatible, but they have never really merged. As one surveys the field, though, and sees the superb works on both sides of this divide, one must conclude that, if political and social forces are ever to be blended, here is where historians have a golden—perhaps their best—opportunity. The research has been so saturating, the intelligence applied to the many problems so acute, and the striving for synthesis so manifest, especially in recent years, that surely the links must soon begin to appear. Moreover, as the last few paragraphs indicate, some of the links are already implicit in the literature.

ON THE SIDE of parliamentary studies the materials are now almost too abundant—a testimony to the breadth of Notestein's inspiration. The publication of these three books means that there is a volume devoted to every single Parliament of James I's reign.<sup>11</sup> The work of J. N. Ball, Conrad Russell, and Christopher Thompson will be adding to our knowledge of the 1620s in the next few years;<sup>12</sup> and the Long Parliament, already amply investigated, is enjoying a new spate of research. When the Yale Parliamentary Diary project is complete most of the primary sources for the 1620s will be published, too. Few major areas of historical research have been laid so bare, with biographies of most of the leading actors also available. The one grievous lacuna is a biography of the duke of Buckingham, an unpleasant and lifelong project that thus far nobody

<sup>10</sup> An example can be taken from the most important study of English society under the Tudors and Stuarts published in the last decade, Lawrence Stone's *The Crisis of the Aristocracy 1558-1641* (Oxford, 1965). Stone disarmingly sidesteps the problem by saying that his book is a prolegomenon to, and explanation of, political history, but that Parliament is "deliberately excluded" from consideration (p. 8).

<sup>11</sup> The only Jacobean session not covered by these books is that of 1614, which is treated in T. L. Moir, *The Addled Parliament* (Oxford, 1958). Mention should also be made of one other most important study of the early Stuart Commons: D. H. Willson, *Privy Councillors in the House of Commons 1604-1629* (Minneapolis, 1940).

<sup>12</sup> These three scholars are working on Sir John Eliot, John Pym, and the earl of Warwick and his circle; respectively.

has been willing to undertake. Its absence, though, hardly undermines the thorough understanding of the politics of the 1610s and 1620s that has been achieved.

What these three works have done is to add further concrete detail to the picture of gradual estrangement between king and Commons that has been familiar since Gardiner and especially since Notestein's classic essay of 1924. But they are rather different in approach and aim. Notestein proceeds at a leisurely pace, with the sure tread of one who is thoroughly familiar with every inch of territory that he passes. The judgments ring true again and again, the chronology is lucidly laid before us, and in two analytic sections the committee system and the role of the speaker are given definitive treatment. The one subject that is missing is the election of the House, and one could have wished for more detailed treatment of the relations between Commons and Lords. But there can be no doubt that this is a worthy *opus ultimum*—where *The Winning of the Initiative* laid the foundations, this book has constructed an imposing edifice of scholarship.

As a contrast in styles, Ruigh and Zaller could hardly be more dissimilar. Zaller leans heavily on Notestein's seven-volume edition of the 1621 diaries and has sought out few other primary sources. His book is short, spare, and sparkling. He has a fine ear for a phrase, and his assessments of men and situations are quick and shrewd. It is a pleasure to read the book, though one often wonders whether a great deal more does not still need to be discovered. Naturally, too, one has occasional misgivings about the flashes of interpretation—Coke, for example, is perhaps overstressed, and the broader implications of Bacon's fall receive inadequate attention. Yet certainly in style, in the willingness to make general comments about Stuart politics, Zaller upholds Notestein's tradition.

Ruigh's is a much more weighty tome, and sometimes rather heavy going. But there can be little need for further work here. He has examined an enormous range of papers, and in his best section—on elections and the patronage of various groups—he has brought some extremely difficult and scattered research to a most successful conclusion. These three chapters are models of painstaking historical work, and there is nothing like them in the literature on Parliament. In the very close study of day-to-day diplomatic history and its reflections in the Commons, though, one sometimes loses the wood for the trees. This book will be a mine for future scholars, and Notestein would doubtless have been proud of it, but he, too, would probably have wished that the sights could have been raised higher than the spring of 1624 more often.

Nobody can dare to say that research is ever complete, but it seems safe to predict that studies of particular sessions of the House of Commons during James I's reign are unlikely to appear again for at least a generation. Related subjects need further work, to be sure—notably

the House of Lords, which has received adequate attention only from Zaller. But surely now the task is to begin integrating the huge accumulation of political history with the multiplying studies of English society. Did the issues raised in Parliament reflect profound social divisions? Was there a fundamental change in direction between James I's and Charles I's reign or merely an intensification of problems already apparent by the 1620s? Were the non-Puritans revolutionary or reactionary? Were economic issues of any significance? Did England really experience a revolution at mid-century? If we can begin to answer these and similar questions, Notestein's legacy will gain new dimensions and a new importance for all historical research.

HAVING PAID HOMAGE to a distinguished historian whose work is now complete, and whose bequest to the future requires serious attention, I feel I cannot end this essay without responding, in Notestein's name as far as I can, to a questioning of his contribution that appeared in print not many months after he died, too late for him to defend himself. In the course of a discussion of the purposes of parliamentary history from the Middle Ages to the present, Geoffrey Elton has expressed a series of doubts about Notestein's scholarship and interests,<sup>13</sup> and conscience drives me to respond.

Elton's first stricture is an unsubstantiated charge that the edition of the 1621 diaries lacks scholarly rigor and is not easy to use. Yet these volumes, meant for the scholar doing detailed work, not for the casual reader, have proved their usefulness for over thirty years. One cannot dismiss the standards or the comprehensibility of the edition, its cross-references, explanations, and superb index, without extensive documentation.

The second stricture is more general. According to Elton the title (and presumably the contents) of Notestein's famous essay "urge us to seek nothing but the 'growth' of the Commons' independence," a "whiggish" view that has been discredited by the Commons' lack of initiative since the mid-nineteenth century. In fact the essay discusses much besides growing independence; furthermore, whether Whiggish or not, Notestein's interpretation of Stuart times is hardly vitiated by developments since Victoria's reign. And what better organizing principle is there for a history of the Commons under James I and Charles I?

Elton broadens his criticism by asking whether "all that matters" is the ambition of "representatives" to limit the "executive," an emphasis he attributes to "American scholars, who perhaps cannot be expected to know better." But Notestein had no such exclusivist view; he did not

<sup>13</sup> "Studying the History of Parliament," *The British Studies Monitor*, 2 (1971): 4-14. Since it is such a short article I have not given references for each quotation. The arguments I discuss can be found on pages 7-11. I might note that the remaining eight paragraphs of my article were added after the rest had been accepted for publication.

treat the struggle between king and Commons in such anachronistic terms; and his analysis of parliamentary history does not demonstrably owe more to American birth than to bicycle riding.<sup>14</sup>

Elton's third stricture is the obverse of the second. To correct the balance, he says, one must ask "the administrative historian's questions." Harmony, not conflict, was the normal political situation; therefore the main emphasis should be placed on procedural stability. Studies of the early Stuarts err in their conflict-inspired "conviction that only opposition entitles a man to respect," and Notestein is specifically reproved for his "almost perverse" emphasis on 1621, when no legislation was passed except for two subsidy bills. Elton is looking for "more original scholars," such as his students, to investigate procedure and give us "real hopes of finding out the truth."<sup>15</sup> After all, he complains, "we do not even know for sure how committees were appointed—by whom, at whose nomination" in the seventeenth century.

Despite his admission that "administrative" and "political" should not be separate categories, Elton fails to recognize that Notestein's work is notable precisely because he interwove administration and politics. Indeed he regarded changes in procedure (for instance, the new use of committees) as *the* essential mechanism whereby political initiative was won. *Pace* Elton, specialists do know how committees were appointed, and the best discussion of the subject is included in the forty-page chapter on committees in Notestein's last book. As for one-sidedness, that same work accords nobody more respect than Cecil or Bacon.

Although Notestein's particular blend of procedure and politics might not satisfy Elton, it is misguided to deny that the blend is there. Notestein perceived that procedures, continuity, and harmony are the necessary backdrop—but only the backdrop—against which change should be discerned. Elton's contrasting opinion, implicit in his belief that a Parliament's significance is related to the number of bills it passed, rests on a remarkable view of history. Most historians would surely say, with good reason, that the importance of a session derives not from the quantity of its formal bills but from the content of its bills or proceedings. In 1621 the great business was the revival of impeachment and the ruination of Bacon, neither of which produced a bill. But impeachment and the lack of bills dealt grievous blows to the harmony of government. Elton might argue that there was still much harmony—so there was,

<sup>14</sup> Elton himself pays tribute to the work of an American, Stanford Lehmberg, and one wonders how Elton can seriously believe that just because he lives in England he is likely to have a better understanding of distant parliaments than a Notestein or a McIlwain.

<sup>15</sup> True to his views, Elton regards the discussion of procedure "the most remarkable part of" Neale's work, and Elton heralds Elizabeth Foster's study of the clerk Henry Elsyng as part of the beginnings of great illumination. I am not sure Professor Foster would regard her admirable biography in quite that light, but in any case Elton does not appreciate that she is a student of Notestein, or that the inspiration of her teacher is apparent throughout her research.

and Notestein remarks upon its persistence into 1640 and 1641—<sup>16</sup> but to concentrate on that to the exclusion (or minimization) of conflict and change is to miss the dynamism that gives history its appeal and distinguishes it from other studies. To alter that situation Elton must first alter human nature.

In sum, Notestein's aims and achievements have served as straw men for an ill-founded polemic. He may be open to criticism, but certainly not to these criticisms. Elton could claim that the book on 1604-10 was printed too late to affect his article. Yet most of my comments could be derived from *The Winning of the Initiative*, and in any case the book not only was published more than three months before Elton's article appeared, but was announced the previous year. The least a scholar should be able to expect is that his colleagues not attack his life's work with unseemly haste once he is no longer alive to respond. In this particular case the critic might also have given the reader some inkling as to why most historians of the period would place Notestein among the five or six scholars who have done most during the last half century to shape our understanding of early Stuart political history.

<sup>16</sup> *Initiative*, 25. Paradoxically Elton published an essay in 1970 in which he suggested that the Civil War was perhaps not so "exceptional," because conflict was a part of English life. See "The Unexplained Revolution," *Encounter*, July 1970, pp. 77-81, especially 81.

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## The Transition from Feudalism to Capitalism in Russia: A Major Soviet Historical Controversy

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A Review Article by SAMUEL H. BARON

*Perekhod ot feodalizma k kapitalizmu v Rossii: Materialy usesoiuznoi diskussii* [The Transition from Feudalism to Capitalism in Russia: Materials from the All-Union Discussion]. Akademiia Nauk SSSR, Nauchnyi soviet "zakonomernosti istoricheskogo razvitiia obshchestva i perekhoda ot odnoi sotsial'no-ekonomicheskoi formatsii k drugoi." Edited by V. I. SHUNKOV *et al.* Moscow: Izdatel'stvo "Nauka." 1969. Pp. 412.

A RANDOM SAMPLING of the views on Soviet historiography of America's Russian specialists would almost surely yield something along the following lines. Soviet historians have zealously mined masses of previously unexploited sources and have made available a wealth of factual data, but their interpretations are often so bizarre that they hardly inspire confidence. This is so partly because these writers frequently have preconceived answers to the problems they investigate, and accordingly their research is one-sided. They seek support for what they wish to prove, exaggerate the significance of such evidence as they find, and ignore or minimize the importance of evidence of a contrary kind. Such failings derive in good part from the heavily ideological character of Soviet historiography. The "classics of Marxism-Leninism" are considered as a kind of scripture in which may be found either the answers *tout court* or strong guidelines to the answers to most questions: hence the extraordinary role of quotations from the "classics" in Soviet historiography and their employment not as hypotheses to be tested but as axioms to be illustrated. The situation is made worse by the imposition of political controls, which bar from many areas the free competition of ideas that might make for self-correction and which tend instead to produce a leaden conformity.<sup>1</sup> For such reasons Soviet

<sup>1</sup> An examination of reviews of Soviet works in the *American Historical Review* and the *Slavic Review* will generally bear out this characterization—but certain important qualifications must be added. American and other Western students recognize that a number of Soviet historians produced significant contributions even in the depths of the Stalin era. In the post-Stalin period, thanks to a somewhat improved climate for historical work, the share of historical production worthy of attention has increased. Works that offer dubious interpretations, moreover, are often partly redeemed by the inclusion of important new material

historiography represents an alien "continent" whose exploration hardly repays the effort, and dialogue between Soviet and Western students of Russian history is well-nigh impossible.<sup>2</sup>

The book under review is of quite extraordinary interest, for it compels a reconsideration of this appraisal, at least in respect to one broad area of historical investigation. In question is the recently published record of a conference held in 1965 to discuss a problem that has bedeviled Soviet historiography for decades—the transition from feudalism to capitalism in Russia.<sup>3</sup> Soviet students of their country's history have long taken it for granted that Russia passed through a feudal epoch, which began as early as the ninth or tenth century and persisted until 1861, when serfdom was abolished and the capitalist era ushered in. Understandably they have considered the investigation of the genesis of capitalism "within the womb of the old order" one of their major tasks.<sup>4</sup> If something like leaden con-

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from the archives. On these matters and on other developments in post-Stalin historiography, see John Keep and Lilianna Brisby, eds., *Contemporary History in the Soviet Mirror* (New York, 1964); Kurt Marko, *Sowjethistoriker zwischen Ideologie und Wissenschaft* (Cologne, 1964); Hans Rogger, "Politics, Ideology, and History in the USSR: The Search for Coexistence," *Soviet Studies*, 16 (1965): 253-75; and Arthur P. Mendel, "Current Soviet Theory of History: New Trends or Old?" *AHR*, 72 (1966-67): 50-73. See also the journal *Kritika* (published three times a year at Cambridge, Mass.), which performs a valuable service by printing long and often penetrating reviews of Soviet historical works.

<sup>2</sup> In contrast to the prevailing view, Fernand Braudel some years ago laid on Western historians the obligation to "follow and follow closely the work of [Soviet] historians" unless they would risk ignoring "an entire 'continent' of history." See the editorial note in *Annales: Économies, Sociétés, Civilisations*, 12 (1957): 127.

<sup>3</sup> The conference, sponsored by the Academy of Sciences' Scientific Council on "the lawfulness [zakonomernost'] of the historical development of society and the transition from one socio-economic stage to another," took place in Moscow on June 2-4, 1965. It was attended by some three hundred historians from all over the USSR. Before the conference began, each received a copy of a report composed by a committee whose members were I. F. Gindin, L. V. Danilova, I. D. Koval'chenko, L. V. Milov, A. P. Novosel'tsev, N. I. Pavlenko, M. K. Rozhkova, and P. G. Ryndziunskii. Pavlenko was the committee's leading spirit and spokesman. The printed record (hereafter *Perekhod*) includes the report, the comments on it offered by thirty-eight of the conferees, a digest of written remarks submitted by persons who, for lack of time, had no opportunity to speak, and a brief set of recommendations developed out of the conference materials. The first published information about the conference was a brief report: Iu. Bromlei, "Izuchenie problemy perekhoda ot feodalizma k kapitalizmu v Rossii" (The Study of the Problem of the Transition from Feudalism to Capitalism in Russia), *Vestnik Akademii Nauk SSSR*, 35 (1965): 114-16. The following year more of the substance of the controversy was publicized through articles by representatives of the contending groups: I. A. Bulygin, E. I. Indova, A. A. Preobrazhenskii, Iu. A. Tikhonov, and S. M. Troitskii, "Nachal'nyi etap genezisa kapitalizma v Rossii" (The Opening Phase of the Genesis of Capitalism in Russia), *Voprosy Istorii*, no. 10 (1966): 65-90; N. I. Pavlenko, "Spornye voprosy genezisa kapitalizma v Rossii" (Controversial Questions on the Genesis of Capitalism in Russia), *ibid.*, no. 11 (1966): 81-102. The Soviet controversy may be compared with the debate among non-Soviet Marxists on the same problem in the history of Western Europe. See Paul Sweezy, ed., *The Transition from Feudalism to Capitalism* (London, 1959).

<sup>4</sup> Students of Russian historical writing are familiar with M. N. Pokrovskii's ill-starred attempt to prove the dominance of commercial capitalism in seventeenth-century Russia. After he and his work were denounced in the early 1930s something of a vacuum existed on the matter of the genesis of capitalism in Russia. The question figured prominently again in the protracted discussions of periodization that occurred in the years following the Second World War. The vacuum was soon filled by the conception shortly to be described. See Konstantin Shteppa, *Russian Historians and the Soviet State* (New Brunswick, 1962), 114-15, 246-75; and Leo Yaresh, "The Problem of Periodization," in C. E. Black, ed., *Rewriting Russian History* (2d ed.; New York, 1962), 42-77.

formity was ever a fair characterization of Soviet work on this theme, it certainly is no longer, for the conference proceedings spotlight a lively controversy of absorbing interest.

The conference centered on a committee report, which sets out the broad range of questions in dispute, including such basic matters as the time the process began and the manner in which it unfolded. On the first point, a few Soviet historians—for example, D. P. Makovskii and Academician S. G. Strumilin—had opted for the sixteenth century, and a large majority for the seventeenth.<sup>5</sup> The committee report rejects both these views and fixes the beginning of the genesis of capitalism in Russia in the 1760s. To the proponents of the seventeenth century the transitional period in Russia, though exhibiting some variations, appeared fundamentally similar to that in Western Europe. In the committee report similarities of a very general kind are acknowledged, but they are definitely overshadowed by profound differences that impart to the Russian historical process its distinctive character. The report canvasses not only substantive but theoretical and methodological issues as well, and indicts modes of investigation and interpretation that brought to predominance what it takes to be a lamentably erroneous conception of a highly significant slice of Russian history. The treatment of these matters makes plain that many of our misgivings about much of Soviet historiography are shared by numerous members of the Soviet historical guild.<sup>6</sup>

It will be readily appreciated that the conference report could not have been a bolt out of the blue. Over a period of time a paradigm for the transition from feudalism to capitalism in Russia had taken shape and served as the framework for research.<sup>7</sup> Beginning in the mid-1950s, however, the findings and reflections of a number of investigators brought one facet of the paradigm after another into question. The doubts voiced, sometimes implicitly and sometimes explicitly, by such historians as N. M. Druzhinin, N. L. Rubinshtein, V. K. Iatsunskii, S. D. Skazkin, and N. I. Pavlenko, apparently went unheeded.<sup>8</sup> But as additional contrary evidence

<sup>5</sup> For a generally positive review of D. P. Makovskii's book (see note 20 below), which included an approving introduction by S. G. Strumilin, see Richard Hellie "The Foundations of Russian Capitalism," *Slavic Review*, 26 (1967): 148-54.

<sup>6</sup> For example, the first pages of the committee report make many of the very same critical points on methodology and interpretation as I did in my review essay of N. M. Druzhinin *et al.*, eds., *Goroda feodal'noi Rossii* (The Towns of Feudal Russia) (Moscow, 1966); see Baron, "The Town in 'Feudal' Russia," *Slavic Review*, 28 (1969): 116-22.

<sup>7</sup> I use here T. H. Kuhn's terminology, for what has occurred in Soviet historiography on the transition from feudalism to capitalism is reminiscent of, though not identical with, the pattern Kuhn described in his book *The Structure of Scientific Revolutions* (2d ed.; Chicago, 1970).

<sup>8</sup> A very important publication implicitly critical of the paradigm was Druzhinin's report to the International Congress of Historians in Rome in 1955, entitled *Genezis kapitalizma v Rossii* (The Genesis of Capitalism in Russia) (Moscow, 1955). V. K. Iatsunskii, a more combative historian, polemicized against the prevailing view in his review of N. V. Ustiugov's highly touted work on the salt industry at Solikamsk (see note 12 below) in *Istoriia SSSR*, no. 1 (1958): 193-96; and in his article, "Osnovnye etapy genezisa kapitalizma v Rossii" (Fundamental Stages of the Genesis of Capitalism in Russia), *ibid.*, no. 5 (1958): 59-91. In a symposium in 1959 Iatsunskii, Academician S. D. Skazkin, and A. L. Shapiro constituted a panel that broadened the attack. Their papers are published in *Ezhegodnik po agrarnoi istorii vostochnoi Evropy 1959 g.* (An-



accumulated, the doubters evidently coalesced into a group prepared to do battle for their convictions. The conference in 1965 was a confrontation between opposing schools, and the initiative lay with the insurgents. The committee report they produced summarized and synthesized the evidence developed over a period of at least a decade for an alternative view of the Russian historical process. A trenchant critique of the paradigm, it not surprisingly called forth counterattacks and strenuous objections to what was dubbed its "historiographical nihilism." Although a few points were scored against them, the committee and its adherents nevertheless plainly carried the day, and subsequent developments appear to confirm their victory.<sup>9</sup> Is it fanciful to see in all this something of a revolution in Soviet historiography?

In the years just after the Second World War Soviet historians found in a statement Lenin made in 1894 a principal guideline for their research.

Only the new period of Russian history (approximately from the seventeenth century) is characterized by the actual amalgamation of all . . . the regions, lands, and principalities into one whole. This amalgamation . . . was brought about by the increasing exchange among regions, the gradually growing circulation of commodities, and the concentration of small local markets into a single, all-Russian market. Since the leaders and masters of the process were merchant capitalists, the creation of these national ties were nothing else than the creation of bourgeois ties.<sup>10</sup>

The extraordinary significance attached to the statement may be gauged by the words of two principals in the controversy, A. A. Preobrazhenskii and Iu. Tikhonov, who have claimed that it "provides the key to the

nual on the Agrarian History of Eastern Europe: 1959) (Moscow, 1961), 21-68. N. L. Rubinshtein was the first Soviet historian, to my knowledge, to call into question the interpretation put upon one of Lenin's statements, which, as will shortly appear, plays a central role in the controversy. See his "Territorial'noe razdelenie truda i razvitie vserossiiskogo rynka" (The Territorial Division of Labor and the Development of the All-Russian Market), in V. V. Al'tman, ed., *Iz istorii rabocheho klassa i revoliutsionnogo dvizheniia: Sbornik statei pamiati Akademika Anny Mikhailovny Pankratovoi* (On the History of the Working Class and the Revolutionary Movement: A Collection of Articles in Memory of Academician Anna Mikhailovna Pankratova) (Moscow, 1958), 87-100. Pavlenko's studies of the metallurgical industry in the eighteenth century turned up irrefutable evidence of the very special character of Russia's socioeconomic development; see especially his *Istoriia metallurgii v Rossii XVIII veka* (History of Metallurgy in Eighteenth-Century Russia) (Moscow, 1962).

<sup>9</sup> The recommendations that grew out of the conference materials and are a part of the printed record clearly aimed to strike a compromise, but this in no way conceals the fact that the insurgents routed their adversaries. This conclusion is confirmed by examination of the follow-up statements of the contending groups (see the two articles cited in note 3, above). The turpidity of the first and the brilliance of the second correspond roughly to the merit of the arguments advanced by each side. E. I. Zaozerskaia's recent study of Russian industry in early modern times, *U istokov krupnogo proizvodstva v russkoi promyshlennosti XVI-XVII vekov* (The Sources of Large-Scale Production in Russian Industry of the Sixteenth through Seventeenth Centuries) (Moscow, 1970), adds further support to the case advanced in the 1965 committee report. The proponents of the disenthroned outlook have not surrendered, but it seems true that something like the views advanced by the committee report now have the support of the greater number of Soviet historians concerned with this problem.

<sup>10</sup> The statement occurred in a polemic against the Populist writer N. K. Mikhailovskii in the pamphlet "Who Are the 'Friends of the People' and How They Fight against the Social Democrats?" See V. I. Lenin, *Collected Works* (Moscow, 1963-68), 1: 154-55.

understanding of a whole epoch in the history of our motherland."<sup>11</sup> Its critical importance evidently rested on three counts: first, the phrase "the new period" demarcated a major division of Russia's history; second, the lines following that phrase sketched the content of the "new period," perceived as nothing other than the genesis of capitalism; and third, the beginning of the process was fixed in the seventeenth century.

If Lenin supplied the basis of periodization, Soviet investigators derived from Marx the stages and criteria, the categories of thought, and the modes of analysis wherewith to trace the evolution from feudalism to capitalism. Armed with these concepts, students of Russia's socioeconomic history in the centuries preceding the abolition of serfdom have worked through vast masses of archival material in search of evidence to underpin them. The countless articles, hundreds of dissertations, and scores of book-length monographs that resulted invariably recited Lenin's statement of 1894 and professed to have corroborated it. For the seventeenth century, evidence was adduced for the separation of agriculture from nonagricultural production in a developing social division of labor; the gradual replacement of a natural, self-sufficient economy by a national market based on commodity production, territorial division of labor, and greatly increased significance of towns and merchants; the rise of large-scale industrial enterprises (manufactories) employing hired labor; and social differentiation in town and countryside, which bespoke the primary accumulation of capital in the hands of a nascent bourgeoisie and the divorce of significant numbers of producers from the means of production.<sup>12</sup>

<sup>11</sup> See A. A. Preobrazhenskii and Iu. A. Tikhonov, "Itogi izucheniia nachal'nogo etapa skladyvaniia vserossiiskogo rynka (XVII v.)" (The Results of Study of the First Step in the Formation of the All-Russian Market [Seventeenth Century]), *Voprosy Istorii*, no. 4 (1961): 80. This article summarizes the evidence accumulated up to that time in support of what I have called the paradigm. Judgments similar to the one quoted are frequently encountered in the literature.

<sup>12</sup> Among representative works one finds such instructive titles as: *Voprosy genezisa kapitalizma v Rossii* (Problems of the Genesis of Capitalism in Russia) (Leningrad, 1960); L. Beskrovnyi et al., eds., *K voprosu o pervonachal'nom nakoplenii v Rossii (XVII-XVIII vv.)* (On the Problem of Primary Accumulation in Russia [Seventeenth through Eighteenth Centuries]) (Moscow, 1958); A. Ts. Merzon and Iu. A. Tikhonov, *Rynok Ustiuga Velikogo v period skladyvaniia vserossiiskogo rynka (XVII vek)* (The Market at Ustiug the Great in the Period of the All-Russian Market in Formation [Seventeenth Century]) (Moscow, 1960); N. V. Ustiugov, *Solevarenniaia promyshlennost' Soli Kamskoi v XVII veke* (The Saltworks Industry in Solikamsk in the Seventeenth Century) (Moscow, 1957); N. A. Baklanova, *Torgovo-promyshlennaia deiatel'nost' Kalmykovykh vo vtoroi polovine XVII v. K istorii formirovaniia russkoi burzhuzii* (The Commercial-Industrial Activity of the Kalmykovo in the Last Half of the Seventeenth Century: On the History of the Formation of the Russian Bourgeoisie) (Moscow, 1959); A. M. Pankratova, *Formirovanie proletariata v Rossii (XVII-XVIII vv.)* (The Formation of the Proletariat in Russia [Seventeenth through Eighteenth Centuries]) (Moscow, 1963); and Druzhinin, *Goroda feodal'noi Rossii*. These titles suggest the great efforts invested in demonstrating that the seventeenth century was indeed the seedbed of Russian capitalism. All the works named, however, cannot fairly be assigned to one neat category. If some were consciously designed to illustrate particular formulas, others exhibit a decided independence of spirit. For example, in spite of the title given her book, Pankratova forthrightly declares her disagreement with those who discern capitalistic relations in seventeenth-century Russia: "There can be no talk of capitalism or a proletariat in the Marxian sense in the seventeenth or even the eighteenth centuries, except for the last two or three decades of the latter." *Formirovanie proletariata*, 9. The views she develops on Russian towns (pp. 82-85) are also markedly contrary to the ebullient portrayal of seventeenth- and eighteenth-century towns given by most contributors to Druzhinin, *Goroda feodal'noi Rossii*.

The prevailing interpretation of the seventeenth century deeply colored the treatment of what followed. If the genesis of capitalism was already well begun, then it was only logical, indeed "lawful" (*zakonomernyi*), that it should continue to develop in extent and depth.<sup>13</sup> The dominant Soviet historiography discerned the more or less smooth build-up of the indexes cited earlier, the passage of quantitative increases into qualitative changes, and the maturing of the crisis of the feudal regime. Contradictions generated between modes of production increasingly capitalistic in nature and relations of production colored by feudalism found expression in class struggle. In the first half of the nineteenth century the contradictions became ever more acute, created a revolutionary situation in the later 1850s, and ultimately made inescapable the abolition of serfdom. Works rooted in the paradigm not infrequently included qualifications that appear to cast doubt on its validity, but in the final analysis they were generally minimized or completely left out of account.<sup>14</sup> As a consequence, even though it brought masses of facts into play, the dominant rendering of this period of Russian history tended to be both highly schematic and misleading.

The committee found especially telling weapons for its assault upon the paradigm in the writings of Druzhinin and A. G. Man'kov. Man'kov, author of virtually the only postwar, book-length Soviet work on the seventeenth century that ran against the current, had called attention to "the peculiar situation that [had come] to prevail in [Soviet] historiography." Most authors, he observed, are concerned with "the search for 'new phenomena' in the economy of seventeenth-century Russia, by which they understand the rudiments of capitalist relations in the spheres of both commodity production and exchange. Meanwhile [they] take into account only to a very feeble extent the dominant feudal-serf relations, which continued to grow in strength." Such investigators paid only lip service to what was dominant while concentrating their attention on tendencies of a relatively minor order. The greater part of Soviet research on the entire transitional

<sup>13</sup> The lawfulness (*zakonomernost'*) of the historical process has of course been a central concept of Soviet historiography. I agree with the observations of Arthur P. Mendel ("Current Soviet Theory of History," 57) and Hans Rogger ("Politics, Ideology, and History in the USSR," 266-68) on the ambiguity of the concept in recent Soviet historical writing.

<sup>14</sup> To illustrate, Tikhonov wrote of the Ustiuga area: "The deepening social division of labor was conditioned by the growth of commercialization of agriculture in the seventeenth century." After this unequivocal assertion we read: "It was not possible to establish quantitatively the relationship between the value of agricultural production and the share marketed." A bit further along he adds: "However, there is no need to exaggerate the extent of development of commodity production in the feudal village. By no means all the peasant economies were closely linked to the market. Even part of the richest of the peasants [and] some townsmen were still only going over to regular sale of their products. Many middlemen did not carry on market operations systematically." Merzon and Tikhonov, *Rynok Ustiuga Velikogo*, 657-58. Despite the qualifications, the initial quotation is taken to define the situation. At the conference Iatsunskii destroyed its last shred of credibility. He had calculated the ratio of marketed to harvested grain for the individual peasant household of the region and found it to average two per cent. *Perekhod*, 267.

period, Man'kov plainly implied, represented Russian historical reality as in a distorting mirror.<sup>15</sup>

The committee underscored the need to attend not only to the old as well as the new but also to the relative weight of the one as against the other and to the effects each produced upon the other. It deplored the narrowly economic focus of much of Soviet research and the failure to give due consideration to a whole range of other forces and factors—such as the merchants and gentry, the reverse actions of the political superstructure on the economic base, and the role of foreign relations and influences—whose neglect made it impossible to perceive the Russian historical process rightly, and in its wholeness. The committee endorsed the use of the comparative-historical method as an effective way to achieve understanding of “the general and the unique in their unity . . . to reveal the *different* forms of the historical process.”<sup>16</sup>

Following some such principles Druzhinin had ten years earlier produced an interpretation of the genesis of capitalism in Russia that, with some qualification and considerable supplementation, the committee embraced. Druzhinin had emphasized not the similarity but the contrast between the Russian and West European transition from feudalism to capitalism. Capitalistic elements began to appear in Russia not after the liquidation of serfdom as in the West but while feudal-serf relations were still developing vigorously. Russia did not participate in the overseas expansion that spurred the commercial revolution in the West. Instead it long remained essentially a country of natural economy with a weakly developed commercial-industrial sector and an inconsequential bourgeoisie. In Russia primary accumulation of capital and the formation of a free labor force proceeded very slowly, and, until the second half of the eighteenth century, much of the industrial base stemmed from state initiative and relied heavily on compulsory labor. Capitalistic development in Russia proceeded simultaneously with the further development of a powerfully entrenched feudalism, which displayed a notable ability to adapt to changing needs and circumstances. Its success in assimilating elements of the new was nowhere better expressed than in the conduct of the nascent

<sup>15</sup> A. G. Man'kov, *Razvitie krepostnogo prava v Rossii vo vtoroi polovine XVII veka* (The Development of Serfdom in Russia in the Second Half of the Seventeenth Century) (Moscow, 1962), 5–6. Man'kov's book focused on the extension and strengthening of the so-called feudal-serf order in the last half of the seventeenth century. Among other things he emphasized that the town populations were reduced to a condition analogous to that of peasant-serfs (see ch. 4).

<sup>16</sup> *Perekhod*, 6; italics mine. The report located in the era of the “cult of personality” a drive to assimilate the national histories of all countries [read: Russia and the West European lands] to a single [read: Western] pattern, which had as its complement the discontinuation of comparative-historical studies. Soviet historians, the committee maintained, unfortunately tended to see Russia's transition from feudalism to capitalism as similar to the “classical” English model (p. 6). This criticism is directed against a too mechanical transposition of Marx's formulas, derived from his extended study of English history, to a radically different context.

bourgeoisie, which essentially accommodated itself to, rather than struggled against, the existing order.<sup>17</sup>

Druzhinin included the seventeenth century in the transitional period, although the general contours of his interpretation would seem to allow for only the most feeble beginnings there. Others directly contradicted the claims for the seventeenth century made above all by N. V. Ustiugov and his followers. In a much-cited article published in 1950 Ustiugov had discovered a significant movement in the seventeenth century from handicraft production for use or local order to commodity production—i.e., a strong drive toward market relations. He wrote a painstaking study of the salt industry at Solikamsk, the greatest center of seventeenth-century Russia's most important industry, and found distinctly capitalistic tendencies there.<sup>18</sup> Under his influence, many other researchers undertook to demonstrate the existence of the same commercial and industrial phenomena in different localities or lines of production. Ustiugov also edited several important volumes designed to flesh out the conception he championed, which is well rendered by the title of one of them: *The Russian State in the Seventeenth Century: New Phenomena in Socioeconomic, Political, and Cultural Life*.<sup>19</sup>

Attacks upon the claims for the seventeenth century, both earlier and in the committee report, were two-pronged. On one hand, they denied the interpretation put upon the evidence adduced; on the other, they contested the prevailing interpretation of Lenin's 1894 statement, which the evidence was supposed to corroborate. The search for the new, the critics asserted, had misled overzealous investigators to antedate the appearance of capitalistic elements or to exaggerate the significance of such

<sup>17</sup> Druzhinin, *Genezis kapitalizma v Rossii*, 7–12, 18, 34, 37.

<sup>18</sup> N. V. Ustiugov, "Remeslo i melkoe tovarnoe proizvodstvo v Russkom gosudarstve XVII v." (Handicrafts and Small-Scale Commodity Production in the Seventeenth-Century Russian State), *Istoricheskie Zapiski*, 34 (1950): 166–97; Ustiugov, *Solevarennaia promyshlennost' Soli Kamskoi v XVII veke*.

<sup>19</sup> Ustiugov, ed., *Russkoe gosudarstvo v XVII veke. Novye iavleniia v sotsial'no-ekonomicheskoi, politicheskoi, i kul'turnoi zhizni* (The Russian State in the Seventeenth Century: New Phenomena in Socioeconomic, Political, and Cultural Life) (Moscow, 1961). Ustiugov also edited the volume by Merzon and Tikhonov (see note 12 above), which purportedly confirmed beyond a doubt the case for capitalistic market relations in the seventeenth century, even as Ustiugov's study of the salt industry was to have demonstrated the capitalistic character of production. One of the two editors of *Ocherki istorii SSSR. Period feodalizma: XVII v.* (Essays in the History of the USSR. The Feudal Period: The Seventeenth Century) (Moscow, 1955), the volume on the seventeenth century in the most important multivolume general work of the last decades, Ustiugov also contributed more heavily to the volume's sections on the economy than any other writer. He plainly exerted a wider influence, but his two most conspicuous disciples were and are Preobrazhenskii and Tikhonov. In 1961 the two produced an article (see note 11 above) summarizing the evidence thus far discovered in favor of the seventeenth century as the period when capitalism began to develop in Russia. Preobrazhenskii contributed an appreciation of his master to the posthumous *Festschrift* to Ustiugov, fittingly entitled *The Towns of Feudal Russia*. Oddly enough Ustiugov was scarcely mentioned, much less directly attacked, in the 1965 committee report, although it is plain that he was one, if not the, major architect of the conception under fire. Perhaps this is to be explained by reluctance to assail a recently deceased (1963) and personally respected and well-liked comrade. Contributors to the discussion frequently cited Ustiugov's interpretations, however—sometimes approvingly, sometimes skeptically.

as actually did arise.<sup>20</sup> The larger industrial enterprises of the seventeenth century were few in number and frequently financed by foreign capital. In any case they could not be characterized as capitalistic manufactories because they involved no significant change in technology or in the organization of production. Market relations in the seventeenth century were no more capitalistic. To be sure, commodity production increased, but production for sale occurred to a limited extent everywhere while feudalism prevailed. The same was true of economic inequality among the peasants and the employment of hired labor, evidence of which was erroneously cited as proof of the advent of capitalistic features in the seventeenth century. Least of all was anything resembling early capitalist relationships to be found in agriculture, the most important branch of the economy.

For the critics, the Solikamsk salt industry and the trade activity of the White Sea region (*Pomor'e*), so important to their opponents' case, actually possessed only a local and transitory rather than a national and transforming significance. Special local circumstances accounted for the development of the salt industry and trade activity, and in the eighteenth century the one and the other were eclipsed.<sup>21</sup> Similarly almost none of the great merchant families that arose in the seventeenth century survived into the eighteenth.<sup>22</sup> If some rudiments of capitalistic relations appeared in

<sup>20</sup> The predating of the advent of capitalistic relations in Russia was a principal object of criticism in the papers by Skazkin, Iatsunskii, and Shapiro delivered to the conference on East European agrarian history in 1959. The extreme to which this tendency might be pushed is illustrated in the work of Makovskii, who proposed in all seriousness that in the sixteenth century "the trade turnover in the markets of [no] country in Europe could have been greater than Russia's." One of the participants in the 1965 conference, M. T. Beliauskii, remarked ironically that someone might take it into his head to fix the beginning of Russian capitalism's genesis in the Bronze Age. See *Exhegodnik po agrarnoi istorii vostochnoi Evropy 1959 g.*, 21, 26-28, 32-33, 48-49, 52-54; D. P. Makovskii, *Razvitie tovarno-denezhnykh otnoshenii v sel'skom khoziaistve russkogo gosudarstva v XVI veke* (The Development of Commodity-Money Relations in the Agriculture of the Seventeenth-Century Russian State) (Smolensk, 1963), 77; *Perekhod*, 308.

<sup>21</sup> A good many of these arguments had already been adduced in the critical writings referred to in note 8 above. The committee report stresses the multifariousness of the historical process in Russia, the large regional variations, which made it hazardous to draw generalizations from the developments disclosed in one area. *Perekhod*, 5, 112. Zaozerskaia's recent book further spikes the claims made for the salt industry and shows that other industries in seventeenth-century Russia were more backward. Not only did technology in the salt industry remain stagnant for centuries, but in the seventeenth century the industry's entrepreneurs persistently strove to convert the workers they hired into a compulsory labor force. See Zaozerskaia, *U istokov krupnogo proizvodstva v russkoi promyshlennosti XVI-XVII vekov*, 64-66, 98, 446-50; she directly contradicts Ustiugov's claims for the salt industry on pp. 49, 151, 175-78, 185.

<sup>22</sup> This very important point emerged initially in S. V. Bakhrushin's work and was confirmed by the researches of K. V. Bazilevich, N. A. Baklanova, and V. A. Aleksandrov. See Bakhrushin's articles, "Promyshlennye predpriiatiia russkikh torgovykh liudei v XVII v." (The Industrial Enterprises of the Seventeenth-Century Merchants) and "Torgi gostia Nikitina v Sibiri i Kitae" (The Commercial Enterprises of Gost' Nikitin in Siberia and China), in his *Nauchnye trudy* (Moscow, 1952-59): vols. 2, 3; pt. 1; K. V. Bazilevich, "Krupnoe torgovoe predpriatie (Bosovykh) v Moskovskom gosudarstve v pervoi polovine XVII veka" (A Great Trading Enterprise [the Bosovs'] in the Moscow State in the First Half of the Seventeenth Century), *Izvestiia Akademii Nauk SSSR, Otdelenie obshchestvennykh nauk*, ser. 7, no. 4 (1932): 783-811; and V. A. Aleksandrov, "Sibirskie torgovye liudi Ushakovy" (The Siberian Merchants—the Ushakovs), in Ustiugov, *Russkoe gosudarstvo v XVII veke*, 131-50. Baklanova's study of the Kalmykovs (see note 12 above) reveals that they suffered a like fate. No Soviet writer has attempted to deter-

seventeenth-century Russia, the committee report concluded, they were sporadic in nature, unstable, and either could not survive in or were deformed by the feudal-serf milieu, whose evolution was then the decisive factor in the country's development.

In 1958 and 1959 Rubinshtein, Academician Skazkin, and others had taken issue with the prevalent interpretation of Lenin's 1894 statement. Skazkin gently hinted at the imprudence of treating as holy writ a brief remark Lenin had made in the heat of a polemic against the populist N. K. Mikhailovskii. Less persuasively, he suggested that when Lenin referred to "the creation of bourgeois ties" he had in mind something quite different from what his interpreters supposed. Neither he nor anyone else pointed out that in 1894 Lenin was a mere twenty-four years of age, and no matter how great his genius he could have possessed little knowledge of seventeenth-century socioeconomic history, if only because the study of the subject had scarcely begun.<sup>23</sup> Rubinshtein and the authors of the report took a different but effective line: they used the authority of the more mature Lenin, the author of *The Development of Capitalism in Russia* and subsequent works, to refute the interpretation widely imputed to Lenin's 1894 statement. Lenin had been intensely aware of the powerful influence of feudal-serf relations on the course of Russian history, and he knew the great difficulties capitalism had in making its way even as late as the first half of the nineteenth century. His more fully elaborated views were hardly consistent with the exaggerated claims Soviet historians made for the seventeenth century, and they also stressed the very special qualities that sharply distinguished Russia's evolution from that of Western Europe.<sup>24</sup>

Among the peculiarities of the Russian historical process the committee stressed uneven development and what others have called combined development. Unevenness refers both to Russia's lag behind the West and the differential rates and modes of socioeconomic change in different parts of the vast Russian realm; combined development to the blending of old forms with new in distinctive arrangements, particularly as a result of borrowing that was calculated to overcome the lag. Thus in the first half of the eighteenth century military exigency impelled the state to import Western technology as a means of promoting large-scale industrial development—but that development was essentially noncapitalistic in type. Large

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mine systematically the survival rate of the big commercial operators. An investigation I have made, which is to be published in *California Slavic Studies*, reveals that in the seventeenth century only one great merchant family in four managed to perpetuate its status longer than one generation and almost none longer than two.

<sup>23</sup> The committee report and Pavlenko's follow-up article in 1966 both embraced Skazkin's view of Lenin's statement, but A. M. Sakharov, one of the conferees, justifiably cast doubt on this tortured interpretation. Pavlenko remarked in his article that little was known of seventeenth-century socioeconomic history when Lenin made his pronouncement. See *Perekhod*, 25, 190; Pavlenko, "Spornye voprosy genezisa kapitalizma v Rossii," 82, 88.

<sup>24</sup> Rubinshtein, "Territorial'noe razdelenie truda i razvitie vs Rossiiskogo rynka," 89-90; *Perekhod*, 38-40, 52, *passim*.

enterprises most untypical of a feudal economy arose, but they were bent to the norms of the feudal-serf order. The working force consisted in the main of compulsory-serf rather than free-hired labor. A goodly share of the enterprises came under the control of the landowning gentry, and merchant-industrialists, the nearest thing Russia had to a bourgeoisie, wanted nothing more than the right to exploit serf labor and to attain gentry status. The feudal-serf order demonstrated its resiliency, its ability to promote the development of productive forces without changing the basic social arrangements.<sup>25</sup>

The committee report presents persuasive evidence for a beginning of genuinely capitalistic relationships in a sector of Russian industry in the 1760s. The characterization of the next hundred years, which culminated in the emancipation of the serfs, however, is equivocal and less compelling. To its credit the committee scotches some dubious claims advanced by certain Soviet historians: that the peasant revolt led by Pugachev "objectively" aimed at the establishment of a bourgeois order and that Russia went through an industrial revolution in the decades preceding the abolition of serfdom.<sup>26</sup> While rejecting viewpoints that plainly derive from ideology rather than evidence, the committee report itself is not entirely free from the same fault. It characterizes the century in question as the period in which the march of capitalism became an irreversible process, when its advance progressively eroded and undermined the feudal-serf order until that system entered upon its final crisis. Evidence for such an interpretation is not wanting, but this view ultimately fails, for the "contradictions" were of course resolved through a reform from above rather than a revolution from below. Though not articulated into a whole, the elements of an alternative—and more convincing—interpretation are to be found in the report. More than once it suggests that even in the first half of the nine-

<sup>25</sup> The image of eighteenth-century socioeconomic development depicted in the committee report derives in considerable part from Pavlenko's studies. G. V. Plekhanov applied conceptions approximating to the theory of uneven and combined development to Russia to a limited extent, but it was particularly Trotsky who in 1906 developed this approach in an arresting manner. Soviet historians consider Lenin as the progenitor of the theory, but irrespective of its source the impulse of Soviet historians to utilize this illuminating approach is not the least interesting recent development in Soviet historiography. In the West, Alexander Gerschenkron has most fruitfully analyzed Russia's economic development in some such terms. The paradoxical view advanced at the 1965 conference that Peter the Great's consolidation of "feudal-serf" relations gave momentum to Russian economic development was anticipated by Gerschenkron. See S. H. Baron, *Plekhanov, the Father of Russian Marxism* (Stanford, 1963), 113-14, 355-57; Isaac Deutscher, *The Prophet Armed: Trotsky, 1879-1921* (New York, 1954), 148-54; and Alexander Gerschenkron, *Economic Backwardness in Historical Perspective* (New York, 1962); for the point on Peter the Great, see pp. 18, 135, 153-54.

<sup>26</sup> The committee report also argued that the Cossacks were the key element in the "peasant wars" (*Perekhod*, 32-33)—a position hotly disputed by a number of conferees. Earlier writers who had espoused the position the committee now affirmed had been sharply criticized in *Ocherki istorii SSSR. Period feodalizma: XVII v.*, 9-13. The argument advanced for a pre-emancipation industrial revolution, notably by Academician Strumilin, was, in the opinion of the committee, informed by a tendency in the 1940s "to exaggerate the technical-economic attainments of tsarist Russia." *Perekhod*, 72.



teenth century the feudal-serf order still gave some indications of vitality; it still had not exhausted its potentialities. The "crisis" might have been protracted indefinitely, we may infer, and all the more so because, as the committee acknowledges (following Lenin), the revolutionary forces in the mid-nineteenth century were insignificant.<sup>27</sup> Seen from this angle Russia's defeat in the Crimean War rather than the specter of revolution becomes the critical factor in the decision to abolish serfdom.

The comments on the report, though uneven in quality, offer fascinating insights into the workings of the Soviet historical guild. The most penetrating and impressive remarks came from the report's supporters (notably Pavlenko, A. L. Shapiro, L. V. Danilova, M. T. Beliauskii, M. Ia. Gefter, and Iatsunskii), from A. M. Sakharov, who shared the committee's critical spirit but disagreed with some of its findings, and from Iu. Iu. Kakhk, who displays notable analytical gifts. The upholders of the paradigm came off rather badly, and none worse than Preobrazhenskii. After Pavlenko and Sakharov, among others, had castigated the substitution of "citationism" for profound study of theory and method, Preobrazhenskii proceeded to defend the construction under attack with a long string of quotations from Marx-Engels and Lenin. Insisting once again that Lenin's characterization of the "new period" remained the correct basis for grasping the essence of seventeenth-century Russia, Preobrazhenskii strikingly confirmed the charges made against his group: in listing the "elements of the new" in the seventeenth century, he neglected to mention what was surely the most important of all—the definitive establishment of serfdom in the law code of 1649. Shaken by the attack, most of the defenders seemed to fall back on a more modest position: weak though the rudiments of capitalism earlier were, nevertheless they existed, accumulated, and laid the basis for the breakthrough of the 1760s, so that the preceding century or so should be included in the transitional period.<sup>28</sup>

<sup>27</sup> The net effect of the report here is to undercut the dubious claims of Academician M. V. Nechkina and others that a revolutionary situation existed in Russia in 1859–61. Contributors to the conference discussion spoke of the exaggerated emphasis given peasant disturbances in explanations of the decision to abolish serfdom, and there was evident a sense of the incongruity of speaking of a revolutionary situation in the absence of a revolutionary class. *Perekhod*, 200, 271–72, 316–17. For a recent appraisal by an American scholar of Nechkina's position, see Charles Adler, Jr., "The 'Revolutionary Situation 1859–1861': The Uses of an Historical Conception," *Canadian Slavic Studies*, 3 (1969): 383–99. The existence in the report of elements of two different explanations of the abolition of serfdom reflects some difference of opinion among members of the committee. Among these differences, according to Pavlenko, was the representation of the "crisis of the feudal-serf system"; another was "the role of the state and the influence of its policy on the socioeconomic processes of the transitional period." *Perekhod*, 109. Ryndziunskii and others also referred to differences within the committee (p. 204). In addition disagreements existed among those who generally supported the paradigm. For example, the views of Troitskii and M. Ia. Volkov on the character of the state in the seventeenth and eighteenth centuries were sharply at variance (pp. 139–40, 151–55).

<sup>28</sup> For Preobrazhenskii's remarks, see *Perekhod*, 213–22. The critics of "citationism" pointed out that great questions could not be settled by a quotation and that in any case one could find support in the "classics" for conflicting points of view (pp. 223–24, 113, 188–89). For the stance the upholders of the paradigm assumed after the conference, see Bulygin, Indova, Preobrazhenskii, Tikhonov, and Troitskii, "Nachal'nyi etap genezisa kapitalizma v Rossii." The group inadvertently made a most damning admission when it argued that Lenin had based his statement on

Like the report, the discussion was remarkable for its demonstration of self-criticism and a mutual criticism expressed without fear or favor. Reference was made to the deplorable effects of the "cult of personality" on historical work, among them efforts to prove Russian priority in all things—"including even the transformation of free people into serfs." E. M. Zhukov identified the often dogmatic rather than dialectical use made of theoretical propositions as the source of "the increased skepticism of our academic youth." Shapiro reminded his colleagues of the historian's obligation "to reconstruct an objective picture [rather than] to write what is congenial to the spirit." Historians of lesser rank rebuked several academicians—the most exalted members of the guild: Strumilin, whose imperfect understanding of the "classics" and faulty reasoning had led him to posit seriously mistaken views on major questions; M. V. Nechkina, whose brief for a revolutionary situation in the late 1850s was quietly shelved and whose objections to the report were termed "mechanistic"; and L. V. Cherepnin, for, among other things, having unjustly charged the committee with expounding Man'kov's views incorrectly.<sup>29</sup>

Certain of the substantive and methodological views expressed exhibited a refreshing openness, a readiness to entertain ideas once considered taboo. The committee acknowledged the tentativeness of some of the positions it took and pointed to many questions that demanded further investigation. Both its members and various contributors to the discussion offered helpful suggestions for attaining greater precision (quantitative studies) and called for the careful formulation of criteria for demarcating the stages of development in the transition from feudalism to capitalism. Sakharov asked his colleagues to compare Russia's historical evolution not just with that of Western Europe but with the countries of the East as well. Some of those who attacked the report implied that its authors regarded the feudal-serf order in the seventeenth and eighteenth centuries not only as progressing but—*horribile dictu!*—also as progressive. Sakharov, P. G. Ryndziunskii, and others openly declared that the feudal-serf order was indeed progressive and, moreover, essential to the nation's very existence at a certain stage of its development, and that it should be judged historically and not by twentieth-century standards. Two conferees challenged the prevailing view, evidently maintained with some differences in detail by both sides, of the character of the Russian state in the seventeenth and eighteenth centuries. In M. Ia. Volkov's opinion, it should not be likened to the European absolute monarchies for it was closer to what Marx had called "the Asiatic mode of rule."<sup>30</sup>

the seventeenth century not on Russian historical works but on pronouncements of Marx and Engels (p. 82). It follows that a statement with no historical basis whatever was uncritically accepted as the authoritative guide to the understanding of a whole epoch by almost an entire generation of Soviet historians.

<sup>29</sup> *Perekhod*, 192, 106, 185, 10, 73-74, 13, 333, 304, 381-85.

<sup>30</sup> For the remarks of Sakharov, Volkov, and also V. I. Koretskii on Russia and the Oriental countries, see *ibid.*, 192, 149-50, 399. These comments recall Plekhanov's interesting conception of

After examining the sketch of Russian history set out by the committee, the Western student is apt to exclaim: "But this interpretation is not so different from our own!" Quite so, and this convergence deserves emphasis. Still, one may be somewhat perplexed by the similarities, for the critical group of Soviet historians shows no inclination whatever to abandon Marxism-Leninism as the basis of historical research and understanding. We note, however, that the committee report and some of the commentators draw a distinction between a mechanistic, "logical" (aprioristic), citation-ridden, and therefore illegitimate employment of Marxism-Leninism and its more flexible, sensitive, dialectical use. I would translate this language into a distinction between poor and sound historical method. What may appear as an ingenuous and question-begging judgment may be tested in two ways. First, the specific methodological dicta commended by the critics command assent; among other things, they require balance in research and interpretation as against one-sidedness, the priority of evidence over formulas, a use of the comparative-historical method calculated to disclose the relationship between the general and the unique. Second, if the proof of the pudding is in the eating, then the substantive findings in the committee report and the research on which it is based testify to the enormous superiority of the newer methodology to the old.<sup>31</sup> Soviet and Western students of at least some phases of Russian history are now dealing with what is recognizably the same "continent," and, contrary to the situation some years ago, dialogue now appears entirely possible.

Though all this is heartening, a few cautionary observations are in order. To all appearances the debate over the transition from feudalism to capitalism has been permitted to run its course, with the laurels going to the victor in a free competition of ideas. The constraints upon work in some of the more sensitive areas—such as the history of the Communist party of the Soviet Union and contemporary history—have diminished little if at all. We cannot know the extent to which the demythologizing tendencies evident in the controversy considered here may spill over into other historical realms or if the "crude administrative interference" of the 1930s, to which one of the conferees referred, may reappear in regions from which it had been withdrawn.<sup>32</sup>

Russia as an Oriental or semi-Oriental despotism in the centuries under consideration. Such ideas were either angrily denounced or blithely ignored for some decades in Soviet historiography. See S. H. Baron "Plekhanov's Russia: The Impact of the West upon an 'Oriental' Society," *Journal of the History of Ideas*, 19 (1958): 388-404; and Karl Wittfogel, *Oriental Despotism* (New Haven, 1957), ch. 9.

<sup>31</sup> If my reasoning is correct, it should serve as an admonition that in dealing with Soviet historiography we not be put off by labels but are bound to look beyond to the substance of things.

<sup>32</sup> That both have occurred, however, is suggested by the recent case of A. Ia. Gurevich's *Problemy genezisa feodalizma v zapadnoi Evrope* (Problems of the Genesis of Feudalism in Western Europe). See David B. Miller's review in the *AHR*, 76 (1971): 756-57. That fresh winds are blowing in other areas of Soviet historiography has been made evident in review articles

Even on the transition from feudalism to capitalism in Russia of course differences between Soviet and Western scholars remain. To a certain extent they seem to me to be terminological rather than substantive, but they are also traceable to a lack of familiarity with other approaches—for example, the Weberian—to the problem of capitalistic development.<sup>33</sup> The distance has been reduced, in part, by greater willingness on the Soviet side to acknowledge the state's role in shaping Russia's socioeconomic history. But while the most perceptive Soviet historians have succeeded in avoiding the Scylla of "economic materialism," they have been so wary of the Charybdis of "statism" that they have still not accorded full value to state policy and action. One finds evidence enough of a better appreciation of the state now as obstructor, now as promoter of economic development, but this line of analysis seems never to be pursued systematically. If it were, Soviet historians might well find "the transition from feudalism to capitalism" a characterization incongruent with the "concrete-historical" facts of their country's past. Were such a perception to gain general acceptance, that would constitute a full-blown scientific revolution.

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that have appeared since the completion of the present essay. See John Meyendorff, "The Cambridge and Soviet Histories of the Byzantine Empire: Religious History and Theology," *Slavic Review*, 30 (1971): 619-23; and Alexander Gerschenkron, "Soviet Marxism and Absolutism," *ibid.*, 853-69.

<sup>33</sup> I have made a tentative application of certain of Weber's concepts to the Russian case in my article "The Weber Thesis and the Failure of Capitalist Development in 'Early Modern' Russia," *Jahrbücher für Geschichte Osteuropas*, 18 (1970): 321-36.

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## Reviews of Books

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### GENERAL

JULIÁN MARÍAS. *Generations: A Historical Method*. Translated by HAROLD C. RALEY. University: University of Alabama Press. 1970. Pp. xi, 220. \$7.50.

The book under consideration is one whose gist could easily be condensed into a ten-page essay. In fact the author obliges us by providing, at the end of his work, a résumé that sums up his views on the relationship between generational succession and the study of history. Throughout his monograph he focuses simultaneously on two tasks. One is to offer a theory of historical development based upon the formation and interaction of generations: units that he defines chronologically, sociologically, and, finally, with regard to their "epoch-making" capacities. He then attempts to prove that the vitalist philosophy of Ortega y Gasset, his own mentor, is the precondition for any proper understanding of historical generations. Supposedly, Ortega wrote conclusively on this subject in *El temo de nuestro tiempo* (1923).

An obvious question presents itself. Since Ortega had treated his topic in an exhaustive way, why should Marías have bothered to go over the same ground? Whatever the answer, one may be glad that he did. *Generations* is full of useful hints for conceptualizing the cycles of ideas and movements against the background of changing times. And apropos of demonstrating the value of Ortega's thought, Marías makes informative excursions into the writings of other Europeans concerned with the generation in history.

Although his treatment of Dilthey, Comte, Ranke, and other non-Spanish thinkers is sometimes grossly oversimplified, there is a sense of purpose that almost redeems the au-

thor's superficiality. Marías wishes to save his countrymen from the impulse of going, cap in hand, to the Germans and French to learn about historical methodology. Ortega, a Spaniard, through the perception of true historical contemporaneity, had made an original contribution to the field.

Mention should be made of Professor Raley's skill as a translator. His clear idiomatic language makes all of Marías's argumentative nuances accessible to American readers. Moreover, his introductory comments are admirably brief and unobtrusive.

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LEO BRAUDY. *Narrative Form in History and Fiction: Hume, Fielding, & Gibbon*. Princeton: Princeton University Press. 1970. Pp. viii, 318. \$10.00.

This remarkable monograph is about the artistic element in historical method, the structure of narrative. Braudy sees Hume, Fielding, and Gibbon as experimenting with narrative forms that not merely presented but controlled their historical materials. It was of course Fielding who through fiction could most explicitly impose an imaginative order on factual reality. But the prominence Braudy consequently accords to his novels—twice as much space as to Hume's *History* and three times as much as to Gibbon's *Decline and Fall*—should not mislead the reader into thinking that the study is concerned with "literature" rather than historiography.

Braudy finds a progressive development in each of his three writers. Emancipating themselves from traditional historiography, with its classical didacticism, they essayed different nar-

rative methods as their work went along. Hume, in his Stuart volumes, began with offering the traditional "character" explanations; he muted them in the Tudor volumes that followed, letting legal and institutional structures come to the fore; finally, in the volumes on the Middle Ages, he had recourse to interaction, contextual development, and the concept of the accretion of time. But Hume managed only to contain, not to control, his materials, presenting sometimes incompatible patterns, and not a consistent narrative voice. Fielding, concerned with private rather than public history, had more scope for redefining the historian's methods and tasks. In *Joseph Andrews* he rejected irrelevant factuality and distilled a factual sanction for authenticating history and fiction. In *Jonathan Wild* he attacked the use of providential and other transcendental explanations. His own program and practice are set out in *Tom Jones*. He searches for a structure to explain what is spontaneous and contingent, for "the balance between abstract and particular, type and context, essence and appearance." Here, as in other novels, he makes the narrator instruct the reader in the conventions employed and the states of affairs they illuminate. Fielding, Braudy remarks, teaches us how to understand reality through variety; his narrator is the model historian.

Gibbon—Fielding's admirer, who made a prediction to the effect that *Tom Jones* would outlive the Habsburg monarchy—also offered a method for understanding rather than a thematic interpretation. Sketched in the early *Essai*, pursued in the first three volumes of the *Decline and Fall*, and achieved in the last three is the ambition to make history the conscious creation of a literary object. The esthetic order gains precedence; themes like corruption matter less than literary structuring; relative and open-ended judgments encroach on causal determinants, and long-range causes yield to suggestive perspectives on time and place. Injunctions to the reader and the narrative "I" increasingly obtrude. Gibbon had no commitment to any systematic historical or psychological explanation, "only to his own narrative voice and the coherence it creates."

Braudy prefaces this study of the evolution of a new species of writing history by chapters on Clarendon and Bolingbroke. The treatment

of Bolingbroke is marred by some misconceptions, attributable to its purely textual nature. That of Clarendon is not as original or searching as the account of the three major writers by which this monograph must be judged. The novelty of Braudy's approach—particularly for historians—marks a distinct advance in historiographical study. Since that study has often suffered from biographical reductionism, the reader will note with relief that Braudy ascribes nothing about Fielding to his politics or about Gibbon to the Enlightenment. Yet some readers may feel uneasy about Braudy's textual asceticism and may spot some narrative procedures that owed less to literary experimentation than to some convention or circumstance in the writer's career. But even they are likely to be swayed by Braudy's persuasive reasoning and effective diction. As a negative tribute to the quality and finish of the work, let it be noted that the only perceptible irritant seems to have been contributed by the publisher: the supererogatory practice of putting elision marks before quotations commencing with a lower-case letter.

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ÁGNES R. VÁRKONYI. *A pozitívista történet szemlélet.* [The Positivist View of History]. (Stúdium Könyvek, Number 67.) Budapest: Gondolat Kiadó. 1970. Pp. 217. 20. F.

The study under review appeared in a series of monographs aimed at the interested layman, which deal with a variety of subjects from philosophy through the natural sciences to the fine arts and literature. A dozen, or one-sixth, of the titles published so far focus on different aspects of history: included are two works by V. Gordon Childe on the dawn of European civilization and the prehistory of European society; one by Federico Chabod on Italy between 1918 and 1948; and several contributions by noted Hungarian scholars, such as the histories of Bulgaria (Emil Niederhauser), Poland (József Perényi), and the Hungarian village (Kálmán Eperjessy).

Ágnes Várkonyi's study is a somewhat abridged version of the introductory chapter of a forthcoming major work that proposes to describe the genesis of and different trends in

Hungarian positivist historiography (p. 213). The author, who has written extensively on the social movements of seventeenth- and eighteenth-century Hungary and who is an expert on the anti-Habsburg struggle led by Francis II Rákóczi, has revealed her interest in the impact of positivism on Hungarian "bourgeois historiography" in two articles, the English versions of which appeared in *Acta Hungarica*. One of these analyzes the echo of H. T. Buckle's *History of Civilisation in England* (1963), and the other the impact of scientific thinking on Hungarian historical writing in the mid-nineteenth century (1968); along with the present booklet they anticipate the opus yet to come.

Writing from a Marxist point of view, Várkonyi's sophisticated approach to positivism is refreshingly different from the standard Soviet evaluation represented, for example, in the relevant article of the *Great Soviet Encyclopedia* (2d ed.; 1955), which still insists, with due reference to Lenin's *Materialism and Empirio-criticism* (1909), that fighting positivism is an important part of the overall struggle against bourgeois ideological influences. As a partial explanation for this difference one may refer to the peculiarities of the Hungarian historical context: between the two world wars official historiography, influenced by German *Geistesgeschichte*, attacked the representatives of positivism from the right equating their activities with mere data collecting. Várkonyi, however, points out that positivist historians followed a well-considered method, the aim of which was not the accumulation of data per se but the detection of the laws of history by a judicious adaptation of the methodology of the modern natural sciences (pp. 17, 85-90). Moreover, she confines herself to the writings of the great nineteenth-century positivists, concentrating on their differing and changing interpretations of the meaning of history. This sober demarcation of her goal has a twofold advantage. On the one hand, it enables her to avoid getting involved in the much debated and debatable issues related to the more recent school of logical positivism and, on the other, it helps her stress the essential without losing sight of important nuances.

Várkonyi draws a sharp line between Marxism and positivism in her analysis of the major historical writings and theories of Auguste

Comte, J. S. Mill, H. T. Buckle, J. W. Draper, W. E. H. Lecky, Herbert Spencer, Émile Littré, and Hippolyte Taine by stressing the common roots of widely differing positivist tenets, namely, philosophical idealism, agnosticism, and a mechanistic methodology (p. 14). But having paid her respect to Marx, Engels, and Lenin, she claims that antidogmatic Marxist historiography must go beyond citations taken from the classics in order to show that the champions of positivism appreciated the first great results of the natural sciences and came "from the ranks of an intelligentsia imbued with liberal and democratic aspirations. In several countries, e.g. Latin American countries, Russia, Poland, Italy, Romania, Serbia, and elsewhere, and not least in Hungary, many progressive people were attracted by [positivism's] ideas" (p. 16). Várkonyi also emphasizes that every place where the remnants of feudalism prevailed in the mid-nineteenth century, the antifeudal tendencies and rational elements contained in positivist thought tended to reflect those interests of the bourgeoisie that pointed in the direction of social progress (pp. 17, 185). To be sure, these progressive features of positivism began to fade away, in Várkonyi's opinion, after the shock of the Paris Commune in 1871 as indicated by the later works of Taine and Spencer who by that time intended to defend established bourgeois society against the movements of the proletariat (pp. 38-41, 84, 120-21, 128-29, 186-89). Obviously the author prefers revolutionary to evolutionary theories; this is also why she attempts to relate the three major phases in the development of positivism to the revolutionary years 1830, 1848, and 1871 (pp. 123-25). Yet it is legitimate to view intellectual trends against the background of the social, national, and scientific revolutions of the age, and Várkonyi's interpretation is far from simplistic: it pays due attention to the variety of emphases in positivist writings, crediting them with the raising of some basic questions about the meaning of history and the introduction of the idea of historical progress into bourgeois historiography (pp. 145-47, 211-12). Noteworthy, also, is the effort to summarize, at least in brief, the lasting impact of positivism on thinkers and writers in Central and Eastern Europe. A reference such as the one made in connection with the third Hun-

garian edition of J. S. Mill's *On Liberty* in 1943, which was a symbolic protest against fascism at the time, goes as long a way toward the re-establishment of much needed intellectual intercourse among scholars of different nations, as does the renewal of reverence for the rational traditions of even antirevolutionary schools of thought (pp. 205-12). Respect for intellectual achievements and recognition of the right to err are, after all, preconditions of meaningful dialogues.

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GEORGE LICHTHEIM. *Imperialism*. New York: Praeger Publishers. 1971. Pp. vii, 183. \$7.50.

George Lichtheim, whose works on Marxism and the history of socialism enjoy a well-deserved reputation, has made an interesting contribution to the study of imperialism. Lichtheim is a Marxist of the left-Hegelian Frankfurt School, and his book may be seen as a useful interaction of both Hegelian and Marxist lines of thought. Unlike the upholders of the Lenin theory he understands that the imperial relationship can exist apart from a capitalist context, and he appreciates, as a Hegelian, the critical role that state power and national consciousness play. At times his primary purpose in writing seems to be that of disabusing Marxists of all shades—a rather substantial number of people these days!—of “crude” economic preconceptions.

The brief volume presents a stimulating but inevitably uneven survey of Western imperialism. A Hegelian emphasis on state and national pride dominates Lichtheim's view of empire from Rome through the Middle Ages, and, perhaps because of Richard Koebner's influence, Lichtheim chooses to discuss such questions as the *Kaiseridee* of the Holy Roman Empire rather than Venetian merchant imperialism or, earlier, the “usurious” imperialism of Rome. The stress shifts to economics beginning with Lichtheim's analysis of the maritime empires of mercantilism. Lichtheim sees, as Hobson or Schumpeter did not, the reality of an imperialism not merely atavistically associated with but erected solidly upon a free trade base. But, unaware of Wakefield's theories of capitalist imperialism in the 1830s and of the role of

classical economics in creating an imperial “ideology,” he views the 1880s as witnessing a “revival,” following an anti-imperialist period, of preindustrial imperialism.

In his final chapters Lichtheim dissects the Lenin theory, to the advantage of Karl Kautsky and Rosa Luxemburg, demonstrating that it does not work in today's world, and depicts a “populist” Maoism, which is more “revolutionary nationalism” than “authentic socialism.” One misses an attempt to deal with psychological or psychoanalytical theories of imperialism—that of Mannoni, for example—more especially since Lichtheim restores *hubris* to a role in political affairs. While Lichtheim does not attempt a theory to rival the classic formulations, it was not asked even of Hercules to do more than cleanse the Augean stables.

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*Anarchici e anarchia nel mondo contemporaneo: Atti del Convegno promosso dalla Fondazione Luigi Einaudi (Torino, 5, 6 e 7 dicembre 1969).* (Fondazione Luigi Einaudi, “Studi,” Number 11.) Turin: the Fondazione. 1971. Pp. 654. L. 6,000.

This book contains the proceedings of a symposium on anarchism held in Turin in December 1969. The goal of the symposium was to give the terms anarchism and anarchy as broad a meaning as possible, to include all antiauthoritarian thinkers and movements from Jean-Jacques Rousseau to Daniel Cohn-Bendit and other student protesters of the late 1960s. Naturally this effort met with some resistance from orthodox anarchists who wanted to exclude everybody else and from libertarians close to anarchism but who refused the label. The participants were of three types: internationally known social and political historians, scholars specializing in the anarchist movement and for the most part anarchists themselves, and veteran fighters from that movement. Each of the six sessions consisted of a paper on a general theme, three to five papers on various aspects of this theme, and a discussion. The six general papers were: “The Genesis of Anarchism in the Nineteenth Century” (Leo Valiani), “Problems in Spanish Anarchism” (Aldo Garosci), “The Contemporary International An-



archist Movement" (Gino Cerrito), "Anarchism between Communism and Individualism" (James Joll), "Anarchism and Bolshevism" (Arthur Lehning), and "Traditional Anarchist Thought and the Contemporary Revolt of the Young" (Jean Maitron). Although the main focus of the symposium was on Europe, with some attention given to Latin America, it is regrettable that virtually nothing was said about the anarchist tradition in the United States; Emma Goldman was mentioned only in passing and there was no discussion of Henry David Thoreau, the Industrial Workers of the World, or the anarchist wing of the North American New Left.

Of particular interest is Cerrito's paper, with its sixty-two-page bibliography. Cerrito claims that anarchism was put on the wrong track by Prince Peter Kropotkin's utopianism and has had to fight its way back to its essential character as an activist movement championing the victims of oppression *à la* Michael Bakunin and Enrico Malatesta. Kropotkin, according to this view, believed that a completely harmonious and libertarian society would come about through an instantaneous, all-encompassing revolution. The anarcho-syndicalists of the pre-1914 era tried to revive the preparation of a working-class elite for the more specific and limited task of destroying the state through political action, especially a general strike. But the First World War drastically weakened the illusion of a self-sufficient working-class movement. Anarchism was further weakened by the rival attraction of the Bolshevik Revolution and by repression from communists and fascists during the interwar years. The most notorious example of such repression came during the Spanish Civil War. Unfortunately, says Cerrito, this event helped revive the myth of an instantaneous, all-embracing revolution even among the practical-minded leaders of the Confederación Nacional del los Trabajadores. (This charge of impracticality was vigorously denied by the Spanish anarchists present.) For the period since the Second World War Cerrito's survey mentions a multitude of minuscule continuing and occasional groupings ranging from the numerous branches of the Movimiento Libertario Español to exiled anarchists from Bulgaria and Cuba to Yiddish-speaking anarchists in the United States and Argentina.

James Joll and Jean Maitron maintain in their papers that the New Left libertarians in many parts of the contemporary world are imbued with the traditional spirit and tactics of anarchism and consequently cannot escape its dilemmas. These dilemmas include communism versus individualism, violence versus peaceful persuasion, and rationalism versus irrationalism. Another dilemma, the insistence on both doctrinal purity and individual judgment, leads to splits and divisions and a shrillness typical of self-righteous ideologues. Lack of direct access to the mass media poses a newer dilemma to those many anarchists who hope to educate the masses along libertarian lines. The bad press their more violent activities get from the media—in both communist and capitalist countries—can draw attention to them, but it can also turn potential sympathizers against them.

In addition to its substantive papers this book has much of value for future researchers on anarchism. There are up-to-date bibliographical references and information on work in progress on many aspects of the movement. For example, A. William Salomone reported that a graduate student at the University of Rochester was filling a gap (noted by Joll) in our knowledge about a group of young libertarians in the German Social Democratic party during the 1890s; Eric Hobsbawm said that someone at the University of London was studying the Cuban tobacco workers during the 1920s. There is a short piece about a new Max Nettlau Library on libertarian movements, which opened its doors in December 1969 in the city of Bergamo. Finally, much of this book itself is a document on the anarchist temperament and the anarchist way of viewing the world. Here the libertarian optimism of Kropotkin lives alongside the bomb throwers and the pessimistic nihilists. Not only do all anarchists hate authority and the state in all its forms, but some, like Pier Carlo Masini, argue that anyone who seeks political power must be sick. Surely there is more to the matter than that.

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MAURICE MANDELBAUM. *History, Man, & Reason: A Study in Nineteenth-Century Thought*. Balti-

more: Johns Hopkins Press. 1971. Pp. xi, 553. \$15.00.

The subtitle of this book—*A Study in Nineteenth-Century Thought*—provides a better description of its contents than the title, *History, Man, & Reason*. The early parts of the work in which the author justifies his undertaking by asserting that thinking in the twentieth century is still widely determined by the notions and conceptions of the nineteenth century is interesting, stimulating, and a healthy corrective to those views which assume that, with the twentieth century, an entirely new intellectual world opened up.

In analyzing the thought of the nineteenth century the author focuses on three different strands of nineteenth-century thought, and this approach explains the book's title. In a first section the author is concerned with historicism. In a second section he deals with "the malleability of man," and this section discusses evolutionist, biological, and psychological theories: the central issue of this section is formed by the question whether conditioning can change human nature. The third section is concerned with the revolt against reason rather than with a defense of reason; the author discusses the positivistic rejection of all non-empirical knowledge and Kierkegaard's and Nietzsche's denunciations of rationalism.

Although the sharp separation of these three trends of thought leads to some overlapping and repetition because certain thinkers—for instance, Hegel—must be treated in the section on "History" as well as in the section on "Man," this form of organization has its advantages because it reveals interesting connections. For instance, I might mention that the philosophical context in which Helmholtz and Mach are placed is somewhat surprising but fully convincing.

The author's detailed knowledge of nineteenth-century thought is admirable. Nevertheless, I have certain reservations. He defines historicism as "the belief that an adequate understanding of the nature of any phenomenon and an adequate assessment of its value are to be gained through considering it in terms of the place which it occupied and the role which it played within a process of development." This seems to me an extremely broad definition, which, I think, allows no distinction between

"history" and "historicism" and which underplays what seems to me the crucial problem of historicism: the impossibility of assuming the existence of generally valid moral norms. Furthermore, the presentation is very abstract; and the criteria that the author uses are primarily of a logical nature. I doubt that this approach does justice to the views of natural scientists; their theories grew out of scientific research and were tentative insofar as they would be changed if further research led to new and different results. Finally, the conclusion of the book is very different from what the reader expects. After having emphasized at the outset that our thinking is dependent on nineteenth-century thought the author simply states that he believes "that the nineteenth-century views of the intellect which have here been examined will, before long, be unacceptable." He does not enter upon any further discussion of the possibilities of survival of nineteenth-century intellectual thought, but the main thesis of the book seems to be that the author shifts the break with nineteenth-century thought from the beginning of the twentieth century to a later period.

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WILLIAM J. BRAZILL. *The Young Hegelians*. (Yale Historical Publications. Miscellany, Number 91.) New Haven: Yale University Press. 1970. Pp. 305. \$10.00.

DAVID MCLELLAN. *The Young Hegelians and Karl Marx*. New York: Frederick A. Praeger. 1969. Pp. ix, 170. \$8.50.

These works on the Young Hegelians (David Friedrich Strauss, Bruno Bauer, Arnold Ruge, Friedrich Theodor Vischer, Ludwig Feuerbach, and others) fill a gap in intellectual history and also contribute to a better understanding of the formative years of Marx—and Engels, too. Professor Brazill, after examining the milieu in which the Young Hegelians emerged and functioned, analyzes their individual careers and thoughts during both the short period when they represented a collective force that influenced their contemporaries (including Marx) as well as in the following years when each of them went his own separate way, often in a more conservative direction. Professor McLellan's book restricts itself mainly to a

study of the thought of each Young Hegelian and to the influence that it had on the development of Marx's views during the period when the Young Hegelians still operated, more or less, as a cohesive group.

The very intensity with which Marx and Engels later attacked the Young Hegelians offers the best proof that they regarded their former associates as a force to be reckoned with. Their first two joint works, *The Holy Family* and the major part of *The German Ideology*, were designed to discredit "Bruno Bauer and Consorts" lest others continue to pray in a shrine where they themselves had once worshipped. It should be clear, therefore, that any evaluation of the judgment of Marx and Engels in this connection must be defective in the absence of the more thorough and separate consideration of the Young Hegelians as such that Professors Brazill and McLellan offer. Simultaneously, the role of the Young Hegelians in discrediting the old order and above all the validity of traditional Christianity is clarified. If a contemporary generation occasionally has proclaimed the death of God, the Germans of the 1830s and 1840s read a few similar pronouncements.

Both authors show a certain reluctance to link Marx too closely with the thoughts and especially the activities of the Young Hegelians. Thus they fail to bring out clearly that Marx's interest in a professorial chair in Bonn perhaps was subsidiary to his desire to join Bruno Bauer there in an atheistic crusade. An equal vagueness is apparent in connection with Marx's earlier role in the policies of the *Rheinische Zeitung*. He accepted, among other things, responsibility for the selection of Adolf Rutenberg, a Young Hegelian from Berlin, as the second chief editor of the paper in 1842. In tracing the impact of the Young Hegelians on Marx Professor McLellan notably confines himself too much to the search for specific ideas and concepts that the latter may have derived from the former. The letters that he consulted certainly suggest that Marx in the course of several campaigns bivouacked frequently with Bruno Bauer and Arnold Ruge. But a cautious approach is justified in many other instances, as when he cites certain concepts that Marx may have adopted from specific Young Hegelians, notably Max Stirner. McLellan states (p. 136) that Stirner's book was "to a large extent

an amalgam of current clichés . . . the ideas of alienated labour and exploitation were by no means confined to Marx at this time, even among the Germans."

Professor Brazill, in citing a special study on the subject, underrates the obstacles that German writers faced under the Carlsbad Decrees of 1819. He states that everything "less than twenty pages in length" was subject to censorship (p. 83). The decrees actually said twenty "Bogen" (printer's sheets of 16 pages each, namely 320 pages). A fuller identification is in order when he quotes the memoirs of a member of the Frankfurt Parliament to the effect that "next to Freiligrath, Feuerbach was the most silent man there" (p. 152). Otherwise the reader immediately thinks of Ferdinand Freiligrath who, following various vicissitudes in 1848, became what one might call the poet laureate on the editorial staff of Marx's *Neue Rheinische Zeitung* from 1848 to 1849. But these are minor lapses that detract little from the broad merits of the work.

The texts of Professors Brazill and McLellan are very readable and rest on a reasonably thorough investigation of the primary sources as well as secondary studies. If Professor Brazill appears somewhat repetitious, this reflects the difficulties inherent in the writing of successive accounts of the views of various Young Hegelians who had certain things in common. The documentation, bibliography, and indexes in both books certainly are adequate, though not painstakingly exhaustive.

OSCAR J. HAMMEN

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JOHN STUART MILL and HARRIET TAYLOR MILL. *Essays on Sex Equality*. Edited and with an introductory essay by ALICE S. ROSSI. Chicago: University of Chicago Press. 1970. Pp. ix, 242. Cloth \$8.75, paper \$1.95.

It has not escaped notice that the sacred text of the Women's Lib movement, "The Subjection of Women," was written by a man. By extending the canon to include "Enfranchisement of Women," the editor of the present volume has been able to issue it under the dual authorship of John Stuart Mill and Harriet Taylor Mill. ("Enfranchisement of Women" had originally been submitted for publication as Mill's own work but was later attributed by him to his

wife, his own share in it, he explained, being "little more than that of an editor and amanuensis.") The two also figure as the dual heroes of Alice Rossi's introductory essay, "Sentiment and Intellect," in which their marriage is represented as the near-idyllic relationship that approximates the "dream in the heart of many young women searching for liberation in 1970."

The nineteenth-century idyll unhappily falls rather short of the twentieth-century dream. Professor Rossi sees its only shortcoming in its denial of sexuality, apart from which she finds it an admirable union of sentiment and intellect. (Not, needless to say, Harriet's sentiment and John's intellect. It would be cruelly ironic if this were the meaning attached to the title of her essay.) Other Mill scholars might find it hard to understand how this vision of the marriage can survive a reading of Friedrich Hayek's edition of their correspondence, from which Harriet emerges as a person without intellectual distinction and notably deficient in sentiment—indeed as being uncommonly vain and overbearing, mean-spirited and small-minded.

One would like to be able to put aside personalities and judge this volume in terms of the very considerable issues raised by the subject itself. Yet personalities constantly intrude and not only in the introduction, which so inflates the intellect and elevates the character of Harriet Taylor. This first essay is followed by an exchange of views on marriage and divorce written by Mill and Harriet Taylor shortly after they met apparently for their mutual edification. Here, too, one cannot resist comparing their respective contributions—twenty pages by Mill of rational, lucid reasoning, and four pages by Taylor of lofty, romantic, inchoate sentiments, uninhibited by conventional syntax or logic. (In printing the latter, Rossi proves herself a better scholar than "Women's Libber," since it is just this kind of prose that confirms the worst stereotype of the "feminine mind.") Nor can one help comparing Harriet Taylor's "Enfranchisement of Women" with Mill's "The Subjection of Women," again to Mill's favor.

The theme of Mill's essay has become familiar enough in recent years. (The complete essay is available in several editions, and excerpts ap-

pear in numerous anthologies.) The subjection and subordination of women, amounting, Mill claimed, to a virtual condition of slavery, was typified in the marriage contract, which gave the husband control of his wife's property and person and made him the guardian of their children. It is sometimes said that the abolition of these legal disabilities has made Mill's thesis obsolete. But this does not take into account his subtle view of subjection, in which the victim may become an unwitting, or even a willing and willing, collaborator in her own abasement. Nor is his argument vitiated by the fact that men have often chosen to exercise their power benevolently, since it is the fact of power rather than its use or abuse that he found, and many still find, degrading.

But if we are to take the essay as a tract for our times, we must also consider another aspect of it, which might give pause to some, particularly among academics, who have so heartily endorsed it. For Mill was arguing not only for a policy of equality but also for a policy of liberty. It was equality of opportunity that he was seeking, which meant, for him, the open, unlimited freedom to compete. The demand for equal or even proportional representation in a faculty or governing body, for fixed quotas at various ranks, to say nothing of "discrimination in reverse," would have been utterly abhorrent to him. His position here, as in *On Liberty*, was thoroughly individualistic; it was not the corporate body of women—their corporate rights, identity, or consciousness—that concerned him, but the rights, identity, and consciousness of the individual person. And even more than in *On Liberty* (perhaps because it was written later), his argument was Darwinian: the free play of competition was necessary to ensure the survival and ascendancy of the fittest—the fittest individuals, whether men or women.

As with all sacred texts, this one has great potentiality for heresy.

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LEWIS H. GANN. *Guerrillas in History*. (Hoover Institution Studies 28.) Stanford: Hoover Institution Press, Stanford University, 1971. Pp. vii, 99. \$3.95.

This is a fine expanded essay on modern guer-

rillas and insurgency. The materials on ancient and medieval slave, peasant, and popular uprisings are too sketchy to be helpful; those that deal with nineteenth- and twentieth-century insurgency and counterinsurgency are excellent. In an essay of this kind an index and an annotated bibliography would have been most useful; the Hoover Institution Press has not had much experience in publishing materials of general military educational interest. The most obvious omissions in Dr. Gann's bibliography are C. E. Callwell's *Small Wars: Their Principles and Practice* (1899) and Wilhelm Rüstow, *Die Lehre von Kleinen Kriege* (1864). That there is no notice of the export of European-mounted rural constabularies as one feature of modern imperialism seems surprising in view of Dr. Gann's interest in colonial history. On the whole, however, this is a thoughtful and well-balanced work. Its tone is one of Clausewitzian respect for the chameleon-like nature of human conflict, and it is quite free of morals and maxims from other species that make some works on war as unhelpful as the experiment with George Smith's dog Dewey, who, instead of getting fits from his failures, proved to be "profoundly incurious about the puzzle."

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RAYMOND LISTER. *Antique Maps and Their Cartographers*. [Hamden, Conn.:] Archon Books. 1970. Pp. 128. \$8.50.

Raymond Lister's new book on the history of map making, following his earlier work, *How to Identify Old Maps and Globes* (1965), is an attempt to present a short survey of the art and science of map making to the general public. Such an addition to the literature of the subject would indeed be valuable; unfortunately, the manner in which the author organizes his material tends to leave the reader confused rather than informed.

Lister presents the information he amassed in three different ways. Some of it is organized in a straightforward chronological manner; some of it is described under the headings of the principal schools of cartography, for example, Low Countries, France, Great Britain; and some information is provided under regional divisions, for instance, the cartography of Africa, Asia, and Australasia. In the process,

some information is conveyed in a sketchy manner; for example, the presentation of Chinese cartographic traditions or the chapter on map making in America, which omits any mention of such pre-Columbian traditions as were preserved in Aztec codices.

While there are several misspellings and a few notable errors in the book, it does nonetheless render service to the general public in two ways. First, its bibliographical sections are truly useful, not pretending to be complete yet providing basic references and a number of detailed studies that clarify the topic under discussion. And second, those sections of the work that deal with British map making contain a sizable body of biographical data on British cartographers, and information on their work, that would have to be laboriously gathered from a number of scattered sources.

A final note on the illustrations seems to be in order. The book contains fifty-eight good full-page photos of maps and a handsome color reproduction on the dust jacket. Yet, while fully recognizing the exceptional importance and width of range of the collections of maps held by the British Museum, I cannot but regret the scanty representation of remarkable maps held in other collections, outside the United Kingdom. This lack is especially noticeable when it comes to specimens of map making prior to 1500 and to specimens of non-European cartography.

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OTTO MAYR. *The Origins of Feedback Control*. Cambridge, Mass.: M.I.T. Press. 1970. Pp. vii, 151. \$7.95.

OTTO MAYR. *Feedback Mechanisms in the Historical Collections of the National Museum of History and Technology*. (Smithsonian Studies in History and Technology, Number 12.) Washington: Smithsonian Institution Press. 1971. Pp. x, 133. \$3.25.

Mayr begins *The Origins of Feedback Control* with a definition of feedback drawn from that given in 1951 by the American Institute of Electrical Engineers. Armed with his definition he seeks in the past mechanisms that fit it and that subsequently form the raw data he uses to construct his chronological narrative. He opens

his narrative with the float valve that was discovered in antiquity, that drifted out of sight during the thirteenth century with the demise of the water clock, and that finally reappeared in the eighteenth century. Temperature regulators, as he recounts, appeared during the seventeenth century in the laboratories of scientists, but the regulators did not become practical devices until the early part of the nineteenth century. The search for efficient safety valves added pressure regulators to the technology of steam engines. The rising use of windmills during the eighteenth century fostered the development of feedback control mechanisms, the most notable of which, the rotating pendulum, inspired the realization of the steam engine governor, so he tells us. After discussing an instrument that permits the clocksmith to regulate clocks accurately, he closes his narrative. In a few final pages he makes an interesting comparison between the rise of liberalism and that of feedback control.

The book *Feedback Mechanisms* amplifies and broadens the treatment given in *The Origins of Feedback Control*. In the former book, which follows the same approach as that followed by the latter, the author chronicles the various types of feedback mechanisms found at the National Museum of History and Technology, thereby realizing a catalog that reveals the richness of the collection of instruments and machines found in this museum.

Mayr's bibliographical references show that he researched extensively in the preparation of his two monographs. In them the historian will find useful details but will look in vain for themes or arguments that would help him understand what factors led to the discovery of feedback controls and to their evolution. A definition drawn from engineering practice of the mid-twentieth century constitutes a poor start for a historical study because it leads to "whig history," to a natural history of technology. Mayr's book reads the way treatises on taxonomy did before the publication of Darwin's *On the Origin of Species*.

ROBERT M. MCKEON  
Tufts University

C. D. O'MALLEY, editor. *The History of Medical Education: An International Symposium Held February 5-9, 1968*. (Sponsored by the UCLA

Department of Medical History, School of Medicine; Supported by the Josiah Macy, Jr. Foundation. UCLA Forum in Medical Sciences, Number 12.) Berkeley and Los Angeles: University of California Press. 1970. Pp. xii, 548. \$20.00.

This is a very venturesome book. Sweeping from ancient times to the present, it tells the history of medical education in nearly every part of the world (with Canada, Australia, and most of Africa being the only major areas omitted). A collaborative effort, the volume brings together articles by nineteen scholars who attended a 1968 international symposium on medical history at UCLA.

Ambitious though it is, the book is not, on the whole, very successful. *The History of Medical Education*, to me, is flawed in two important respects. Most serious is the lack of any introductory or other editorial comment tying the various (and numerous) selections together into some semblance of a unified whole. A single title page is the only guide one finds to the book's four major sections ("The Earlier Period in the West," "The Modern Period," "Eastern Europe and the Far East," and "Western Hemisphere"). Admittedly, writing such an introduction would be tough going, given the diversity of the nineteen selections; but that does not reduce the need. It magnifies it. Not having editorial comment, one feels like a man who was expected to reassemble a bag of parts into the original machine without any blueprint to guide him. Such an introduction might have noted the historic contribution of rulers and national governments to medical progress, the frequent reform role of medical students, the anticipation of modern medical advances in earlier periods, and the influence of Islamic and Western medicine (chiefly in Renaissance Italy, the Netherlands, Scotland, and Germany) on other national systems.

The other shortcoming, less serious because it is not generally characteristic, was the failure of several authors to consider relevant social and cultural factors. More contributors might profitably have heeded their colleague, William F. Norwood, who suggested that medical historians follow the approach of the physician, who always "examines the history as well as the physical condition of the patient, including his total environment." Instead, several authors wrote as if medical education developed

in a social vacuum, unaffected by the wars, politics, social needs, and cultural forces that beat against medical school walls. As a result their articles were too much a tale of who taught what, when, to whom, and by what means. Into that category fell the selections on Classical Antiquity, the Middle Ages, Italy after 1600, the Netherlands, Scotland, India, and Ibero-America (although there, the concluding portion did note the influence of external factors).

Other selections, however, took a much broader view, and a few were solid historical accounts. I found the selections on Japan, Russia, the U.S. to 1900, England, and France very rewarding. The piece on Russia, besides pointing up the advances during the Tzarist period, reminded us again how far ahead of us the Soviets are in providing adequate medical care for all citizens. Other useful articles are those on twentieth-century American medicine, valuable chiefly as a bibliographic essay and on Indian and Islamic medical education, which forcefully (perhaps too forcefully) presented the claims of those regions to one-time world leadership.

*The History of Medical Education* will prove useful and interesting to cultural historians and historians of science and medicine, provided they read it selectively. Overall usefulness, however, is pretty much limited to reference purposes. One simply cannot read this book through and come away with any sense of a unity in the history of medical education on a worldwide basis.

EDWARD H. BEARDSLEY

University of South Carolina

JOHN R. PAUL. *A History of Poliomyelitis*. (Yale Studies in the History of Science and Medicine, Number 6.) New Haven: Yale University Press. 1971. Pp. xv, 486. \$15.00.

The history of disease has always interested medical historians, and writing the entire history of a single disease is an immensely challenging enterprise. Rarely has the task been carried out flawlessly, because it is difficult for one man to encompass with equal competence a span reaching, for instance, from Imhotep to Jonas Salk. Dr. John R. Paul (1893-1971), who for many years was professor of preventive medicine and epidemiology at Yale, has accom-

plished the feat better than most. In forty-two chapters he tells the story from ancient evidence of the crippling effects of polio through the modern recognition of its viral nature, its epidemiological and serological characteristics, and its growth on tissue cultures, to the effective means of immunization now used.

Though an ancient disease in all probability, only in the last hundred or so years has polio been recognized in widespread epidemic form. We now know that it is an intestinal as well as a neurological disease and that it is endemic in the less highly sanitated areas of the world, where most infections are unapparent. The development of the iron lung, the controversy over Sister Kenny's treatment with warm packs, the avoidance of immobilization of limbs, and the scientific excitement of the 1954 field trials with Salk vaccine followed by the tragic events of the Cutter-produced vaccine of the following year are beautifully told.

Some negative criticism, however, must be made. Because in two books of the *Epidemics* of Hippocrates no cases of polio are included, Dr. Paul therefore assumes that the disease existed in antiquity only in sporadic form. In the first place, the argument *ex silentio* is weak and, furthermore, of the seven books of *Epidemics* that have survived, only two have been translated into English. I hasten to add that Dr. Paul makes no pretensions about being a historian; yet he has written a book of history, so one is put on guard. More than half the book is devoted to the last thirty-five years, and quite justifiably so. Here one might question his relationship to the National Foundation for Infantile Paralysis, which never seems to have been close. At times he sounds a bit querulous, and he has failed to develop the theme of private philanthropy and basic medical research, which is central to the story of poliomyelitis. Perhaps Dr. Paul may be excused from the latter charge because he has relied heavily on Saul Benison's memoir of Dr. Tom Rivers, and he knew that Benison is at work on a major study of the National Foundation's role.

All historians of twentieth-century medicine, especially of the rapidly developing field of virology, will be indebted to Dr. Paul. He illustrates how a scientist in the midst of events in the recent past sees their evolution and implications. His honesty and modesty are appeal-

ing, but perhaps the latter is overdone. The inscription on a fine photograph of Jonas Salk on page 416 says: "To John Paul—who long pointed the way."

GERT H. BRIEGER  
Duke University

GILLIAN T. CELL. *English Enterprise in Newfoundland 1577-1660*. [Toronto:] University of Toronto Press, 1969. Pp. 181. \$7.50.

A new book on Newfoundland is always welcome to the growing community of students of Canadian history as well as to those interested in the expansion of Europe. This work deals with two areas of English enterprise, the development of the vast fisheries off the Newfoundland coast from 1577 to 1660 and the attempts to establish permanent settlements on the island for both strategic and economic reasons. Her work is based on the port records of the English west country, and for the colony she has discovered papers and a journal of Sir Percival Willoughby, one of the principal investors in the London and Bristol Company for the colonization of Newfoundland. These throw some new light on John Guy, the first governor of the colony and later mayor of Bristol. Since Willoughby was an owner of iron works, Guy reported the discovery of iron deposits but concluded that the colony's economy would be based on timber and fish. This, of course, involved difficulty with the fishermen, who made seasonal journeys to Newfoundland. The company also made grants to five other patentees including Baron Baltimore. Of all these attempts at settlement between 1610 and 1630 Mrs. Cell concludes none "had proved the answer to the problem of how to colonize Newfoundland," although they had proved it possible to live on the island. How many settlers remained is not estimated.

On the fishery itself the work gives only partial results. The impressive attempts at quantification indicate the development of a substantial re-export trade of Newfoundland fish by west country ports and fishermen, but the records are incomplete. On the enforcement of fish-eating legislation, Mrs. Cell differs from Samuel Eliot Morison in believing that it was usually evaded, and her work lacks the precise discussion of shipping and seafaring that the

work of J. H. Parry, Morison, and others has led us to expect. "Sack ship" even if meaningful to Newfoundlanders has little meaning when it is realized that such ships varied from seventy to five hundred tons, nor does she explain how a fishing ship of seventy tons could carry eight dories (p. 4) each weighing three to five tons to Newfoundland. In conclusion this is a useful work with an excellent bibliography but more tantalizing than satisfying in its conclusions because of the nature and scarcity of the sources.

FRANCIS COGHLAN  
University of New Brunswick

JOHN HOHENBERG. *Free Press/Free People: The Best Cause*. New York: Columbia University Press, 1971. Pp. xvii, 514. \$9.95.

The literature pertaining to freedom of the press is fairly extensive, including such representative titles as Morris L. Ernst's persuasive *The First Freedom* (1946) and Leonard Levy's legalistic *Legacy of Suppression; Freedom of Speech and Press in Early America* (1960). Yet it may well be true that nowhere in print is there as broad-gauged and informing an account of the history of press efforts to criticize the government as the volume under review.

*Free Press/Free People* is the seventh book of John Hohenberg, quondam working journalist in the United States and abroad and presently professor of journalism at Columbia University. His thesis appears to be that there is a "sensitive and little understood relationship" of interdependence between a free press and a free people and that neither can exist very long without the other in any country or any age. To substantiate this viewpoint, the author moves rapidly through history, focusing on such topics as the Zenger case, the struggle for survival of the United States abolitionist press, the contribution of Bismarck to the ancient and dishonorable art of press manipulation, and affirmations of the libertarian creed by John Stuart Mill, Franklin D. Roosevelt, and others. Whereas much of the book deals with the United States, Britain, and Western Europe—where the battle for a free press primarily has been waged—it also treats significantly the press in China, India, Japan, and Latin America.



Although the privately financed Hutchins Commission on the Freedom of the Press reported in 1947 that the current danger to press freedom lay within the press rather than outside, Professor Hohenberg has relatively little to say about the social responsibility of the press. Also there seems to be some contradiction between the quotation (p. 468) from Walter Lippmann about the fragility of an independent press and Hohenberg's vigorous affirmation that the days of the free press are not numbered.

The lack of either a bibliography or any other form of documentation seems unfortunate in the light of several unsupported statements about important issues. How, for example, does the author know that Japanese intelligence never picked up any information about the Chicago *Tribune* disclosure that the United States had broken the Japanese code (p. 258)? And what is his basis for saying (p. 348) that American newspapers had not fallen so low in public esteem for more than a hundred years as after the Kennedy and Oswald assassinations? These reservations aside, *Free Press/Free People* embodies a useful synthesis of the tensions between newspapers and their governments within broad dimensions of space and time.

J. CUTLER ANDREWS  
Chatham College

BRIAN CHAPMAN. *Police State*. (Key Concepts in Political Science.) New York: Praeger Publishers, 1970. Pp. 150. \$5.00.

This compact study deftly achieves the aim of Praeger's Key Concepts series: to clarify certain "emotive" terms through historical and semantic analysis. Five chapters trace the "police state" from its eighteenth-century origins to recent times; four chapters discuss "new meanings" of the term, police methods and psychology, and the author's conclusions. The analysis rests upon historical data from European states, whose concepts and practices of the police function have differed from those in English-speaking countries where the term "police state" has been used "indiscriminately" as a pejorative since the 1930s.

Chapman discerns three kinds of police states. The "traditional" model evolved within

the bureaucratic structures of the pre-Napoleonic French, Prussian, and Austrian monarchies and reached fruition in Fouché's "state apparatus" as protector, censor, and moral guide for society, subject to law and a rational division of administrative labor. After 1815 the traditional police state both continued (symbolized by Napoleon III) and then gradually lost its intrinsic qualities in the legal and institutional reforms preceding 1914. A "modern police state" model arose as Weimar Germany's *Rechtsstaat* yielded to Hitler's system: "It was like using a Rolls Royce not in order to carry passengers in comfort, but to run people down in the street." Nourished by an extraconstitutional taproot of power, the police penetrated the entire state structure and fused once-dispersed powers into a monolithic "offensive force" in society that finally became the director of internal policy. At that point the "totalitarian police state" superseded its "modern" progenitor. Himmler's police apparatus replaced the party as the state's ideological and administrative vanguard. Police power proliferated proportionally with the state's authoritarianism; in a state where authority knew no limits the police in time sought to become the state. Such, too briefly, is the author's historical argument.

Very perceptively Chapman relates the problem of police powers to modern society generally. Even in nontotalitarian states police necessarily wield degrees of arbitrary power, so that "the application of the law, although not the law itself, becomes relativistic," and the potential for abuse of power is endemic. Not only legal, but viable social norms shared by the police are necessary restraints. Impatient social idealism applied by reforming zealots bent upon rapid change can spawn a modern police state. We must constantly choose between the police as "either an instrument or a master."

The book is a stimulating challenge to further research and interpretation. One regrets only that the "modern" and "totalitarian" police state types are derived nearly exclusively from the Nazi experience; only passing reference is made to the USSR and none to Fascist Italy.

HOWARD C. PAYNE  
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*Mouvements nationaux d'indépendance et classes populaires aux XIX<sup>e</sup> et XX<sup>e</sup> siècles en Occident et en Orient.* In two volumes. (Commission Internationale d'Histoire des Mouvements Sociaux et des Structures Sociales.) Paris: Librairie Armand Colin. 1971. Pp. xvii, 402; 414-715. 100 fr. the set.

KONSTANTIN SYMMONS-SYMONOLEWICZ. *Nationalist Movements: A Comparative View.* Meadville, Pa.: Maplewood Press. 1970. Pp. ix, 91. Cloth \$5.50, paper \$3.50.

The two volumes of the first study are issued with the cooperation of UNESCO, the Institute for Social and Economic History at Heidelberg, and the Volkswagen Foundation. They are published under the auspices of the International Council of Philosophy and the Human Sciences and the National Tunisian Commission for UNESCO. The reports result from a four-year cycle of study in two stages: the first gathering held in Tunis and the second at the Twelfth International Congress of Historical Sciences at Vienna in 1965. Some forty scholars contribute to the synthesis; two-thirds of the reports are in French, one-third in English.

The purpose is to investigate the participation of popular classes in national movements of independence throughout the world and to find possible connections between national and working-class movements. Major sections are devoted to Europe, the Arab countries, black Africa and Madagascar, Asia, Canada, and Latin America. There is a summary for each major section, and six scholars (Labrousse, Portal, Bédarida, Droz, Chesneaux, and Bureau) contribute to the general conclusions.

As is to be expected in studies of this kind, no amount of briefing beforehand can assure common treatment. The result is that synthesizers find difficulty in extracting trends or currents. The varying approaches reflect the diversity of national movements. (Scholars of nationalism rarely find consensus.)

The papers are uneven in value. On the plus side is the brilliant contribution of Eric Hobsbawm (London), who explains why Ireland provides a classic case of nationalism, whereas Scotland and Wales have developed no serious movements for national independence. Equally meritorious is the paper by Werner Conze and Dieter Groh (Heidelberg) on "Working-Class Movement and National Movement in Ger-

many between 1830 and 1871." Of lesser importance are the contributions by historians in underdeveloped countries who cannot resist the urge to continue beating the dead horse of Western imperialism.

The final section is devoted to tentative conclusions on such matters as the existence of a diffuse national conscience, opposition to the foreigner, organic bonds between social struggle and national liberation, attitudes of revolutionary parties to nations, problems of actions between countries, and obstacles to national emancipation.

The contribution by Vladimir G. Trukhanovskii (Moscow) on popular masses in the Soviet Central Asian Republics reveals the sad fact that some Russian historians either do not know or are unwilling to accept the difference between history and historicism. Apparently there is a chasm between historians contaminated by the bourgeois Enlightenment and Soviet scholars blessed with total understanding.

Trukhanovskii opens with what he believes to be a correct interpretation of historical development. He is certain that, notwithstanding all its multiformity and contradictory character, history constitutes a single law-governed process. Mankind passes through five socioeconomic formations: primitive-communal, slave-owning, feudal, capitalist, and communist. The transition from one formation to another is determined not by the free choice of men but by objective laws operating independently of man's will under the principal law of social revolution. "With the establishment of the communist formation, antagonistic contradictions between the production forces and relations of production come to an end, the exploitation of man by man is abolished and the antagonistic classes and the class struggle disappear."

Trukhanovskii exempts the Soviet Central Asian Republics and Kazakhstan from his law of progression. Fortified by an eight-page bibliography from the "Archives," by "Documentary Publications," and by "Articles in Scientific Journals," he tells how popular masses in this area acceded to the socialist socioeconomic system by "by-passing the capitalist stage of development." The general law-governed process is not violated: "individual exceptions merely serve to confirm the general rule."

The Russian historian informs the newly emerged Asian and African states that they, too, "have the good fortune of being able, if they so desire," to obtain Soviet support. He quotes Lenin (1916): "We shall exert every effort to establish cordial and fraternal relations with the Mongols, the Persians, the Indians, the Egyptians, we consider it our duty and our interest to do so."

One turns with a sense of relief to the modest but scholarly study by Konstantin Symmons-Symonolewicz. *Nationalist Movements: A Comparative View* is a welcome addition to the growing literature on nationalism, certainly the finest sociological study since Florian Znaniecki's *Modern Nationalities* (1952). As a sequel to his earlier book, *Modern Nationalism: Towards a Consensus in Theory* (1968), in which he gave a valuable typology of nationalism, Symmons-Symonolewicz presents a comparative analysis of nationalism as a social movement. He seeks to fill a gap by considering nationalism as a social movement. He uses as his focus the factors that underlie the origin of nationalist movements, shape their development, and determine their source or failure.

Historians will be pleased by the author's approach: he is sensitive to nationalism in time and space. Unlike other sociologists he uses language readily understandable by scholars in other disciplines. He knows the enormous literature of nationalism well, and he uses it effectively. He concentrates on essentials, and his interpretations are rational and impressive.

In compact chapters Symmons-Symonolewicz discusses nationalism from cultural crisis to cultural and political self-assertion, national language and national boundaries, nationalism as ideology, the nation as a moral community, the crucible of alien rule, and nationalism in historical perspective. He describes the most common type of nationalism—that of subject peoples striving for cultural and political emancipation—and gives attention to the "life cycles" of European movements. He does not ignore the distinctive features of non-Western nationalism. Like the late Hans Kohn, he is convinced that there never has been and never can be any justification for a policy of national oppression. "So long as national, or ideological, oppression exists there can be no durable peace."

Symmons-Symonolewicz's study is encouraging to historians of nationalism who favor a multidisciplinary approach. It does much to help clarify an ism suffused with paradox, inconsistency, and contradiction. Above all, this first-rate study reveals that there is still merit in the Rankean goal: to tell it "wie es eigentlich gewesen ist."

LOUIS L. SNYDER

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BARRY M. GOUGH. *The Royal Navy and the Northwest Coast of North America, 1810-1914: A Study of British Maritime Ascendancy*. Vancouver: University of British Columbia Press. 1971. Pp. xvi, 294. \$12.00.

The northwest coast of North America was so remote from Great Britain and so close to the rising power and population of the United States that it is remarkable that Britain succeeded in holding it. Gough's thesis is that the main explanation lies in the strength of the Royal Navy. No matter how deeply Americans felt about "Fifty-four forty or fight," even President Polk could not overlook British power. The book traces the history of Britain's rule on the northwest coast during about a century, from the time when her fur traders appeared on the Pacific to the transfer to Canada in 1910 of the naval facilities at Esquimalt on Vancouver Island. Other topics of interest deal with the Oregon crisis, minor naval engagements on the Pacific with Russia during the Crimean War, Anglo-American difficulties during some small gold rushes, the dispute over the San Juan Islands, and repercussions in the northwest of the Anglo-American antagonism during and after the Civil War—all, except the Oregon controversy, events of little importance.

Gough naturally tries to make as much as possible of the impact of the navy in determining the outcome of these incidents. As a result he is apt to minimize other factors that may have been more significant. In analyzing the Oregon settlement, for example, he mentions Aberdeen's threat to commission thirty ships of the line and concludes: "In all likelihood, this alarming news induced the Americans to adopt a less belligerent attitude" (p. 79). Perhaps so; but the point is a contested one, and there is

more reason than Gough admits to believe that the thirty ships were not crucial.

Beyond this point of detail there is an overall lack of clarity as to whether the decisive factor was, not British naval operations along the northwest coast, but British sea power in general. It scarcely needs arguing that the fact that Britain had the world's strongest navy threw a shield over British Columbia, just as it did over all British colonies. Much less evident is the contention that the occasional appearance of one or more warships on the coast had any additional effect. Thus the book is primarily a history of British Columbia developing under the protection of a mighty motherland, rather than a history of the specific contribution of naval units operating on the northwest coast.

The book is well written and thoroughly documented. The appendixes give details about ships and officers on the northwest coast. For anyone interested in a fringe area of British sea power, the book is to be recommended.

CHARLES S. CAMPBELL  
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ROBERT A. HUTTENBACK. *Gandhi in South Africa: British Imperialism and the Indian Question, 1860-1914*. Ithaca: Cornell University Press. 1971. Pp. ix, 368. \$9.75.

"The genius of the British constitution requires," M. K. Gandhi wrote in 1911 as he struggled with the South African government, "that every subject of the Crown should be as free as any other, and, if he is not, it is his duty to demand and fight for his freedom so long as he does so without injuring anyone else." It is one of the many curious aspects of this struggle, as of the later and more fateful one in India itself, that Gandhi did not appeal to any primordial rights of man but always stayed within the framework of a legalism that depended upon appeals either to existing British laws or to what he conceived to be the spirit of the British Constitution. This fact is probably more significant for the successes and failures of the movements he led than any insights that may come from the elucidation of his theological and philosophical positions. A principal merit of Robert A. Huttenback's study is that the juridical element is kept to the forefront, a reminder that while the con-

frontations between Gandhi and the bigots are a moving testimony to the dignity of the human spirit, the basic confrontations took place in the law courts—and in government offices. It was there that the officials—South African, Indian, and British—who were faced with the political dilemma of liberalism tried to work out a solution for a problem that was insoluble.

The problem was very neatly stated by Sir West Ridgeway, a former Indian governor, in the inevitable letter to the *Times*. He noted that the people in England who wanted all citizens in the Empire to enjoy equal rights were at the same time believers in the Empire—"academic imperialists" he called them (p. 332). With brutal clarity he pointed out that it was precisely the self-governing colonies—that is, the ones run by the whites—that would never tolerate the entry of blacks in any number and certainly would never give them equal rights. There is a special poignancy, then, in following Gandhi's quest for equality before the law for his fellow Indians in South Africa, for one senses that he really believed that to appeal to the law was to reach beyond the oppressors to a source of freedom.

Huttenback has written a worthwhile book, but like the rest of the works that have sought to illumine Gandhi's place in history, it depends to a very considerable extent upon our prior acceptance of Gandhi's greatness. We are told what Gandhi did, and how people responded, but not much about why he acted, or why he engendered the response he did. As Gandhi recedes in time and public memory his life and times become more enigmatic, less susceptible to the methodology of political history.

As a comment on the continuing lack of academic communication despite all the efforts of recent years, it may be noted that B. N. Mishra completed a doctoral dissertation for Patna University in 1969 on this same topic of Gandhi's South African years. Reference to it probably would not challenge Huttenback's general emphasis, but it adds to the understanding of the issue by greater attention to its importance for Indian politics.

AINSLIE T. EMBREE  
Duke University

ARNOLD THACKRAY. *Atoms and Powers: An Essay on Newtonian Matter-Theory and the Development of Chemistry*. (Harvard Monographs in the History of Science.) Cambridge, Mass.: Harvard University Press. 1970. Pp. xxiii, 326. \$12.00.

The evolution of science immediately before and after the development of Newtonian physics has frequently been described as the gradual refinement of a powerful analytical approach involving reductive descriptions of natural phenomena and the quantification of forces. Seen from this standpoint, scientists of the sixteenth, seventeenth, and eighteenth centuries were arriving at the "mechanization of the world picture," a phrase recently considered to be synonymous with the emergence of modern science. Historians have occasionally described the work of such scientists as Lavoisier and Dalton as a "delayed revolution," indicating the belief that the introduction of the mechanical view came later in chemistry than in physics.

Arnold Thackray, who teaches the history of science at the University of Pennsylvania, has presented a different view of the development of chemistry. In this impressive and sensitive monograph in the Harvard series on the history of science, Thackray demonstrates that the triumphs of nineteenth-century chemistry were not built on a reductionist foundation but rather were achieved in isolation from the science of physics.

Thackray's thesis is that the descriptive model used by the partisans of the new chemistry rested on a different concept of matter than that of the physicists in the Newtonian tradition. The physicists were enamored with the measurement of micro-scale forces of attraction and wished to explain chemical reactions in terms of a mathematical law that would do for "chemical mechanics" what Newton's inverse-square law had done so brilliantly for celestial mechanics. Furthermore, many of them accepted Newton's "nut-shell" theory of matter, which assumed both the inertial homogeneity of matter and the inaccessibility to observation of its most elementary constituents. These assumptions were unproductive and even obstructive when applied to chemistry; progress in that field came only when these concepts had been replaced by a nonreductive emphasis

on macro-scale weight studies, the assumption of the inertial heterogeneity of matter, and a definition of elements that rested on the results of laboratory analysis rather than matter theory.

Thackray has not only revealed the internal characteristics of rival conceptual schemes of chemical structure but has also attempted to illustrate the links between these schemes and theology, philosophy, and developing industry. In a book of modest size no one could entirely succeed in the latter assignment; Thackray, realizing this limitation well, has written an unusual conclusion that is more a call for additional research than a summary of his own work. Both the achievement of the book and the invitation in its conclusion merit attention.

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THOMAS HAWKINS. *Lebesgue's Theory of Integration: Its Origins and Development*. Madison: University of Wisconsin Press. 1970. Pp. xv, 227. \$12.50.

Equivalents to the concept of integration had been developed by Eudoxus and Archimedes almost two millennia before differentiation methods, such as that of Fermat, achieved dominance in the calculus of Newton and Leibniz. Integration, as distinct from antidifferentiation, resumed its rightful place in analysis when Cauchy in 1821 and 1823 defined the integral of a continuous function. During the next few decades Dirichlet and Riemann set the stage, well outlined by Hawkins in his first chapter, for the problem central to this volume—the distinction between continuous and integrable functions. The remaining five chapters, covering the years from 1870 to about 1910, describe how, as more pathological types of functions came under scrutiny, theorems had to be amended under new standards of rigor. Questions were raised as to the integrability of functions such as  $\sin[1/\sin(1/x)]$ , which has infinitely many infinitely dense points of discontinuity. We read how, during the 1880s, the role of measure-theoretic properties of sets was examined by such men as Weierstrass and Cantor; and the last decade of the century is described by the author as "a period of transition" in which "the notion of measurability . . . formed the crucial link."

Peano and Jordan, in 1887 and 1892 respectively, had adumbrated the notion of measure of a set, leading in 1895 to Borel's concept of a set of measure zero.

Lebesgue, like Borel a graduate of the École Normale Supérieure, shortly after his graduation published in the *Comptes Rendus* a series of research announcements that culminated in his doctoral thesis of 1902, "Intégrale, longueur, aire," and his classic *Leçons sur intégration* of 1904. These works presented a concept of measure more general than that of Borel and a definition of the integral that included that of Riemann as a special case. In a series of further publications (references are found in Hawkins's ample bibliography) Lebesgue developed his integral into "an analytical tool capable of dealing with—and to a large extent overcoming—the unresolved problems that had arisen in connection with the old theory of integration." His work was warmly received, and its triumph was assured by further abstractions, such as the Lebesgue-Stieltjes integrals of Radon (1913).

Hawkins's volume is not to be digested in a casual perusal by a mathematical tyro; but a reader with seriousness of purpose and a thorough grounding in real analysis will be rewarded with a well-reasoned account of the step-by-step development of one of the most important concepts in modern mathematics.

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CHARLES HARVARD GIBBS-SMITH. *Aviation: An Historical Survey from Its Origins to the End of World War II*. (Science Museum.) London: H. M. Stationery Office; distrib. by British Information Services, New York. 1970. Pp. xvi, 315. \$9.00.

When Charles H. Gibbs-Smith's *The Aeroplane: An Historical Survey* appeared in 1960, it received rave reviews in numerous trade journals. Unfortunately, the significance of the book escaped the academic community, partly because the author so organized his material—less than 150 pages of narrative, followed by numerous quotations, a glossary, charts, and lengthy sections on the development of such things as kites and parachutes—

that the book was more a dictionary or an encyclopedia than a historical survey.

*Aviation: An Historical Survey*, an extensive revision of the former work, corrects some of the organizational problems as well as some of the minor errors. Among other things, the glossary and quotations are kept, but the narrative is expanded (discussions of technical problems are reserved for a later volume). The result is one of the finest books published on the history of aviation, one as impressive for its perspective as for its detail.

Gibbs-Smith devotes almost half his narrative to the pre-Wright brothers era. The long and laborious birth of aviation, he feels, was in many ways more important than the natural maturing in an age of technology. The former involved thinkers and dreamers centuries removed from practical flight, followed by more than a century of "forerunners" who attempted to turn hope into accomplishment. Then, in little more than a decade, success. What followed was mostly one technological development after another.

Gibbs-Smith's sterling reputation, bearing the imprint of an impressive background in aviation history and a sound understanding of the science and technology associated with flying, certifies both his emphasis and his conclusions. His view of the Wright brothers, for example, contrasts sharply with the commonly accepted idea of two lucky bicycle manufacturers. He shows the Wrights to have been first-rate inventors and thorough students of flight who grasped certain essentials that somehow escaped their European counterparts. These essentials, according to Gibbs-Smith, were mastery of glider flight as a necessary prelude to powered flight, acceptance of the best aerodynamic theory, adequate testing before flight, and perhaps most important, the courage born of understanding to build inherent instability into their aircraft. In deliberately making their airplanes so they would have to be piloted with skill, the Wright brothers were selecting the one path that allowed mastery of the air. This, Gibbs-Smith suggests, was the reason why the Wright brothers were flying long distances and turning with ease, while their European rivals were still trying to get airplanes to stay in the air.

The refreshing perspective of *Aviation: An*

*Historical Survey* is, however, only one of its prime qualities. There is also Gibbs-Smith's skillful blending of ideas, budding technology, scientific theory, and the perseverance of pioneers in describing one of the greatest advancements of all time. Hence the book should appeal to the intellectual historian, the historian of science and technology, the aviation buff, and perhaps the area specialist who, thanks to efforts beyond his present horizons, now races with ease to lands and archives that were once well out of reach.

PHILIP M. FLAMMER  
*Air University Review*

BARNET LITVINOFF, editor. *The Letters and Papers of Chaim Weizmann*. Series A, Volume 2, November 1902–August 1903. New York: Oxford University Press. 1971. Pp. xlvi, 489. \$12.75.

This second volume of the letters of the man who was for many years the leading figure in world Zionism maintains the high standard set in the first. One might have some reservations about the wisdom of the decision to publish all the letters in the Weizmann Archives, if only because at the present rate the publication is likely to require several decades. The condensation of letters to Weizmann to extracts or summaries in footnotes is at times awkward, and transliteration from Yiddish and occasionally Russian is not standardized. But overall the editing, translation, and annotation of this multilingual collection are excellent.

If the contents of the letters are relatively unspectacular, it is because in the period covered (November 1902–August 1903) Weizmann was not yet in the forefront of Zionist leadership, and in any case the movement itself was at a low ebb. The present volume closes on the eve of the Sixth Zionist Congress and the great controversy over the so-called Uganda (East Africa) colonization scheme, and instead of Uganda the largest single group of letters deals with Weizmann's activities in behalf of the premature project of establishing a Jewish university in Europe or Palestine. Nevertheless there is much of great interest here. The reader gets glimpses of Weizmann's enormous charm and intelligence and considerable material for Zionist and general Jewish history in the early twentieth century. The highlight of the collec-

tion is undoubtedly Weizmann's long letter of May 6, 1903, to Theodor Herzl, which gives a broad picture of the state of Russian Jewry and especially of its youth—a youth seen as drifting into the arms of socialism and the *Bund* and as alienated by the semiclerical and "Western" official Zionist orientation. Herzl and the early Zionist leadership appear unresponsive to the needs of the East European Jewish masses, whose spokesman Weizmann tried to be.

Future volumes in this monumental series will no doubt be of even greater interest, for they will trace Weizmann's rise and Zionism's stormy history. The high standards set in the early volumes ensure that scholars will find them fascinating to read and, above all, a comprehensive and useful source.

SOLOMON BEINFELD  
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RONALD W. CLARK. *Einstein: The Life and Times*. New York: World Publishing Company. 1971. Pp. xv, 718. \$15.00.

The present state of Einstein studies invites the energetic biographer. There exist vast archival materials largely untouched; a great number of relevant specialized works; and, in the way of competition, recollections of former associates, collections of anecdotes, hagiographies, picture books, potboilers, and "great lives in science." Mr. Clark, a practiced biographer who only three years ago gave us three generations of Huxleys in one volume, has responded to the invitation with the ambitious *Life and Times* under review.

Mr. Clark's particular contributions are the wide range of his sources and the admirable determination to fashion a full and balanced biography from them. They are, however, less easy to exploit than to discover. Mr. Clark's superficial knowledge of modern physics puts him at a disadvantage. The spirit as well as the content of Einstein's work eludes him; he must substitute romance or mystification for analysis (Einstein "soared up into the mathematical stratosphere where the battle had to be fought"); and he consequently cannot describe his subject's genius in terms of its most characteristic output. Likewise Mr. Clark's innocence of both the history and philosophy of modern science

prevents him from placing Einstein's contributions in context. In particular, he misses the point of the heroic debates between Einstein and Bohr over the interpretation of quantum mechanics, and he exaggerates the bearing of the discussion on the scientific isolation of Einstein's later years. Nor does Mr. Clark appear to have the temperament or style required for his task. Where Einstein is economical, witty, careful, and precise, Clark is repetitious, prolix, pallid, and diffuse.

These characteristics mar even those parts of the book where Clark is most at home. His accounts of Einstein's external career—the employment in the Bern patent office, the steps up the academic ladder, the special professorship in Berlin, the pacifist and Zionist activity, the move to Princeton, the sailing, the fiddling, and so forth—are interesting and informative. But Clark gives his reader little help in constructing a consistent picture of the actor in these events. He emphasizes what he calls “paradoxes”—Einstein the unreligious Zionist, Einstein the pacifist behind the atomic bomb, Einstein the naturalized Swiss and German hater, who clung for twenty years to his position in Berlin. He tells us that the ill and soxless recluse of Princeton, the weary world figure, the uprooted European, differed little from the vigorous and dapper patent clerk of Bern, or from the famous Berlin professor, the friend of Rathenau, Weizmann, and the queen of the Belgians. He describes Einstein's nonscientific views and plans sometimes as “naive,” sometimes as “shrewd,” and sometimes as “prescient.” When one ponders these dicta in the light of Clark's claim that Einstein was “similar in work and outlook” to Max Planck—a strong nationalist, a good German, stiff, formal, and conservative—one conjectures that Mr. Clark has not read Einstein's character any more closely than he has read the theory of relativity.

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GLENN B. INFELD. *Disaster at Bari*. New York: Macmillan Company. 1971. Pp. xii, 301. \$7.95.

In August 1943 reports reached the United

States that Germany might use gas. President Roosevelt responded with a public warning and confidential permission for the dispatch of a supply of mustard gas to the Italian theater. Some one hundred tons of the chemical were sent in strictest secrecy to the depot at Bari aboard the *John Harvey*. At this time the Allied air forces were overconfident; Bari, under “that old handmaiden of disaster, multiple command,” lay open, exposed, and well lighted. The Luftwaffe struck on December 2, 1943; it destroyed seventeen ships, damaged eight others, and caused a death toll of over a thousand among Allied military and Italian civilians. The *John Harvey* went down with all its crew and, thus, those who might have given warning were eliminated.

The Bari tragedy was well hushed up at the time and remained rather obscure until now. Infield, himself a pilot in the U.S. Army Air Force during the war, took great pains in gathering the material for his book. In Germany he interviewed General Student and Luftwaffe pilots who served under him; in England officials who had been involved gave information albeit reluctantly; in the U.S. Infield received full cooperation from the military. The bulk of the material, however, came from the author's visit to Bari, his survey of the area, and his discussions with survivors. He has put together a very readable monograph with clever chapter titles and has piled up the evidence to sustain his conclusions. Very few of the 617 military and merchant marine casualties need have died, he states, if knowledge of the chemical agent had been immediately available. “Most of the deaths were due primarily to the prolonged exposure.” Infield insists that “secrecy, then and now, is the main cause of such tragedies. All matters pertaining to chemical warfare agents are cloaked in this ‘iron curtain’ of secrecy, usually to the detriment of all concerned.”

There are no footnotes but the author provides a full bibliography and appendixes, and the sources for specific fact statements are rather clearly indicated by the context of the narrative.

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D. C. WATT. *Survey of International Affairs, 1962*. (Issued under the auspices of the Royal Institute of International Affairs.) New York: Oxford University Press. 1970. Pp. ix, 558. \$14.50.

The Royal Institute's superbly useful Survey of International Affairs series has now reached 1962 with the release of the second volume prepared under the editorship of D. C. Watt. In addition to taking overall responsibility for the 1962 volume, Watt has written most of the book himself, delaying publication to take account of "the literary 'fall-out' of the Kennedy régime." This fallout greatly enriches the narrative in a number of chapters, for the *Survey* is based on already published materials and the information now available on the events of 1962 is unusually full, particularly with regard to the involvement of the United States in the world arena.

Given the inherent limitations in writing current history without access to government archives, this volume is admirably broad in coverage, objective in tone, and clear in perspective. Watt summarizes again the Cuban missile crisis and threads his way with clarity through the maze of Anglo-Franco-American policy in Europe, carrying this subject to the defeat of President Kennedy's "Grand Design" at the hands of General de Gaulle early in 1963. Watt includes also a brief treatment of the strategic policies of the nuclear powers (the United States, the Soviet Union, the United Kingdom, and France); a chapter on what he calls the "Intermarium," which deals with Finland, Poland, Yugoslavia, and Austria, with a separate section on the South Tirol; a summary of the crisis in the United Nations; and a very interesting survey of the acquisition of western New Guinea by Indonesia.

John Erickson is responsible for the illuminating discussion of Sino-Soviet relations from October 1961 to January 1963, while John Major treats the difficult problems of Africa, with a special chapter on the explosive relations of France with Algeria. Michael Donelan skillfully compresses into twenty-five pages the financial policies of the Western powers.

The crucial problems of Southeast Asia, the Sino-Indian conflict, and the Arab world centered on Cairo fall to Richard Gott. Gott did not, of course, have available the *Pentagon Papers* when he chronicled the events of 1962 in

Vietnam and Laos, but their revelations probably would not have altered his conclusions materially.

The usefulness of this volume as a reference work is marred by the shabby treatment given by the Oxford editorial staff to senior United States officials below the very top level. Some officials (Rostow and Sorensen) are the victims of mere typographical errors, but the names of others—Arthur M. Schlesinger, Jr., Foy Kohler, Livingston Merchant, and Howard Trivers in the Executive branch and Harry Flood Byrd and Wayne Hays in Congress—seem to have been maltreated through deeper carelessness. The indexer has copied the mistakes of the text and has then added errors of his own. Finally, there is a certain lack of precision in the titles ascribed to various officers, notably Paul Nitze, who turns up in the wrong department. Americans surely deserve editorial care equal to that bestowed on statesmen from Europe, Asia, and Africa.

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Department of State

#### ANCIENT

HANS GOEDICKE. *The Report about the Dispute of a Man with His Ba*. (Baltimore: Johns Hopkins Press. 1970. Pp. vii, 248. \$12.00.

The ancient Egyptian literary work commonly known by the title of the *Lebensmüde* and here given a new title is one of the best known of all ancient works of literature, and many Egyptologists have made attempts to translate it and to understand its complexities. This new edition is an important contribution to the discussion that has continued ever since the first edition by Erman appeared in 1896, and the full treatment of both subject matter and language in this study advances our understanding of a fascinating document.

The text almost certainly dates from the first Intermediate Period or early in the Middle Kingdom and is one example of a group of writings all of which have, so far as we can understand them, considerable literary merit and are known for their pessimistic view of life and of the condition of Egypt at the time. The work studied here does not entirely fall into this category and as the editor shows is a specu-

lative work concerning the nature of the life of man. It takes the form of a dialogue dispute between a man and his *ba*, the man defending an idealistic view of the afterlife while his *ba* takes a more practical and material view and deprecates the excessive concern of the man with death and its aftermath. The nature of the *ba*, that important element in Egyptian religious belief sometimes translated as "soul," is discussed by the editor at some length.

The book consists of a very interesting and elaborate discussion of the nature of the text, followed by a detailed and scholarly commentary on grammatical, lexical, and semantic aspects. The hieratic text is given by photographs of the original papyrus accompanied by a hieroglyphic transcription. This is preceded by a straightforward English translation from which it can be seen how difficult it is, in view of our limited knowledge of Egyptian, to have a proper understanding of religious and philosophical writings.

The non-Egyptological reader may well find it difficult to appreciate how the very complex and closely argued thesis in the main discussion can be sustained by our still hazy knowledge of what the document really says. The edition and commentary is a tour de force and gives some glimmering of the nature of Egyptian thought and literature.

P. L. SHINNIE

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E. W. MARSDEN. *Greek and Roman Artillery: Technical Treatises*. New York: Oxford University Press. 1971. Pp. xviii, 277, 14 plates. \$16.00.

Engineering handbooks are a byword for meager style and turbid jargon; without the accompanying diagrams they would be well-nigh impenetrable. Ancient technologists conform to this pattern except that their illustrations, when not totally obliterated, are corrupt. Recently one cluster of these perplexing writers has engaged the attention of E. W. Marsden of Liverpool; his *Greek and Roman Artillery: Historical Development* (1969) collated the evidence in a novel but plausible synthesis. Syracuse, it seems, gave us the crossbow in 399 B.C. Somewhat later, perhaps about 350 B.C., Philip's Macedonian artificers invented the cata-

pult, which despite appearances is not a crossbow—the arms are not joined but are mounted individually in twisted rope springs. Sundry refinements followed; certain larger versions were adapted for stones. Altogether the catapult reigned supreme without radical innovation until the advent of the trebuchet in the twelfth century.

Now we have the companion volume with texts of the five primary sources, translations (often the first), commentary, and drawings. The first primary source, Ctesibius of Alexandria's *Artillery* as re-edited by Hero of Alexandria, discusses the crossbow and modifications of the catapult until 270 B.C. One quibble: the text figures for euthytone catapults show the curved side of the crossbeams facing backward; according to a sketch transmitted in the manuscript they should be reversed. The second source, Bito of Pergamum's *How to make War Engines and Artillery*, specifically deals with four crossbows, a siege tower, and a scaling ladder. Marsden's date (ca. 240 B.C.) makes Bito old-fashioned; with the alternative, 140, he is hopelessly anachronistic. "The most curious thing about this work is that no one so far has been able to understand it," A. G. Drachmann commented in 1963. Marsden unravels every knot. Philo of Byzantium's *Artillery*, the third source, is a discussion of standard catapults for stones and javelins and four experimental designs. It was probably written between 230 and 200 B.C., for it ignores developments after 200 B.C. Marsden has made a full-size three-span arrow firer, weighing seventy-five pounds and shooting over three hundred yards. Vitruvius's *Architecture* (10. 10–12. [ca. 25 B.C.]), the fourth source, describes two catapults. The figures in his table of dimensions for different calibers are unexpectedly low; Marsden suggests that they are Roman inches rather than Greek digits. The fifth source, Hero of Alexandria's *The Hand-Catapult* (after A.D. 62), has baffled commentators such as Rudolf Schneider ("das Bruchstück eines . . . Lexikons für Konstrukteure," "vom Geschützwesen kaum eine Spur" [*Römische Mitteilungen* (1906)]). In Marsden's view it sets forth the standard Imperial arrow thrower that was pictured on Trajan's column and mentioned by fourth-century authors. Paradoxically some sources designate one component as the "bow"; it is not a bowstave, says

Marsden, but the sighting arch. His reconstructed *cheiromballistra* shoots 150 yards.

The final chapter concerns the sturdy "one arm" or "wild ass" (medieval mangonel), Hellenistic invention that finds no description before Ammianus. Marsden's scale model shot small missiles almost five hundred yards.

In short Marsden has curbed a whole stable of uninviting and refractory texts. His book is serviceable, ingenious, and persuasive.

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C. BRADFORD WELLES. *Alexander and the Hellenistic World*. Toronto: A. M. Hakkert. 1970. Pp. 265. Cloth \$8.00, paper \$2.95.

This book was ready for the publishers at the time of the author's sudden death in 1969, and it was seen through the press by Alan Samuel, who has added a brief epilogue. Welles inherited from Rostovtzeff the tradition of introducing Yale undergraduates to Hellenistic history, and every teacher who has attempted a similar task has wanted a book that complemented Tarn's *Hellenistic Civilisation* by offering an account of political history with more detail than in his introductory chapter. The difficulty of writing such a book is enormous, even for someone who is as perfect a master of his subject as Welles was.

After a brief introduction describing the achievement of Philip, there is an account of Alexander in chapter 2 in the "modern" manner, in full reaction against the interpretation of Tarn, who defended Alexander against his attackers and believed he was influenced in great part by idealistic motives. Welles insists on Alexander's ruthlessness, invites us to believe that his detractors are less likely to be lying than his defenders, and that Arrian's faith in the veracity of Ptolemy and Aristobulus is unjustified. The argument is presented with skill and conviction, but to those of us who want to know why Alexander was respected by some while others feared and hated him, it may seem rather one-sided.

The next chapter sets out to tell the story of the Hellenistic kingdoms from the death of Alexander to the battle of Actium, covering nearly three hundred years in a hundred pages.

It is perhaps too much to expect that a summary narrative of this kind will guide a student safely through the forest of these troubled centuries. It is easy to say that Welles included too much detail, but if one starts to cut down trees in the hope of better revealing the character of the forest, the result may be to transform the forest into a desert that has neither character nor form. Is the traditional "survey" the best method of introducing students to this period of history? It seems to me a pity that Welles adopted the plan of a single continuous narrative, attempting to keep track of simultaneous events in several different areas, instead of splitting up the narrative with digressions or giving separate accounts of the individual dynasties and the different problems and themes that call for discussion. Must an account of political developments necessarily precede a discussion of society and culture, and must the story necessarily be told in chronological order? Would it be helpful to describe the situation as it was about 250 B.C. before trying to explain how this result was reached?

I raise these questions the more readily because the highest praise is due to the subsequent chapters on "Social and Economic Aspects" and "Hellenistic Culture." Inevitably every reader will miss something that he would have liked to see included (my own regret was to find so little attention devoted to Hellenistic schools and the development of the gymnasium), but this is where a student will discover how much is known about the Hellenistic world that cannot be known about earlier periods of ancient history and how much knowledge has been gleaned from papyri and inscriptions.

LIONEL PEARSON  
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STYLIANOS SPYRIDAKIS. *Ptolemaic Itanos and Hellenistic Crete*. (University of California Publications in History, Volume 82.) Berkeley and Los Angeles: University of California Press. 1970. Pp. 113. \$3.00.

The title of this small volume is somewhat misleading; much of the text deals neither with Hellenistic Crete nor Ptolemaic Itanos but with eastern Crete in the Hellenistic period. In brief, Spyridakis argues that by Hellenistic times three cities of consequence, Itanos, Prais-

esos, and Hierapytna, survived in this area. Because of the "natural antagonism" between Eteocretan and Dorian, an explosive atmosphere, rife with tension and continuous conflict, prevailed. This incessant warfare, he believes, prompted the Itanians to invite the Ptolemies into their city about 270 B.C. Once established there the Ptolemies promoted peace in the area and used Itanos as a base for recruiting mercenaries until their deteriorating position in the eastern Aegean forced them to withdraw about 200 B.C., though they did return briefly under Ptolemy Philometor.

While Spyridakis's discussions of individual inscriptions and particular events are adequate, the work suffers from a number of serious defects. Only a few can be mentioned here. His desire to include everything, relevant or not, has resulted in a poorly organized volume. Matters of consequence are ill defined; thus on a single page (p. 3), Itanos is referred to as a virtual protectorate, a protectorate, and a possession of the Ptolemies, and the confusion is never entirely resolved (cf. pp. 82, 88). Of greater consequence, the author's conclusions seem to emerge more from his own basic assumptions than from the evidence he presents. His discussion of events after 145 B.C. demonstrates conclusively that warfare did not decline in eastern Crete after the destruction of Praisos, the one supposedly Eteocretan stronghold in the area. This negates his conclusion that warfare between Itanos, Praisos, and Hierapytna derived from some natural antagonism between Eteocretan and Dorian.

Anyone interested in Hellenistic Crete will find Van Effenterre more rewarding; anyone interested in Ptolemaic Itanos would be well advised to draw his own conclusions from the epigraphical evidence assembled by Guarducci.

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JAMES H. OLIVER. *Marcus Aurelius: Aspects of Civic and Cultural Policy in the East*. (Hesperia: Supplement XIII.) Princeton: American School of Classical Studies at Athens. 1970. Pp. xv, 160, 8 plates. \$10.00.

The purpose of this work is to publish a text, translation, and discussion of a remarkable document of Marcus Aurelius from Athens, con-

taining his decisions in cases concerning office holding, membership of the council and Areopagus, and the appointment of the Athenian members of the Panhellenion (the Pan-Hellenic council of cities instituted by Hadrian). Documents revealing decisions by the Roman emperors continue to be published in ever-increasing numbers. But this one surpasses in importance and difficulty both the *Tabula Banasitana*, also from Marcus Aurelius' reign, now being published by William Seston in *Comptes-rendues de l'Académie des Inscriptions*, and the long series of imperial letters from the excavations of Aphrodisias.

The puzzles it presents as a document are rather briefly touched on by Oliver (pp. 35-37), as also by C. P. Jones in his prompt and valuable re-edition of the text, "A New Letter of Marcus Aurelius to the Athenians" (*Zeitschrift für Papyrologie und Epigraphik* 8 [1971]: 161). In fact it is clearly not a letter but appears from its final paragraph to be a collection of imperial legal decisions on Athenian cases put together and translated into Greek at the imperial court. The occasion and purpose of the compilation is not clear from internal evidence but may have been mentioned in the missing prescript.

Oliver is similarly brief on the background of imperial jurisdiction, a major, indeed central, part of the emperor's functions, on which the new document provides a flood of new evidence and new problems. Instead, as his wide-ranging title implies, he moves immediately to a series of problems about the constitution of Athens, events in the reign of Marcus Aurelius, and, above all, the Panhellenion, on which his chapter 4 provides the fullest collection of evidence now available. In short, he has published, with admirable speed, a vast and fascinating new document and has also provided with it both less and more than the reader could have hoped for.

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JÜRGEN DEININGER. *Der politische Widerstand gegen Rom in Griechenland, 217-86 v. Chr.* Berlin: Walter de Gruyter. 1971. Pp. xviii, 279. DM 48.

The title of this book is to be taken with strict

literalness; it is not a discussion of Roman imperialism in Greece but of the resistance, mainly the internal resistance, of Greek states to the expansion of Roman political and military power in their country. Roman imperialism is taken as an unanalyzed fact. The foregoing is a favorable criticism; it is good and serviceable to have a book of this kind. Nevertheless it is perhaps not quite so original as the author thinks. Writers in the past held the concept that the "democrats" were anti-Roman and the "oligarchs" pro-Roman; Deininger points out that we have been aware for some decades that the word "democracy" in the Hellenistic Age regularly connotes what classical Greece had known by "oligarchy." Therefore, he argues instead that the upper classes were divided into pro- and anti-Roman factions while *hoi polloi* were regularly and consistently anti-Roman, whether moderately or extremely. All of this is quite true, but one doubts whether in recent decades even those scholars who sometimes slipped, anachronistically, into using democratic in its classical sense really understood the word in that sense. One notes that Deininger himself uses the word anachronistically (pp. 240-41).

Deininger, in effect, discerns three phases in Greek anti-Romanism; the first, limited to the late third century, stigmatized the Romans as barbarians. During the second phase, from the Second Macedonian (or better, the Aetolian) War to the Third Macedonian War, there was a quarrel between anti- and pro-Roman upper-class leaders, the former supported by *hoi polloi*. The defeat of Macedonia and the rigorous suppression of anti-Romanism in Greece in 168-67 B.C. ended upper-class resistance to Rome; yet Deininger must admit that Greek leaders during the Achaean War, two decades later, certainly included upper-class figures (cf. pp. 228, 234). Finally, one may quibble about the terminal date as marking the absolute end of political resistance to Rome: even in the principate, Athens, at least, witnessed such resistance, if anti-Roman riots of *hoi polloi* meant anything.

In all, this is a useful book but not as important or profound as is the author's previously published study of Roman provincial councils.

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CHESTER G. STARR. *The Ancient Romans*. New York: Oxford University Press, 1971. Pp. 256. \$7.95.

It is occasion for praise and gratitude when a distinguished scholar sets his hand to an introductory work aimed at high school and junior college audiences. Students who enter Roman history via Professor Starr's admirable volume will not have to divest themselves of multiple misconceptions when they reach more advanced levels.

The work is intelligently conceived and successfully executed. Starr abandons the traditional textbook's concern for "coverage." There is not much bald narrative or tedious recital of facts. Four major subjects receive emphasis: the clash of Rome and Carthage, the career of Julius Caesar, the Antonine Age, and the rise of Christianity. Starr rightly prefers ample treatment of a few topics to a superficial touching of all bases. Special essays are devoted to certain other items, like the Etruscans, the army, Roman law, and late imperial art; Starr offers them as separate objects of attention, not buried in a mass of narrative. Excellent illustrations, charts, and maps grace the text; and the illustrations possess full explanatory comments, not just standard captions. Selections from the sources are placed at the end of each major section, organized under headings and elucidated by brief introductions. A convenient and appropriate design.

Some criticisms may be registered. It is misleading to portray the Twelve Tables as guaranteeing fair justice to all citizens (pp. 15-16) and quite erroneous to apply the term "democracy" to the Roman Republic (p. 16). Montesquieu and the Founding Fathers may have seen in the Republican constitution a system of checks and balances and a separation of powers, but their notions ought not to be confused with the facts (p. 59). The equestrian order as a "separate wealthy class" can hardly be discerned in the third century B.C. (p. 25). Nor should the equestrians be identified with commercial and financial leaders (p. 70). Starr omits all mention of agrarian problems and barely notices the lower classes in the late Republic (pp. 71ff). The concept of a "golden age of the good emperors" (p. 180) is belied by many of the sources that he himself includes. Little attention is paid to the Jewish back-

ground of Christianity and no hint given of the conflict in early Christianity over conversion of the Gentiles (pp. 185-87). The stress on imperial opposition and persecution of Christians fails to make clear how little official persecution there was before the mid-third century A.D. (pp. 189-92).

It is easy to find fault. But the merits of Starr's work earn appreciation. He has achieved lucidity without excessive oversimplification. The problems confronting an ancient historian and the tentativeness of conclusions are emphasized—a feature uncommon in introductory books. As an initial exposure to Roman history for younger students this volume has few peers.

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DONALD R. DUDLEY. *The Romans: 850 B.C.—A.D. 337*. (The History of Human Society.) New York: Alfred A. Knopf. 1970. Pp. xxvi, 316. \$7.95.

This volume in the History of Human Society series has an excellent introductory essay by the editor, J. H. Plumb. At one point Plumb reveals the assignment of the individual authors in the series: "Their aim will be to reconstruct the societies on which they are experts. They will lay bare the structure of their societies—their economic basis, their social organizations, their aspirations, their cultures, their religions, and their conflicts. At the same time they will give a sense of what it was like to have lived in them."

Dudley obviously wrote this book with the general series in mind, but the task is difficult for a Roman historian. The sources for Roman history, particularly the Roman Republic, are essentially political. The ancient Roman historians did not write about "human society," and Dudley knows his Roman historians well, perhaps too well to write the kind of book this series calls for.

Dudley's problem is evident in his treatment of the Republic. The social and economic problems of the Republic are directly related to politics and warfare, and the historian who writes about those problems must either assume that his readers know the political background

or he must summarize it for them. Since the History of Human Society series is not directed to specialists, Dudley decided to summarize. Unfortunately he did not strike a balance between political background and the problems of society at large. There are too many brief chapters on politics. (The chapter on the three Carthaginian wars is five pages long, and the one on Antony, Cleopatra, and Octavian is not quite three full pages.) Marius is introduced as the creator of the professional army, but the reader is not told what happened to him after his victory over the Cimbri and Teutones.

The discussion of the Roman Empire is much more in keeping with the purpose of the series. There is more evidence, particularly archeological, for the history of society in the Roman Empire, and Dudley makes good use of it. There are brief chapters on such topics as "Travel," "Public Careers," "Frontiers," and "Universal Religions." In addition there are two good chapters on the provinces. Although Dudley is probably not critical enough of Christian tradition, his judgment is generally good and his treatment interesting.

Unfortunately almost half of the book is devoted to the Roman Republic and too much of that to unsatisfactory political summary. There are points in the last half of the book where the reader will begin to understand "what it was like to have lived" under Roman rule, but in general the book fails to give a clear picture of Roman society and culture.

There is a good index and bibliography. In the acknowledgements the author says that the illustrations were planned as "an integral part of the book," but some of them are unnecessarily poor, especially the ones of Hadrian's Wall, Masada, and Cosa. The title of the book is somewhat misleading since it does not end with the death of Constantine, as the date suggests, but with the final collapse of the Roman Empire in the West.

ARTHER FERRILL  
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ROBERT E. A. PALMER. *The Archaic Community of the Romans*. New York: Cambridge University Press. 1970. Pp. ix, 328. \$12.50.

Each citizen of republican Rome found himself in the peculiar position of belonging to three

different groups and three different assemblies involved in the functions of government: he was a member of a *curia* (*comitia curiata*), a century (*comitia centuriata*), and a tribe (*comitia tributa*). Of these the oldest and the most important for our knowledge of archaic Rome is the *curia*. Attempting to see through the haze of myth in Livy, Dionysius, and Varro by critical sifting of the evidence and with the help of heortological records, Palmer has given us his interpretation of what the *curiae* were and what role they played in the early government. At times he has let his imagination run too far and has seen *curiae* or suspected their influence in inscriptions where, in fact, they are not mentioned—for example, the Caeretan *curia* is really attested (*CIL* XI, 3593), but the *curator* of *CIL* XI, 3614 is a *curator civitatis*, and there is nothing there to suggest any reference to a *curia*. In general, however, Palmer's work contains much sound reasoning. He is not a slave to traditional scholarly views and is quite ready to break new ground. But his excavations will not always support the weight of his superstructure.

Palmer believes that there were, as tradition says, thirty *curiae*, but that the last three of them were not formed until the very early years of the Republic. He accepts a common etymology for the word (*co-vir-ya*) and defines it as an "assemblage or congregation cooperating by common consent." The Quirites turn out to be the members of the *curiae*. He concludes that the *curiae* were originally the distinct ethnic groups that were brought together in the hill district of Rome, eventually forming the Roman state. The assembly of all the *curiae* largely gave direction or advice to that united state, but it in turn was presided over and controlled by that very king whom it put into power. This curiate government was conservative and was ultimately confronted by a reactionary and progressive movement called the centuriate or Servian constitution. The opposition of the curiate to the centuriate can be seen, Palmer believes, in the power of the curiate assembly to elect the plebeian tribunes and the military tribunes, power the curiate ultimately lost. The new military centuriate system curbed and limited curial religion, eventually winning political success, but down to the Licinian-Sextian Laws of 367 B.C. the two sys-

tems existed side by side. Thus, for almost 150 years these two systems coexisted, the curiate system gradually losing power in all but a few areas. Apart from an occasional vagueness and a certain inability to see the weakness of details that arise from his main thesis, Palmer's interpretation of the curiate system makes good sense. For one thing it illuminates the conditions under which the curiate assembly had acquired the power to confer imperium on the kings and, later, on the higher magistrates.

This is a scholarly work, not easy to read, certain to evoke controversy, and required reading for all those interested in early Rome. But the reader must be careful not to confuse Palmer's theories with established truth.

ROBERT K. SHERK

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Buffalo

STEPHEN BENKO and JOHN J. O'ROURKE [editors]. *The Catacombs and the Colosseum: The Roman Empire as the Setting of Primitive Christianity*. Valley Forge, Pa.: Judson Press. 1971. Pp. 318. \$6.95.

When he composed his *History of the Decline and Fall of the Roman Empire*, Edward Gibbon chose to introduce the Christian Church to his readers only after discussing Diocletian's tetrarchy. After two centuries of scholarly endeavor the Gibbonian approach is still widespread. Today a historian of the Roman Empire is likely to postpone an assessment of the Christian Church until the eve of its fateful alliance with the Roman state. The dangers of this procedure are manifest: despite precautions the Roman historian inevitably glosses over some aspects of the early Church; and just as inevitably his reader fails to understand why the Church was so powerful in late antiquity.

The present volume is a work of church history. Its purpose is to call attention to an analogous sin of church historians, their failure to take full account of the Roman Empire in their study of the early Church. This work consists of a series of essays, most of which were presented at the Philadelphia Seminar on Christian Origins in 1969, on the Roman environment of the Church to A.D. 135. Following an introduction by Robert M. Grant, there are treatises on early imperial politics by Stephen

Benko; the institutions and social structure of the principate by John G. Gager, Clarence L. Lee, John T. Townsend, and John J. O'Rourke; administration in the early Empire by James L. Jones and William White, Jr.; and early imperial religion by Robert A. Kraft, Donald Winslow, Gerhard Krodel, and Robert L. Wilken.

Since this work is intended mostly for the student of the early Church, church historians will be the primary judges of its timeliness and value. But the essays will also be of use to the historian of the Roman Empire, and it is this aspect of the book that I wish to emphasize. The historical merit of these essays is not immediately apparent, for at first glance they seem to offer simply a conventional synthesis of the Julio-Claudian, Flavian, and early Antonine principate. Although some essays—notably those of Gager, Jones, and Wilken—are excellent, no new discoveries are presented. Furthermore, there are numerous small errors of fact and interpretation. In Grant's introduction, for example, Hadrian's favorite Antinous becomes "Antoninus" (p. 18). Grant and Kraft (pp. 17 and 82), influenced by the recently discovered inscription designating Pontius Pilate as [*praefectus Iuda[ea]e*], ignore the fact that after the accession of Claudius provincial governors of equestrian rank generally bore the title of *procurator* (see A. H. M. Jones, *Studies in Roman Government and Law* [Oxford, 1960], 115–25). The Roman historian, then, might view this collection of essays solely as an orthodox piece of early imperial history, but in doing so he would miss a spirit of curiosity that is evident throughout the book—a spirit that the contributors obviously wish to be contagious.

In effect, the results of this work are of less importance than the interest behind them. A group of church historians has compiled some fairly up-to-date information about the Roman environment of the early Church. Might Roman historians conduct a similar investigation of Judaism and Christianity during the early principate? All contributors to the present volume implicitly or explicitly invite a response from the historian. Grant's explicit invitation (p. 24) may serve as a Parthian shot at the Gibbonian approach: "The early history of Christianity is Roman history, and I should

claim that Roman history itself needs the collaboration of those who try to relate the Christian movement to the whole life of the Empire."

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Madison*

EDITH MARY WIGHTMAN. *Roman Trier and the Treveri*. New York: Praeger Publishers. 1970. Pp. 320. \$15.00.

This study is intended as a survey of the historical development of the land of the Treveri under the Roman Empire. The work is divided into seven chapters on the land and the people, the Treveri and Rome, the city of Trier, the countryside, daily life and economics, and religion. Also included is a four-page epilogue on the transition to the Middle Ages, a forty-five page bibliography, twenty-four plates, twenty-five figures, and eight maps. Despite its scholarly appearance the book is intended essentially for the "reader with a general interest in Roman studies." Experts are encouraged to read between the lines because the work is not adequately documented. The use of Latin quotations and the frequent allusions to scholarly controversies, however, seem to project this study beyond the competence of all but the most dedicated amateur.

These drawbacks might discourage the reader of even a great synthesis, but here they are a blessing in disguise since they will keep both experts and amateurs from wasting their time. Chronology, an essential element for the study of both history and development, is largely ignored. It might be charitable to suggest that the archeological record is insufficiently complete at this time to provide the exact chronology necessary for writing history. In economics, the one area where the author seems to have some notion of development, she misses the significance of vital evidence. She maintains, largely on the basis of mosaic evidence, that the Trier area reached its peak in prosperity during the late second and early third centuries. Yet, during the later second century cheap sandstone replaced the more expensive and more durable limestone as the common building material.

Perhaps it is unfair to apply the criteria of



historical criticism to this work. An author whose speculations lead her to attribute causality to "the extravagant Celtic temperament" seems to lack a professional grasp of historical method. This book seems to be an archeological catalog comprised largely of descriptions of artifacts. The section on villas reads like a real estate salesman's brochure. The author's justification for discussing an artifact is frequently its "most interesting" construction or "very interesting" workmanship rather than its value as evidence for historical development.

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Minneapolis

BARRY CUNLIFFE. *Fishbourne: A Roman Palace and Its Garden*. Baltimore: Johns Hopkins Press. 1971. Pp. 228. \$15.00.

Readers of the *Journal of Roman Studies* will recognize the fine ability of British archeologists to reconstruct the Roman presence in Britain from scanty evidence. When the physical remains are abundant, as on the vast site at Fishbourne near Chichester in Sussex, and are meticulously excavated, the reality of Roman Britain may be dramatically restored. Barry Cunliffe, who excavated Fishbourne, 1961-69, and supervised its restoration, has provided the visitor with the experience of a Roman palace and garden, sensitively preserved in a suburban setting largely with the help of volunteers and the local community. Although he has published a lengthy scientific record of the excavations elsewhere (1971), Cunliffe's book, addressed to a broad public, is a model of *haute vulgarisation*, clearly and imaginatively written, well illustrated with good photographs, plans, and reconstructions, and neatly demonstrative of archeological methods and Roman architecture.

The excavator's concern for the topographical implications of the site, its changing occupants, and the architectural history of the palace articulate his skillful presentation. The geographic nature of the site on the edge of a marshy seashore limited activity in pre-Roman times to salt-panning and fishing, jeopardized and complicated building during the Roman occupation, and probably contributed to the abandonment of the place in the fourth cen-

tury. Fishbourne's location on the south coast of England opened it to Roman conquest from Gaul, then made the area important as a secure base for Roman operations in central Britain, and last exposed the place to the barbarian raids that ravaged the coastline. A similar interest in the occupants of the site enlivens Cunliffe's descriptions of the palace during its 250 years of settled inhabitation and culminates in a fine bit of historical detection that offers Tiberius Claudius Cogidubnus, client-king of the area under Claudius and the Flavians, as the probable owner and patron of the palace and gardens at Fishbourne.

It is the palace itself that captures our attention, the largest (about 450 by 500 feet), the costliest (approximately \$3,500,000), and the most lavishly appointed yet discovered in Roman Britain. Although the site was first occupied by Claudian military buildings, later transformed into a proto palace under Nero, the vast palace seems to have been deliberately planned and built in the Flavian period by an architect who adapted Italian models and building practices to local materials, crafts, and climatic conditions. If Cunliffe's account of the adaptive process is fascinating, so, too, is his careful description of the design, construction, and decoration of the palace, of its division into public, administrative, and residential wings, and of the magnificent formal garden (250 by 330 feet), fully enclosed by the architecture that it complemented.

Frequently repaired and remodeled in the second and third centuries, the palace at Fishbourne was destroyed by a great fire ca. 300 A.D., and its building materials were subsequently looted or broken by plowing. But the palace and the life it contained have been vividly reclaimed by the author in his conscientious and entertaining account.

RICHARD BRILLIANT  
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JOHN HOLLAND SMITH. *Constantine the Great*. New York: Charles Scribner's Sons. 1971. Pp. 359. \$8.95.

Constantine the Great has more than one thing in common with the other figure from the ancient world who has merited the same epithet: Alexander III of Macedon. Both started a new

age. Both had an *instinctus divinitatis* about them, although its manifestations were different. Both changed the course of history about as much as individual men can hope to do. And each generation of scholars has found a new Constantine and Alexander the Great. The nineteenth century stripped Constantine of his Christianity and left him a hard-boiled politician. This century has allowed him to be at least sincerely superstitious, a child of his age.

John Holland Smith's Constantine is a compromise figure. He resurrects the view that Constantine's vision before the battle of the Milvian Bridge, if it was anything at all, was the sun high in the atmosphere shining through a veil of ice crystals. Visions in the late Empire are easier to explain by psychology. Like many before him, Smith sees something significant in the fact that Constantine waited until near death before he received baptism. Yet deathbed baptism was a common practice of the time; even Theodosius the Great planned to receive his baptism then, although he miscalculated and recovered from what he thought was his last illness after baptism. There is no point resurrecting this old chestnut.

At the same time, something that "may or may not have been sudden conversion to faith in Christ" happened before the battle of the Milvian Bridge. Smith does not commit himself. There is nothing wrong with suspending judgment, but Smith's compromise Constantine is not quite believable.

Smith generally writes from original sources with disappointingly few references to modern scholarship. There are occasional errors in judgment: he argues, for instance, that Constantine's edicts of 326 concerning sexual irregularities could not refer to the "affair" of Crispus and Fausta, since they do not mention it specifically. However, Smith's *Constantine* is a readable biography, no great contribution to scholarship, but reasonably reliable.

J. A. S. EVANS

McMaster University

## MEDIEVAL

EDWARD PETERS. *The Shadow King: Rex Inutilis in Medieval Law and Literature, 751-1327*. New

Haven: Yale University Press. 1970. Pp. xv, 257. \$10.00.

O, what pity is it/ That he had not so trimmed and dressed this land/ As we this garden! (*Richard II*, III, iv.) Medieval men did not normally think of kings as gardeners. The problem that concerned Shakespeare in *Richard II*, however, was one that had long concerned them as well: the king who was unsuitable to his high dignity. In this volume Mr. Peters has assembled some aspects of the history *rex inutilis* from the deposition of Childeric III to the deposition of Edward II. He has cast his net widely. Childeric, Louis the Pious, Boson of Vienne, Arnulf of Corinthis, and Pepin of Aquitaine—as well as Gregory VII's use of this early deposition tradition—have been caught in the first chapter; Beowulf, the *chansons de geste*, the *Heimskringla*, and the early Arthurian romances in the second; and a hundred years of canonist discussions in the third. From these he moves to the deposition of Sancho II of Portugal by Innocent IV in 1245, later Arthurian literature, the abdication of Celestine V, and, finally, the depositions of Adolf of Nassau and Edward II.

Mr. Peters is at his best when analyzing particular texts. His discussion of the Childeric reference in Gregory VII's letter to bishop Hermann of Metz is one of several that are both perceptive and persuasive. Medieval historians will also be grateful for the extended treatment of Sancho's deposition, an event that has not won the attention it clearly deserves in histories of *regnum* and *sacerdotium* in the Middle Ages.

The collection of so many events and literary traditions between the covers of a book ought to be justified by a synthetic vision that gives them patterned relationships and significance. The present book, however, does not do so. It is a collection of studies on a theme rather than a coherent history. The result is, in places, a curious imbalance, in others a piece without a place. In the chapter on the Arthurian romances, for example, he gives us an elaborate summary of the plots of the Vulgate narratives; in the chapter on canonist deposition theory, however, only the briefest mention is made of legal discussions concerning episcopal *inutilitas*, though this discussion bears directly on the deposition of King Sancho II. In

another chapter there is a lengthy and very interesting discussion of changing theological conceptions of *acedia*. But this is connected with the theme of the chapter in only the vaguest possible way ("the concepts of *acedia* and other vices . . . had contributed to a background against which royal inadequacy might be viewed") and then drops completely from sight. If Mr. Peters is really saying what the structure of his book implies—that the phenomena he discusses are parallel but not connected—it would have been interesting to have his reflections on how the legal, the literary, and the moral-philosophical traditions managed to go their separate ways for so long within a single intellectual culture. As it stands, the book will serve as a collection of well-crafted articles.

FREDRIC CHEYETTE  
Amherst College

IRVING A. AGUS. *The Heroic Age of Franco-German Jewry: The Jews of Germany and France of the Tenth and Eleventh Centuries, Pioneers and Builders of Town-Life, Town-Government and Institutions*. New York: Yeshiva University Press; distrib. by Bloch Publishing Company, New York. 1969. Pp. xi, 380. \$12.00.

There is no question that Professor Agus knows his *Responsa* literature and has a superb understanding of Jewish intramural relations. How can one properly discuss the Jews as "pioneers and builders of town-life," however, when one not only does not discuss the town but also makes no use of recent socioeconomic scholarship that deals with the problems of the period's urban and economic development? Professor Agus's other studies based upon the *Responsa* are the chief authorities for this work: in chapter 1, for example, 60 of 131 footnotes are from others of Agus's works; in chapter 4, the proportion is 150 of 178; in chapter 7, it is 59 of 62, while in the crucial last chapter (11, "Relationship between Jews and Non-Jews") it is 56 of 65 footnotes.

Because Agus pays scant attention to non-Jewish material, he overstates his position. He speaks of "The Heroic Age," and the "pioneers," yet I found neither heroes nor pioneers in this work. While it is true that Jews did make the dangerous trip to the East (as did

some Christian pilgrims), most of the Jews of whom he writes lived in the established communities west of the Rhine. Mainz seems to be the eastern outpost in Germany. The author states that "the Jews were the first self-ruling town dwellers of Western Europe in the early Middle Ages. In many places northwest of the Alps where a town sprung up, it thus developed around a nucleus of a group of Jewish families organized as a community." Not only is no evidence cited for this, but when Agus does discuss Jewish resettlement it is in terms of being invited into an established community.

The author briefly quotes August Kluckhohn (1857) and James Westfall Thompson (1928) as "modern scholars" describing life in the tenth and eleventh centuries. Life during that period was indeed difficult, but to conclude that Europe was "war-torn, constantly feuding, robber infested, and completely lawless" is an overstatement that modern (later than 1928) scholarship would not support.

The late Professor Cecil Roth once warned against neglecting the "hidden Jews," that is, the poor Jew out in the countryside. Professor Agus denies their existence. For him the source of the Franco-German Jew (and for the entire East European Jewry) was about 5,000 (or 1,000 families) rich, prosperous, landowning merchants with a "tremendous dedication to the study and practice of Judaism." Nowhere is this proven.

There is another major problem. The book is impersonal and general. The reader is constantly faced by a lack of specific names, dates, and places: "The Queen of Hungary relied on two Jews. . . . She often sent one of them on important missions." Which queen? When? Where? The footnote in this case cites Agus's two-volume *Urban Civilization* (p. 232). The reader learns that Jew A did this, that, or the other; but where or when is not stated. It would seem as though time and place had no meaning for Jewish life.

There are some minor problems: what, for example, is R. Glaber's first name? There is a choice: Rudolfus, Ralph, or Raoul. Footnote 1 of chapter 3 (p. 96) starts out "See *ibid.*, pp. 99-100, where," and ends, "See *ibid.*, p. 101, note C." I presume the *ibid.* refers to his two-

volume *Urban Civilization*, cited in footnote 107 (p. 77) of chapter 2.

Agus's work is excellent as a study of Jewish life, and it is also helpful as a supplement for European economic development (the sections on the Jew as a borrower of Christian capital, and on the purchasing power of a pound of silver are significant), but when Agus writes of the non-Jewish world or when he begins to compare and contrast, let the reader beware.

J. LEE SHNEIDMAN  
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WOLFGANG VON STROMER. *Oberdeutsche Hochfinanz, 1350-1450*. In three volumes. (*Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, Beihefte, Numbers 55-57.) Wiesbaden: Franz Steiner Verlag GmbH. 1970. Pp. xxi, 218; 220-460; 463-608. DM 30; DM 30; DM 36.

URSULA SCHWARZKOPF. *Die Rechnungslegung des Humbert de Plaine über die Jahre 1448 bis 1452: Eine Studie zur Amtsführung des burgundischen Maître de la Chambre aux Deniers*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, Number 23.) Göttingen: Vandenhoeck & Ruprecht. 1970. Pp. 217. DM 26.

These three numbers of the *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte* are designed to be bound together as a single volume of some six hundred pages. The first traces the outlines of a general commercial-financial history centering on the city of Nuremberg from the mid-fourteenth to the mid-fifteenth centuries. There is little that is remarkably new in this picture of south German relations with the Hanse in general and with portions of Flanders in particular, with Lombardy (particularly with Milan and Venice), and with Austria-Hungary and the trans-Carpathian region. The great value of this part of von Stromer's work lies in his extensive research in public and private archives, his confirmation of many commercial documents used by earlier scholars, and his discovery of numerous additional documents not utilized by economic historians before this time. The result is to fill out much that has been mainly conjectural heretofore.

The second portion of von Stromer's work is more original and will probably need to be modified somewhat when future scholars have investigated the sources from a different point of view. Here the author is concerned with the

relationship between Nuremberg financiers and the policies (especially the Italian policies) of the imperial Luxembourg dynasty. Undoubtedly the availability of extensive credit as well as financial expertise allowed the members of the Luxembourg dynasty to act with greater confidence and more ambition than would otherwise have been true—and perhaps the ease and availability of such financing contributed to their failure to take a sufficiently realistic view of the peculiar problems of the medieval empire.

The last section of this work is filled with transcriptions of original documents, tables, bibliography, and indexes. Von Stromer's bibliography, even though presented in curiously abbreviated form, is excellent. The various indexes, covering the individual members of mercantile families as well as the great houses and places mentioned in the documents, are most useful. Only the inflated price at which this work sells will detract from its general scholarly appeal.

It is more difficult to fit Professor Schwarzkopf's monograph into the general theme of economic history. Her work is more narrowly the study of the accounts kept by one man, Humbert de Plaine, during a brief period (1448-52) while he exercised financial control at the Burgundian court of the duchess, Isabella of Portugal, and was also master of accounts for her son, Charles the Bold, Count of Charolais. As such, this is a careful study of accounting methods developed in the Burgundian territories and practiced by Humbert de Plaine; but at the same time it offers something a little broader—a study of the social and economic place held by members of those families who had devoted themselves for several generations to mercantile and financial activities. Quite obviously the original humble beginnings of the Burgundian mercantile families did not prevent their entering the ducal service and being rewarded for their services by grants that might bring them into the lower ranks of the feudal aristocracy.

For the student interested in the development of accounting procedures as well as for the social historian interested in the activities and products for which greater or lesser sums of money might be spent in the Burgundian

territories while that conglomerate duchy was at the peak of its prosperity and power, this will be an extremely useful work.

K. F. DREW  
Rice University

JOSEPH RATZINGER. *The Theology of History in St. Bonaventure*. [Chicago:] Franciscan Herald Press. 1971. Pp. xv, 268. \$12.50.

This book, a translation by Zachary Hayes of *Die Geschichtstheologie des Heiligen Bonaventura* (1959), is unchanged from the original, as Ratzinger comments that the reviews of his work did not substantially alter his basic thesis, except for some modification of his views of the dependence of Bonaventure on Joachim of Fiore. He analyzes the historical views of "the prince of mystics" through his university sermons delivered at Paris in 1273. These *Collationes in Hexaemeron* were mined in an attempt to discover how the theology of history had changed from the great static concepts of the Church fathers like Augustine to the new dynamic views of the Joachimites.

Accepting much from Joachim's *Concordia Veteris et Novi Testamenti*, Bonaventure was led to agree that "new knowledge arises constantly from Scripture; and this happening, this history, continues onward as long as there is any history at all." Scriptural exegesis clarifies the past and in turn permits prophecy. As Christ is the center of the ages and as all is determined by "that same divine order which is the unifying law of all reality," a theology of history becomes imperative, seeking not only the "meaningfulness of events" but the "eventfulness of meaning." Chaos, randomness, and caprice are thus ruled out of the main stream of human history.

Bonaventure found the source of his inspiration through the progressive reinterpretation of Scripture and from a mystical attempt to ascertain the inner meaning of the Old and New Testaments and their relationship to each other. The Middle Ages were, like most ages, full of change and controversy. The battle raged between the views and conclusions of an Aquinas and those of the advanced mystics. Somewhere in between was the "orthodox" mysticism of Bonaventure. If allegory and numerology, doctrines of six or seven ages of man,

the great chain of being, and the identification of Francis of Assisi with Elijah and John the Baptist cannot be taken for granted by many of today's pragmatic historians, Dr. Ratzinger has nevertheless done all a great service by producing a clearly worked out map of part of the mind of a great medieval thinker.

ALLEN D. BRECK  
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ANTHONY BLACK. *Monarchy and Community: Political Ideas in the Later Conciliar Controversy, 1430-1450*. (Cambridge Studies in Medieval Life and Thought, Third Series, Number 2.) New York: Cambridge University Press. 1970. Pp. xii, 189. \$10.50.

From the thirteenth century on, the relationship between the head and members of bodies politic was a recurrent issue in the political life of European societies, and each such struggle for authority occasioned new efforts to define in theory the relation of the ruler to his community. This book, true to its title, examines the theories advanced for and against the claims of the Council of Basel (1431-49) to be the supreme authority in the Church. The work is admirably organized as three concise essays.

Part 1 delineates the views of the Baslean Conciliarists, among whom John of Segovia emerges as both the typical and leading thinker. At Constance the Conciliarists had claimed that the council was in some special cases superior to the pope; most notably the Basleans generalized this claim into superiority in every respect. Among Segovia's political ideas the most remarkable is "the concept of trust," which measures the injustice of government by, as it were, the credibility gap. Thus any *presidens* who acts contrary to the intention of his people, or the majority of that *multitudo*, has in fact lost the trust (*credulitas*) that for Segovia was the basis of authority. Such an evil president is deposed *ipso facto* because, once he has discredited himself by opposing his own private judgment to the will of the majority as duly expressed in a representative assembly, he can no longer be presumed to be a public person who represents the common will.

Part 2 treats the apologists employed by Pope Eugenius IV (1431-47), whose doctrines

are accordingly dubbed "Eugenian Papalism." Led by Torquemada's uncle John (alias Turrecremata), they founded the necessity for all monarchies on a Neoplatonic natural theology in which all earthly authority emanates from God, the supreme hierarch. One wonders whether in this view temporal power descended to kings from the papacy, but presumably the papalists, bidding for royal support, tactfully left that implication carefully folded up.

These two essays may themselves be characterized as an emanation from Walter Ullmann, whose distinctive style of scholarship they eminently reflect. Judged by Ullmannesque standards Antony Black is largely successful, for he achieves a highly selective synthesis, broadly conceived and executed with praiseworthy clarity and concision. But since nonspecialists especially will be attracted by this masterful manner, it is only fair to advise the reader that Black's grasp on scholastic thought before 1350 is not as broad and firm as his frequent bold generalizations might lead one to suppose. His errors, however, are for the most part those *peccata proficientium* that experienced scholars should tolerate in a first work displaying great talent and much effort.

For example, Black overestimates the originality of his Basleans because he does not recognize their debt to Marsiglio of Padua, who had concluded that only a general council has the authority to designate and depose the pope (*Defensor pacis* 3. 2. 32). Concerning deposition we are told instead that "what Marsiglio [*sic*] had already said of the small state, Conciliarism applied to a larger community" (p. 44). Still worse, a Baslean doctrine in which Turrecremata himself discerned Marsilian influence "more probably had a common source in civic and other associations" (p. 54).

The third part, in contrast to the first two, marks a welcome descent from those lofty realms of abstracted theory where we too often make it appear that theories were framed solely for their own sake rather than in response to particular historical circumstances. With a political scientist's keen sense of the interplay of theory and practice, Black shows how, as propagandists and diplomats, the theorists on both sides developed, deployed, and artfully adapted

their distinctive themes in the course of two decades of political maneuver.

One can only hope that Black will not rest content to be a brilliant essayist but will now do for Segovia what Francis Oakley, whose interests resemble his own, has done so solidly for Pierre d'Ailly.

RICHARD KAY

University of Kansas

JANE E. SAYERS. *Papal Judges Delegate in the Province of Canterbury, 1198-1254: A Study in Ecclesiastical Jurisdiction and Administration*. (Oxford Historical Monographs.) New York: Oxford University Press. 1971. Pp. xxv, 398. \$17.75.

The use of local ecclesiastics as judges delegate was an innovation in the twelfth century that made possible an effective papal jurisdiction over provinces as distant as Canterbury. The legal theory and something of the practice in certain *causes célèbres* have been fairly well known, but evidence for the complete functioning of the system of judges delegate has remained buried in scattered ecclesiastical archives. The author has completed the tedious but important task of ferreting out the cases and abstracting the administrative procedure and has done it well.

The main strength of the book lies in the description of procedure and of the types of cases. In fact, the principal use for the book other than by those persons specifically interested in the subject is likely to be as a handbook to guide other medievalists who are faced with the problem of coping with documents from cases before judges delegate that they may encounter in the course of related researches. In an appendix there are specimens of the various types of documents produced at stages in the procedure that can well serve as comparisons.

As a sometime practitioner of administrative history, I may be permitted the comment that one of the weaknesses of the genre is that the writing often becomes a summary of case after case and instance after instance. Miss Sayers's work is no exception, with entire chapters pounding relentlessly on as a slightly expanded and heavily documented outline. The reward from this meticulous scholarship is only partially forthcoming, for the author has a marked reluctance to develop general conclusions. Her

two pages of formal conclusions, even when supplemented with some general remarks in the introductory pages, can do little more than suggest some of the broader questions on which her views would have been valuable.

It is true that F. W. Maitland and Z. N. Brooke showed that England was an integral part of the ecclesiastical jurisdiction of Rome, but how did the use of judges delegate contribute to the growing power of the papacy? Although mentioned, the parallel appointment of papal legates is also not fully explored in relation to judges delegate. Most of all, the book lacks a sense of development or emphasis on trends that would allow a reader to understand why Archbishop Pecham in 1284 called appeals to Rome one of the major abuses of the Church.

The author's opinion that ecclesiastical courts drew cases away from the secular courts is startling. If this is so, the situation had changed since the days when Ranulf Glanville twitted Walter Map about the inefficiency of ecclesiastical justice. The major disappointment is that so little emphasis is placed on development that the book would not provide an answer either way on this question of efficiency, even though the question is central to an evaluation of the overall effect of the practice of using judges delegate.

CHARLES R. YOUNG  
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J. A. WATT. *The Church and the Two Nations in Medieval Ireland*. (Cambridge Studies in Medieval Life and Thought, Third Series, Volume 3.) New York: Cambridge University Press. 1970. Pp. xvi, 251. \$13.50.

This solid volume deserves to take its distinguished place with other scholarly monographs in the Cambridge Studies in Medieval Life and Thought. The author handles his difficult subject, the Church in Ireland through the twelfth, thirteenth, and fourteenth centuries, with detachment and reserve—virtues that other scholars who have written about that troubled island have not always possessed. What renders the practice of these virtues imperative here is, first, the sensitivity of the subject, and, second, the care with which the limited and frequently uncertain documentary evidence must be examined. So we find the au-

thor commiserating with himself over his task: "There is no more difficult problem in medieval ecclesiastical history than to know the mind of an Irish bishop" (p. 147).

The reader is inclined to agree with Dr. Watt that Henry II's invasion of Ireland was "the most important event in the history of the Irish Church between the fifth and sixteenth centuries" (p. 40). The author also makes a strong case for his view that the Church in Ireland, which he believes may have been on the threshold of beginning its own reform, was destined because of that invasion to remain throughout the Middle Ages "a remote and backward province of the Church, stunted in its growth, distorted in its development" (p. 2).

What blighted the expectations of the papacy—which approved English *dominium* of the island—and of the Irish hierarchy, which appears initially to have acquiesced in that rule, was a dormant nativism that the intrusion of the foreigner caused to surface. So acute did this antagonism between Irish and English grow that religious orders as dedicated to the spiritual ideal as the Cistercians and Franciscans found it impossible to accommodate both Irish and English in the same communities. The Crown, whose principal concern was political control, did not help matters by applying the principles of common law as they touched the clergy and by demanding the same voice in the selection of bishops in Ireland as it enjoyed at home. What might have proved a stronger unifying force than a common religion was royal taxation, and had Edward I and his successors pressed their fiscal demands with greater harshness and persistence, time might have served to dull the sharp animosities that divided the Irish, the Anglo-Irish, and the English clergy.

Although Dr. Watt leaves the reader with a bleak picture of the Church in Ireland, he introduces on the way a number of fascinating Irish churchmen whom, one hopes, he and other scholars will shortly bring into fuller view.

JOSEPH DAHMUS  
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RÉGINE PERNOUD. *Héloïse et Abélard*. Paris: Éditions Albin Michel. 1970. Pp. 298. 19.50 fr.

This is an extremely fine book that is well

worth reading on several counts. Using the story of Héloïse and Abelard as her focus, Miss Pernoud has recreated much of the complexity and diversity of the thought world of the twelfth century. In her technique she belongs to the family of historians that includes Christopher Brooke; she is steeped in the milieu she describes, and because it clearly has an immediacy for her she is able to convey her sense of the period vividly to the reader.

Abelard's education, the full range of his scholarly development, and his philosophical ideas are presented very lucidly; but rather than seem like old ground gone over again, Pernoud catches the intellectual excitement of the period. Abelard's passion for dialectic becomes comprehensible in human terms, though humanizing Abelard does not in any sense detract from Pernoud's portrait of him as the greatest genius of his era. She seems, in fact, to agree with Abelard's assessment of himself—an assessment that should probably be slightly modified. But the key to his personality and character, as Pernoud presents it, lies in the fact that his extraordinary intellectual drive was the main force in his life. Seen in this way, his love for Héloïse was basically his response to her unusual intellectual gifts.

The portrait of Héloïse is beautifully drawn and the love story is told very much from her point of view. An interesting and tragic tension existed between Héloïse and Abelard because she was passionately in love with him but able to perceive his shortcomings as an emotional being. Pernoud compares her to Simone de Beauvoir and sees a parallel between de Beauvoir's reasons for not wanting to marry Sartre—her reluctance to see him become "professeur en province et, définitivement, un adulte"—and Héloïse's wish not to marry Abelard. But Héloïse still belongs to the twelfth century, and her devotion to Abelard and the way she saw their love was colored in good measure by the ideals of courtly love. She was a woman of real spirit—both literally and figuratively.

I consider it appropriate (and probably not an accident) that a woman was asked to review this book, because I can say with confidence that in it Héloïse really has her day.

JILL N. CLASTER  
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PETER LLEWELLYN, *Rome in the Dark Ages*. New York: Praeger Publishers, 1971. Pp. 324. \$10.00.

In this brief account Rome refers primarily to the capital city, and the dark ages encompass the centuries between 476 and 962, from the days of King Theodoric the Ostrogoth to the coronation of Emperor Otto I. Though Italy and the Empire come under discussion, the argument basically focuses upon the continuity and the eventual victory of papal Rome. The book is well written, and the author intersperses his narrative with colorful translations from the sources; however, the volume is compact, replete with unfamiliar names, and not easily read.

After a sympathetic account of Theodoric, the author relates the reconquest of Italy by the Byzantine forces of Belisarius and Narses. It is an exciting account that arouses sympathy and admiration for the defeated Ostrogoths and disdain for the Byzantines and Romans. The reconquest brought no great relief to Rome and Italy; instead Franks, Lombards, plague and floods, and the inept administration of many Byzantine officials decimated the population and impoverished the survivors. In perilous times Pope Gregory I assumed the political and spiritual leadership of Rome and much of the peninsula. Not until the eighth century did the Roman popes rise high above the machinations of Byzantines, local dukes, and archbishops. This chapter, "Rome and the Byzantine Empire," is very convincing. In "Rome and the Pilgrims" the author very ingeniously uses the pilgrims to indicate the architectural changes from imperial to papal Rome and likewise to extend Roman ideas and practices northward and westward.

The victory over the Byzantines left only the Lombards and the Roman noble factions. The struggle involved intrigue, treachery, and murder; in the end the Frankish support against the Lombards saved Rome and papal authority. These successes did not always guarantee papal control in Rome itself where political factions resorted to the most nefarious activities to dominate the papal office, and they often succeeded. Even the close alliance between Leo III and Charlemagne and the imperial coronation of that monarch did not eliminate completely the factional threat. In fact the



deterioration of Carolingian authority permitted the return of factional control that brought disaster, ignominy, and the intervention of Otto I in 961.

As a political, diplomatic, and military narrative the volume is convincing. I hope that the author will offer a similar account in the pastoral, social, and economic sphere to complete his arguments.

HILMAR C. KRUEGER

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GABRIELLA ROSSETTI. *Società e istituzioni nel contado lombardo durante il Medioevo: Cologno Monzese*. Volume 1, *Secoli VIII-X*. (Archivio della Fondazione Italiana per la Storia Amministrativa. First Series, Monografie, ricerche ausiliarie, opere strumentali, Number 9.) Milan: Dott. Antonino Giuffrè. 1968. Pp. 242.

Miss Rossetti has presented us with a mass of detail concerning the history of Cologno Monzese and its environs, a rural area of the Milanese contado, during the eighth through the tenth centuries. It requires some patience to wade through a cluttered presentation, but Miss Rossetti has drawn from the welter of place and proper names (to which an index is mercifully provided) certain conclusions of interest to the economic and social historian.

Leaving aside the subordinate arguments and myriad problems of interpretation, the one recurring theme of the work is the economic penetration of Cologno by the Milanese monastery of Saint Ambrose. Although Cologno remained socially Lombard throughout the period under consideration, by the ninth century Lombard landowners had fallen upon hard times. The division of the patrimony among male heirs had resulted in breaking up the relatively large unitary familial holdings of the preceding century into small and economically marginal farms. The coming of the Franks and feudalism further aggravated the economic situation of the Lombard population of Cologno since the Franks occupied positions of power and burdened the land with a variety of dues. Into this situation stepped the abbots who pursued, from the mid-ninth century, a seemingly continuous policy in Cologno of putting together unified blocks of land. They secured land both by donation and foreclosure, but they apparently relied most heavily upon ex-

change as the means to their end. In exchange for ground contiguous to their existing holdings in Cologno and elsewhere in the contado, the abbots traded ground in Milan and sites in the castle of Cologno, which had been built under their aegis and jurisdiction in the mid-tenth century. This policy drew a scattered Lombard population to the castle, which subsequently acted as a social and economic focal point for the region. At the same time it stimulated immigration from Cologno to Milan as Colognese exchanged patrimonial parcels for sites in the city. Thus, those able to immigrate were from the relatively well-to-do class of landowners who, when in the city, turned to ecclesiastical or legal careers as entrees into urban life.

Such a picture of monastic penetration of Cologno and its consequences overlooks a number of important questions. For example, how were these acquired lands exploited? Were they leased or worked directly within the larger framework of monastic holdings? Furthermore, the subject is slighted of emigration from the city in the form of investment as urban merchants, ecclesiastics, and others purchased rural land. Miss Rossetti relegates this important aspect of the history of Cologno to a footnote, although she promises to take it up in the sequel. Still, we know from Professor Cinzio Violante's study, *La società milanese nell'età precomunale* (1953), that of the names of the holders of adjacent properties mentioned in the Colognese documents of the beginning of the eleventh century, twenty-eight were lay and but twenty-one ecclesiastic. In any event, should not this counterflow from city to country have been examined within the context of the present volume and thus counterbalance the impression of an all-conquering monastery gobbling up the lands of a depressed peasantry in Cologno Monzese?

Miss Rossetti promises a second volume. We may look forward to this with the hope that she will undertake a greater effort at synthesis and that she will cast her results in a less bewildering form.

THOMAS W. BLOMQUIST

*Northern Illinois University*

ALFRED HAVERKAMP. *Herrschaftsformen der Frühstaufer in Reichsitalien*. Volumes 1 and 2.

many sources and a careful cataloging of diverse applications of the term, it is not entirely satisfactory. Wehlen fails to convey any general sense of what *res publica* meant to ninth-century men concerned with the nature of the state largely because he refuses to inject himself into the discussion as interpreter, summarizer, and synthesizer.

I therefore proceeded to the treatment of Nithard and Paschasius Radbertus without a firm sense of the ideological context in which they operated. Wehlen's analysis of his two documents is more rewarding. He carefully sorts out the uses made by each author of the concept *res publica* and explores in precise terms the sense they gave to the term. His investigations will provide students of Carolingian political theory with considerable help in identifying and defining certain elements of the vocabulary of political thought and with valuable insights into the major concerns of those interested in the ninth-century state. In a larger sense, Wehlen establishes that both historians found in the term *res publica* a "programmatischer Begriff," an ideal against which they could evaluate the political difficulties of the era stretching from about 830 to 843. Wehlen's study has the added advantage of being based on two documents that deal with actual historical events, thereby permitting the reader to see how at least two Carolingian writers applied theoretical concepts to concrete situations.

The work, however, has serious limitations. Wehlen's technique of piling quotation upon quotation, apparently based on the assumption that the sources speak for themselves, is not conducive to a clear understanding of the ideas behind these quotations. The task he sets for himself demands a greater attention to analysis and explication. Nithard and Paschasius Radbertus stand isolated in their age because of Wehlen's failure to relate their ideas effectively to those of their predecessors and contemporaries. Given the fact that both were partisans and apologists (Nithard for the cause of Charles the Bald and Paschasius Radbertus for the "Einheitspartei" and Lothair), one is never certain whether either is seriously concerned with the nature of the state in a theoretical sense. Wehlen's fixation on their use of the concept *res publica* delimits any concern with a larger view of the political consciousness of his authors. Fi-

nally, the study does not effectively establish the critical point that *res publica* was a fundamental concept in the political ideology of the early ninth century. At least I felt that the perspective of the study was too narrow to permit a fuller understanding of what the title promised: "Staatsauffassung im Zeitalter Ludwigs Frommen."

RICHARD E. SULLIVAN

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KURT-ULRICH JÄSCHKE, *Die älteste Halberstädter Bischofschronik*. (Mitteldeutsche Forschungen, Volume 62, Part 1. Untersuchungen zu mitteldeutschen Geschichtsquellen des hohen Mittelalters, Volume 1.) Cologne: Böhlau Verlag. 1970. Pp. vii, 238. DM 54.

Some twenty years ago Helmut Beumann, an intellectual historian at the University of Marburg with a special interest in medieval historiography, wrote a brilliant interpretative study of Widukind of Corvey, which, alas, went unnoticed in these pages. He is now editing some studies on the historians of central Germany during the High Middle Ages, of which the volume under review is the first. This book by Kurt-Ulrich Jäschke is a painstaking attempt to determine content and structure of earlier, nonextant versions of the chronicle of Halberstadt, a work of the thirteenth century. Beyond that it seeks to establish whether the growth of Church organization in central Germany during the later tenth century found an echo in the historiography of the period. The author rejects the assumption shared by an older school of historians that Thietmar of Merseburg (d. 1018) had been the first to write history in central Germany and that his work was the source for all subsequent historiography in that region. He also refutes a more recent thesis of Bernhard Schmeidler, who claimed that nobody wrote history in such ecclesiastical centers of the German frontier as Magdeburg, Nienburg, and Halberstadt before the twelfth century. In an amazing application of the great-man theory, Schmeidler made Abbot Arnold of Nienburg and Berge, who lived during the middle of the twelfth century, the first historian in this area and the author of an array of local annals and chronicles. Instead Jäschke returns to the view that the origins of these works are diverse and complex.

More specifically he advances the thesis that an influential prototype of the episcopal chronicles of Halberstadt was composed as early as 992 or 996.

The historiographical revisions proposed in this meticulous study may seem less earthshaking to a reader on this side of the Atlantic than to a German medievalist not so many miles from the scene; one might even feel, and wrongly I believe, that here a mountain of rigorous medievalism has given birth to a revisionist mouse. True, there is an air of unreality about discussing authorship, motive, impact, or popularity of works whose being and nature are postulated on the fragile foundation of deduction and hypothesis. Yet from a methodological perspective Jäschke's closely reasoned and heavily annotated book cannot be faulted: the microscopic examination of the texts, the cautious establishing of complex textual interdependencies, and the exhaustive recourse to every conceivable clue, especially charter evidence and cursus, make this an important work, though difficult to read.

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GEORGES and DÈMÈTRIOS TORNIKÈS. *Lettres et discours*. Introduction, text, analyses, translation and notes by JEAN DARROUZÈS. (Le monde byzantin.) Paris: Éditions du Centre National de la Recherche Scientifique. 1970. Pp. 381. 86 fr.

This is a useful book, useful not only for the texts it offers, but also for the prosopographical information it contains. The brothers Tornikes were Byzantine functionaries, and to a degree also intellectuals, who lived in the twelfth century. George, the eldest, made his career in the Church, becoming eventually bishop of Ephesus. This is the first complete edition of his letters and discourses. Demetrios, George's younger brother, was a functionary of the state who achieved the position of logothete during the reign of Isaac II Angelus. There are three letters by him included in the present book.

The main body of the book consists of the Greek texts and a summary in French of their contents. Four texts, however, George's long encomium of Anna Comnena, his letter to the Pope, and two other letters to the Pope by Demetrios, are accompanied by translations in full. There are some notes, but more important

is the long introduction where such problems as the careers of the authors, the chronology of the composition of the texts, and their manuscript tradition are treated and where a detailed analysis of the family background of the men to whom the letters were addressed is given.

Byzantine epistolography is highly rhetorical, difficult to read, and very often barren of any concrete information. There may be a few exceptions to this generalization, but the correspondence of the Tornike can hardly be one of them. Nevertheless, we do learn something: the decadent state of the city of Ephesus; the ruinous conditions of its famous church of St. John the Theologian; the reluctance of ecclesiastics, domiciled in Constantinople to accept provincial bishoprics; the differences that separated the Greek and Roman Churches. We learn something also about Anna Comnena, perhaps the greatest woman intellectual of the Middle Ages, both East and West. The encomium of Anna by George Tornikes is no doubt the most important text offered by the present book. Composed several years after her death, it describes at length her intelligence, education, intellectual activities, and other experiences. An encomium can hardly be said to be objective; at best it tells only one side of the story. In this encomium, for instance, the author passes over in silence the political ambition of Anna, an ambition so intense that it involved her in plots against her emperor brother. Nevertheless for the study of the life of that remarkable Byzantine woman and of the intellectual currents in Byzantium during the first half of the twelfth century, this encomium, despite its rhetorical declamations, is invaluable.

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#### MODERN EUROPE

PETER G. BIETENHOLZ. *Basle and France in the Sixteenth Century: The Basle Humanists and Printers in Their Contacts with Francophone Culture*. [Toronto:] University of Toronto Press. 1971. Pp. 367. \$12.50.

This survey complements Bietenholz's previous work on Italian humanism and the Basel print-

ing industry. Its goals are limited: Bietenholz begins by assuring that no extant studies "will be duplicated, and none will be made obsolete" by his book (p. 19), and concludes that he will "stop well short of a complete survey" of Franco-Balois relations (p. 242). Furthermore this volume will be followed by another that has been set aside for an analysis of books and for a number of lengthy *pièces justificatives*. His volumes attempt to see how far the culture of Erasmian Basel and its influence formed a "recognizable factor in the composite tissue of the sixteenth-century French mind" (p. 20). His primary mode of approach is a meticulous examination of Francophone participation in Basel's printing industry from the 1470s until 1650.

The result is an interesting but uneven compilation in which the whole never quite equals the sum of its parts; the synthesis has been deferred for his second volume. Bietenholz has quantified his subject excellently in his first part, "France and Printing at Basle," especially on pages 51-53 (one wishes that the divisions by subject matter were not buried in a footnote). The connections between France and Basel reached a visible peak between 1520 and 1555, the age of Erasmus and of his Balois epigoni, most attractively represented by the Francophone exile Castellio. This section is supplemented by a valuable short-title bibliography (pp. 249-336) that lists the thousand-plus titles on which his study rests. While some of these entries make one wonder about Bietenholz's definitions (for example, why were Jakob Sturm, George Buchanan, Peter Lombard, or Johannes of Segovia "Francophones"?), the principal French authors published at Basel emerge clearly: Castellio leads with 55 entries, followed by Ramus (35), Gilbert Cousin (33), and François Hotman (31). And sometimes the absences are equally revealing: Bodin has only three entries and Calvin five (including of course the original *Institutes*).

Bietenholz's two other sections are uneven. "Francophone Expatriates at Basle" succeeds very well when discussing the French church, Castellio, Baudouin, or Ramus, while "The Focal Contacts in France" succeeds best with Francis I's court or the post-1550 *politiques*. Many other sections, however, are a largely unconnected, discontinuous series of notes, from

which only a few memorable vignettes, like the portrait of the Bauhins (pp. 63f), emerge.

E. WILLIAM MONTER

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DAVID C. STEINMETZ. *Reformers in the Wings*. Philadelphia: Fortress Press. 1971. Pp. viii, 240. \$8.50.

Most studies of the Reformation, whether written by sympathetic or hostile historians, focus on the major Reformers such as Erasmus, Luther, Müntzer, Zwingli, or Calvin. It requires originality to devote an entire book to the second line of reformers, "the reformers in the wings, the supporting members of the cast who by their lesser and often unnoticed activity furthered the course of the drama to its final curtain." Such a study is facilitated by the fact that some of these reformers were powerful thinkers in their own right. Reading this book, one wonders why it was that the early sixteenth century produced such an amazing number of original theologians.

Devoting an essay of eight to twelve pages to each of the reformers, Mr. Steinmetz presents his study in an attractive and lucid form. Altogether twenty reformers are presented, arranged in four major groupings: late medieval Catholic Reform (Geiler, Staupitz, Contarini, Stapulensis, and Pole), the Lutheran tradition (Melancthon, Bugenhagen, Osiander, Amsdorf, and Brenz), the Reformed tradition (Bucer, Bullinger, Hooper, Vermigli, and Beza), and the radical reformers (Carlstadt, Schwenckfeld, Hubmaier, Denck, and Marpeck). Although the reformers discussed are men of very diverse tendencies, the author skillfully avoided the danger of publishing only a collection of disparate essays. Briefly sketching the life and actual work of these men, Mr. Steinmetz, a theologian, concentrates on the theological issues, thus providing a connecting link between the essays. Indeed, the essays offered him the opportunity to introduce practically all significant issues of sixteenth-century theology: the Nominalist background; the problem of the literal and spiritual interpretation of the Scripture; the relationship between Old and New Testaments; the covenant theology; and the various conflicting doctrines on justification, predestination and free will,

the Lord's Supper, the teaching office of the church, and the relationship between Church and state. Although he bases most of his essays on secondary works, Mr. Steinmetz does use the writings of Staupitz, Melancthon, Carlstadt, Schwenckfeld, and Hubmaier. A useful bibliography follows each essay.

As Mr. Steinmetz's book offers an overall study of a variety of colorful personalities and complex theological topics, it may be used to great advantage in courses on Reformation history. Most textbooks mention these reformers only in passing. On the other hand, students generally do not have the time to read the voluminous works that have been written on these reformers. Mr. Steinmetz's readable and attractive book will fill a definite need. This is not to say that it is without minor flaws. I do not understand why the essays on Geiler and Bugenhagen were included, for they do not tell much about either man. Should these essays have been added to the four sections only for the sake of symmetry?

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W. FRED GRAHAM. *The Constructive Revolutionary: John Calvin & His Socio-Economic Impact*. Richmond, Va.: John Knox Press. 1971. Pp. 251. \$7.95.

Professor Graham presents an interpretation of John Calvin as a revolutionary leader and thinker, perfectly aware of the social and economic implications of his own teachings and deeply involved in the problems of the beleaguered Genevan republic. To the accepted portrait of Calvin as chief architect of a theology that was revolutionary in its own time, Dr. Graham adds and explores the perspective of Calvin as an activist, and a secular, revolutionary leader. While the author sees the origin of Calvin's social views in the reformer's own theology, it is the non-nihilist revolutionary who "wanted to uproot evil and usher in a new age based on the old, without destroying society" that is emphasized. Searching the records of Geneva's municipal councils, the Venerable Company of Pastors, Calvin's works, and a host of relevant secondary sources, Professor Graham has effectively documented most of his as-

sertions. Calvin's struggles with money lenders, merchants, grain speculators, city bureaucrats, the cost of living, wage and price controls, education, food and medical care for the poor, and the staggering influx of refugees are presented with commendable clarity and brevity; the portrait of the involved activist is indelibly drawn. But Professor Graham is consistently judicious, and Calvin's shortcomings are weighed with the rest. He does not hesitate to describe Calvin's nearly merciless repression of anyone he considered a threat to the internal discipline of the community; in fact the author suggests the theological origins of this behavior in Calvin's Christology. It is a remarkably comprehensive and convincing case for so brief a volume. The perennial Weber thesis controversy is treated with unusual accuracy, sophistication, and originality; but some of the author's other suggestions are perhaps less likely to carry his readers along. Probably the most fertile sources of comment will be such assertions as "history discloses how men make their own future," and Calvin's career is an example of "how revolutionary leaders have helped man in the past." But Dr. Graham is a capable advocate, and a succession of potentially obscure topics are skillfully dealt with in eminently readable prose. The handsome dust jacket, however, is an inadequate substitute for a clear black and white map or schematic view of Calvin's Geneva. For the general reader as well as the scholar this volume merits wide circulation.

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*Studia Leibnitiana: Supplementa*. Volume 4, *Theologie; Ethik; Pädagogik; Ästhetik; Geschichte; Politik; Recht*. (Akten des Internationalen Leibnitz-Kongresses, Hannover, 14.-19. November 1966.) Wiesbaden: Franz Steiner Verlag GmbH. 1969. Pp. vi, 263. DM 44.

In 1966 a Europe forever purged, we may hope, of arrant nationalism by the ordeals of the twentieth century paid generous tribute to one of her most celebrated ecumenical spirits when she marked the two hundred and fiftieth anniversary of the death of Leibniz. Commemorative observances in Paris, Geneva, and Milan culminated, appropriately enough, in an international congress held in Hanover from November 14 to November 19. The Leibniz

Gesellschaft instituted on that occasion commissioned a new quarterly, the *Studia Leibniana*, which first appeared in 1969. A vehicle for the publication of longer monographic studies has also been provided in the form of supplementary volumes to the *Studia*. The first five of these, however, contain the addresses presented at the 1966 congress, with volumes 1-3 devoted to philosophical and scientific subjects and volume 5 to the history of philosophy.

Volume 4 of the *Supplementa*, with its sixteen *Vorträge* associated with seven titular rubrics, is especially wide ranging. The notorious multifariousness of Leibniz's ideas and activities requires no comment, but it is well to remember that a philosophical system based on the notion that individual beings are at once infinitely varied, interrelated, and expressions of an ultimate unity offers certain problems for the scholar who wishes to investigate any particular aspect of the system. And the difficulty is more rather than less severe in the case of the student of Leibniz's historical work, for here the empirical side of his method tends to disorganize, though by no means to discourage, his search for progressively more inclusive "unities-in-variety."

Of those contributors to the volume under discussion whose subjects are historical, some are content with one dimension while others, as a group more ideologically inclined, pursue Leibniz the "panhistor," but all understand Leibniz's profound interest in history, which is sometimes overlooked in general works. Eduard Winter sees Leibniz's *Kulturpolitik* as bound up on the conceptual level with the *Grundgedanken* of harmony, universality, continuity, and progress, as functionally related to the advance of education along many lines, and as geographically manifested in a kind of world civilization. Waldemar Voisé keeps his panhistor busy explicating several relationships: that linking the past, present, and future in the historical process; the bond between history and politics that might serve as the basis of a "science of a new sort"; and, very nearly (as well as somewhat extravagantly), the intimate affiliation of politics and statistics. Günter Scheel follows in detail the elaboration of Leibniz's plan for a typically monumental *Opus historicum*, which was, just as typically, never

brought to realization. The political ideas of Leo Stern's dialectically bedeviled panhistor lead to the depiction of an ideal world that is to issue from the reconciliation of competing interests, though these ideas originate in reflections upon the political problems of the day and especially in Leibniz's famous attempt to achieve a reunification of religious disciplines. This last subject is approached from different points of view by Jean Guitton and Lotte Knabe, but both predictably make the connection with the current ecumenical movement. The panhistor is demoted to *Apollon antisaxonicus* by Jean Neveux and merely watches as Paul Wiedeburg ably investigates a statement alleged to have turned Louis XIV against Leibniz's Egyptian Plan.

Many signs indicate a promising prospect for Leibniz studies: the abiding interest of a genuinely international group of capable scholars—and this country is very honorably represented; the modest but pleasing progress made on the German Academy edition of Leibniz's complete writings; and the plans made by the Leibniz Gesellschaft for another congress in the summer of 1972. And for this the 1966 meeting set a worthy standard.

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RAGNHILD HATTON and M. S. ANDERSON, editors. *Studies in Diplomatic History: Essays in Memory of David Bayne Horn*. [Hamden, Conn.:] Archon Books. 1970. Pp. xiv, 384. \$11.00.

The editors of this volume of essays in honor of the late Edinburgh historian D. B. Horn permitted considerations of persons to mar its thematic integrity as set forth in its title. Three of the eighteen historical essays (Denys Hay writes an introductory chapter on Horn himself) have little or nothing to do with diplomatic history, although Dietrich Gerhard's piece on regionalism and corporate order in European history (first published in *Historische Zeitschrift* in 1952) makes one wish that he had turned his particular gifts of erudition, imagination, and incisive thought toward the general problem of the states system. The essay by Guido Quazza is for the most part a sketch of the internal history of the Italian states in the first half of the eighteenth century; C. A. Macartney contributes a close study of the pre-

cise character and origins of the Austro-Hungarian Compromise of 1867.

All the other essays are on themes of diplomatic history, and most lie in the period between the Peace of Westphalia and the French revolutionary wars, the period in which Horn himself did most of his work. Some are concerned with episodes that have been little known or at best misunderstood, and they rest upon fresh archival research as well as wide-ranging command of printed sources. Notable among these are the study by Andrew Lossky on Dutch diplomacy and the trade negotiations between France and Russia in 1681, built around the correct identification of a document twice printed in the nineteenth century but wrongly attributed; Ragnhild Hatton's own contribution—a study of John Drummond, an English merchant turned diplomatic agent in Holland during the War of the Spanish Succession; and Stewart Oakley's account of the thwarted plans of Gustavus III for war with Denmark in 1783–84. The essay by G. C. Gibbs on the laying of treaties before the British Parliament in the eighteenth century links diplomatic history with domestic constitutional developments in a way significant for both, as does the study by Henry L. Snyder of the problem of diplomatic appointments during the Godolphin ministry for domestic political history. V. G. Kiernan lightens the fare with wry anecdotes of the small fry of diplomacy, careerists of greater ambition than influence. M. S. Anderson's discussion of theories of the balance of power in the eighteenth century—one of the key problems of European diplomatic history—is informative and thoughtful.

The volume is rounded off by a characteristically sagacious essay by Sir Herbert Butterfield on the significance of diplomacy in the relations of states and peoples; he observes in particular that diplomatic negotiations are not essentially efforts at persuasion but the verbal equivalent to war, in the Clausewitzian sense of the imposition of one will upon another—but better than war for all that. The other essays by James N. M. Maclean, John C. Rule, Walther Mediger, Alice Carter, Michael Roberts, I. H. Nish, and Esmonde Robertson display the same qualities of excellence of research, intelligence, and perceptiveness. The absence of nationalist passions should not go

unremarked, all the more when it is accompanied, as in these authors, by an awareness that what was at stake in the episodes they study was not the private affairs of the statesmen, great or small, but the fate of peoples.

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PIERRE CHAUNU. *La civilisation de l'Europe des Lumières*. (Collection Les grandes civilisations, Number 11.) [Paris:] Arthaud. 1971. Pp. 664.

Within the past five years, and in some instances perhaps prematurely, increasing numbers of quantifying historians in France have turned aside temporarily from their magisterial regional studies of *ancien régime* society and have undertaken striking syntheses intended for the nonspecialist and the cultivated public. Often applying to the archival and statistical findings of an entire generation an intuitive genius worthy of the heirs of Marc Bloch and Lucien Febvre, scholars such as Fernand Braudel, Pierre Goubert, Pierre Léon, Robert Mandrou, and Pierre Chaunu have written a remarkable series of essays. They sweep confidently across France, Europe, America, and Asia of the sixteenth through eighteenth centuries, and guarantee for this period the central place of a social and economic interpretation in the widest sense. On the right wing of present-day *annaliste* scholarship, Chaunu is perhaps the most productive member of the group. Completing his twelve volumes on Seville and the Atlantic (1504–1650) in 1960, he has devoted much of the past decade to two studies of European expansion for the *Nouvelle Clio* series. He has capped his career at Caen and the Sorbonne with a pair of syntheses, one for the civilization of seventeenth-century Europe (1966) and more recently, one for that of the Enlightenment.

Six years ago Chaunu concentrated upon the apparently paradoxical circumstance of the birth, development, and triumph of a mechanistic cosmology within a self-contained Europe suffering from illiteracy, war, economic crisis, and demographic stagnation. Environmental difficulties notwithstanding, the Age of Reason survived; and as its spokesmen turned from ontology to empiricism in the eighteenth century,

it commenced molding a material civilization in its own image. The present volume underscores the coming to terms of mind and environment in the Enlightenment. From the 1680s on, increasing numbers of Europeans consciously or instinctively ordered their collective existence in a manner consistent with empiricist thought. To Chaunu the fruit of this filtering down of certain ideational processes, this triumph for intelligence, is illustrated in most dramatic fashion demographically. The gain of ten years of life from 1680 to 1780 and the sparing of forty millions, who a century earlier would have perished in infancy or adolescence, derive—according to Chaunu—from the practical concerns of Enlightenment thought, from “le retour des pensées sur les choses.”

In support of his position Chaunu entertains a polemical stance that runs consistently through his volume. Though he avers that the eighteenth century represents the climactic moment of traditional European civilization, the heroes of his hypothetical Enlightenment number those pragmatists who led others into the value system of nineteenth-century liberalism. They include improving English landlords; those enlightened despots and their servants sincere in combating illiteracy and hunger; thinkers such as Locke and Voltaire who constructed political theories upon a conception of the possible; all empiricist scientists and scholars from Leeuwenhoek through Abbé Nollet and the Benedictines of St. Maur; Kant who legitimized an interior faith as he fixed the limits of reason; and Chardin who painted “des choses qui ont une âme, parce qu’elles sont la récompense de l’effort intelligent de l’homme, du travail.” On the other hand, Chaunu’s villains are those who refused to let ontology die, the pure theorists who turned their backs upon the facts or progress. They include French *rentiers* and *parlementaires*; Rousseau and Mably who pushed European political thought toward the utopian, the unrealizable, and the destructive; materialist theoreticians such as Holbach and La Mettrie whose “systématiques grossières trahissent l’esprit [empiriste] des Lumières”; and rococo artists working in a false baroque, a baroque denuded of its supernatural *raison d’être*. Chaunu’s opinions are sometimes outrageous and occasionally amusing. His associating the

Curé Meslier with the Black Mass and the (unproven) French practice of *coitus interruptus* with the poison of neo-Augustinian pseudoskepticism provoke chuckles, not thought.

Chaunu’s idiosyncrasies notwithstanding, his *Civilisation* has a good deal to recommend it. There are 247 superb photographs with commentary, representing a marriage of illustration to text that brings to mind Lewis Mumford’s *City in History*. Yet the book’s chief merit lies in the summaries on the state of historical quantification in France today. The chapters on population densities, growth rates, migratory habits, and the correlations between literacy and mortality are highly useful. Chaunu accepts uncritically too many of Le Roy Ladurie’s hypotheses on the relationships between climate and social habit, but it is good to see such seminal work obtain the advertising it deserves. The same holds true for the quantitative historical studies of the book and its eighteenth-century readership now being undertaken in France and the United States, though a slip in transcription leads Chaunu to estimate for four years (1723–27) what in reality represents legal French book production for sixty-six years (1723–89). The aforementioned areas, as well as those concerning social attitudes toward marriage, women, children, and death, represent fields where vigorous research in Western Europe and the United States is resulting in an imaginative and carefully constructed monographic literature. Enduring Chaunu’s hypotheses and polemic is not too high a price to pay for being introduced to work of those who are transforming our entire conception of society in the *ancien régime*.

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FRANCO VENTURI. *Utopia and Reform in the Enlightenment*. New York: Cambridge University Press, 1971. Pp. v, 160. \$8.50.

Professor Venturi of Turin, widely known as a cosmopolitan, erudite, and judicious writer on the political history of the Enlightenment, has concentrated in *Utopia and Reform* upon two aspects of the subject about which he has been publishing for the last thirty years. He examines the extent to which eighteenth-century republicanism derived less from the exemplars of



classical antiquity than from the recent history and character of Italian, Flemish, and other communes and of Dutch, Swiss, English, German, and Polish state systems. He then isolates the problems of vital importance in the developing social consciousness of the age, problems involved in considering the right to punish. Though discussion focuses upon these two matters, the volume is full of illuminating remarks on the bibliography of the *Aufklärung*, on its motto, "Dare to know," on England's puzzling shortage of philosophes comparable with those of the Continent and Scotland, and on the chronology and geography of the Enlightenment.

In the sixties, though even then tension was growing, the crisis of conscience was over; religious and moral problems had given way to political and social concerns, Pyrrhonism was supplanted by a new faith in nature, philosophic system by experimentation, and legal questions by economic inquiry. Even in remote parts of Europe the encyclopedists of Paris stimulated thought, while slowly revolutionary movements in Corsica, Russia, America, and elsewhere in turn encircled France.

With the French Revolution the cult of the ancient world revived and must be distinguished from the republicanism inherited from the Middle Ages and Renaissance. Montesquieu's scrutiny of the problems facing republics was always in the context of his own times, which forced him to consider size, federation, moral spirit, and internal and external dangers in relation to the threatened contemporary republics. Venice and Genoa, the United Provinces and Switzerland suggested questions. A new republic in America later illustrated one answer to these, as in mid-century the English compromise had offered another.

Venturi connects the right to punish with the fundamental relationship of individual, society, and property. Philosophes confronted by the existence of crime, and the need to repress it, decided that the state's only tasks were to estimate the damage caused by law breaking in both public and private spheres, and to restore harmony. Legislators and jurists, Beccaria declared, should rule "tremblingly" and try to combine equality and freedom with order and security.

A brief description does but scant justice to

the richness and originality of a slim, but difficult and concentrated book.

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MICHEL DEVÈZE. *L'Europe et le monde à la fin du XVIII<sup>e</sup> siècle*. ("L'évolution de l'humanité," Volume 71.) [Paris:] Éditions Albin Michel. 1970. Pp. 703. 12 fr.

This will be an encouraging book for those who fear that history may disappear in an accumulating mass of microscopic studies of local matters. Michel Devèze, a professor at the University of Reims, has indeed proved himself in the field of intensive research, having produced some years ago two large volumes on the forests of sixteenth-century France. Now he offers us a work of synthesis in the best French tradition of that difficult art. It is a true synthesis, carefully put together, piece by piece, with over eleven hundred references to hundreds of specialized books in half a dozen languages. It reflects the broad views associated with the name of Fernand Braudel and the École Pratique des Hautes Études, and it is at the same time volume 71 of the series *Évolution de l'humanité*, founded by Henri Berr over sixty years ago and now carried on by the Centre International de Synthèse.

The author begins with the formation of the concept of Europe in its modern sense in the seventeenth century. This he attributes to a new perception of the globe and the continents that came from the age of discovery, to a differentiation between the old and new worlds, and to a feeling that the peoples of western Eurasia were distinguished, no longer as Christendom, but by sharing in a common secular culture, which had technical and organizational advantages in the production of wealth and power. The superiority of Europe in these respects, while becoming apparent in the fifteenth century, was fully evident in the eighteenth, thanks in part to the decline of Islam and the withdrawal of China and Japan into themselves, but owing mainly to the unprecedented development in Europe of a spirit of innovation. This spirit, which favored science, invention, governmental restructuring, conquest, and expansion, is explained (as by Ranke and others) by the competitive relationship among the European peoples in contrast to the more

peaceable and stable but hence less innovative civilization of China. In any case, by the eighteenth century, and by the use of a marked maritime superiority, both naval and commercial, Europe was the most active and aggressive member of an interconnected worldwide system of which it was also the main if not the only beneficiary.

It is impossible to summarize the rich detail of this enormous canvas. Its main sections present the impact of Europe on Asia, Africa, and the Americas, and then the impact of these other continents on Europe itself. We are not put off with mere generalizations. There are fascinating particulars on a wide array of topics—the Jesuits in Paraguay; the Dutch in Japan; the Russians in Siberia; the production of gold and silver in Spanish America and Brazil; the impact of tea, coffee, cotton, and South Pacific astronomical observations in Europe; the democratic ideas of John Wise in Colonial Massachusetts; and the deism of Ethan Allen in early Vermont. The author's knowledge is so enormous that it seems absurd to point out the few errors that any one reader can identify; nevertheless, his mistaken idea that J. S. Copley and Benjamin West were born in England leaves him with a less than adequate appreciation of the state of the arts in the Anglo-American colonies. His bibliography shows an extraordinary familiarity with books in English. He knows the work of Needham on China, of Curtin on the Atlantic slave trade, and of Bamford on forests and French sea power. His treatment of Japan is derived almost entirely from works in English, including those published in English by Japanese scholars. It is surprising and unfortunate that he has apparently missed a few works that could have been useful to his purpose, including McNeill's *Rise of the West*, Lach's *Asia in the Making of Europe*, and Davis's *Problem of Slavery in Western Culture*.

For so vast and complex a theme no single century offers a very satisfactory basis for periodization. The author finds, however, that the close of the eighteenth century was in its way the end of an era. If Britain won out as a colonial power in the Napoleonic wars, the French won the ideological battle with the beginning of diffusion of ideas of the French Revolution to the rest of the world. The antislavery move-

ment portended the end of the eighteenth-century system. With Abbé Raynal and others doubts were expressed on the racism and exploitation that underlay the great positive accomplishments of the worldwide system of commerce, exploration, and science. The French expedition to Egypt in 1798 stirred Islam, the slaves in Haiti rebelled, and the independence of Latin America could be foreseen. The ultimate limits of the expansion of European power could be faintly discerned—in the West by the independence of the United States and in the East by the resistance to Europe of the sleeping giants, Japan and China.

It is books like this that, for teachers, might help to solve the problem of combining the negative thing called "non-European" history with European history as we have known it.

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DONALD DREW EGBERT. *Social Radicalism and the Arts: Western Europe. A Cultural History from the French Revolution to 1968*. New York: Alfred A. Knopf. 1970. Pp. xxxiii, 821, liii. \$15.00.

Professor Egbert's work on *Socialism and American Life* (1952) led him into a study of modern radicalism "as reflected in theories of art, works of art, and the social activities and beliefs of their creators" from the French Revolution to the present day. The result is, as the author notes in his preface, not art history but the history of "social conceptions as they affect and are affected by the theories, actions and creations of . . . the modern artist." It is also the story of the alienation of avant-garde artists from the state and society. The first part of the book deals with the chief ideologists of social radicalism in the eighteenth and nineteenth centuries; the subsequent three sections cover France since 1815, England, and the rest of Europe. The survey of the esthetic views of the early socialists and anarchists was no doubt the least complicated part of the book to write, and it is also the least controversial. While Marx and Engels are frequently quoted and even more often invoked to this day in this, as in so many other contexts, they did not really aim at providing specific guidelines for art and artists. In the West, at any rate, the direct political impact of utopian socialism and anarchism on the arts was probably larger than that

of Marxism. Saint-Simon's concept of the artist as a leader of society had considerable appeal; Fourier's influence on surrealism (and beyond) and the impact of anarchism on the symbolists and the New Left is well known. There are some curious lacunae: Lassalle is mentioned, but only in passing; Franz Mehring's name does not even appear; nor does the Gotha party conference of 1896 figure, the first occasion, I believe, in modern times, on which the attitude of the proletariat toward modern art was discussed by the highest body of a Socialist party with millions of followers. Generally speaking, Professor Egbert devotes far more space to developments in France and Britain (and seems more familiar with them) than to trends in Central and Southern Europe. There is a highly informative chapter on Belgium, but Austro-Marxism and its attitude to modern art is not even mentioned. The same applies, incidentally, to the narrower field of art history; the reader will look in vain for a discussion of Avenarius and his circle, for Max Liebermann and the Berlin *Sezession*.

But the real difficulties in a work of this kind lie beyond the discussion of esthetic theories. The author does not restrict himself to the fine arts and the "minor arts" but frequently refers to the theater and the cinema and to literature and music; this is a sensible approach because it is clearly impossible to deal with the "fine arts" in isolation. But once the scope of the investigation is broadened, the choice of illustrations and references becomes by necessity selective and arbitrary.

It is of course well known that the political and the artistic avant-garde have by no means been identical, and the discrepancy between the two has, if anything, become more pronounced over the last seventy years. It is comparatively easy to point to attempts to change the environment made by artists and critics from Ruskin and William Morris on; it is far more difficult to show, except in a very general way, to what extent artists have been determined in modern times by their environment. Art forms and, a fortiori, personal style have to a large extent developed independent of the social order, and there is no obvious connection between revolution in the arts and political revolution. To give but one example: the founding fathers of nonobjective art, Kandinsky and Kupka,

Klee and Feininger, were all radicals of sorts, but what Professor Egbert says about Feininger applies to all of them: "his social interests scarcely affected his art." The same, needless to say, goes for Schönberg and Stravinsky, for Proust and Kafka and Joyce. Only a few artists have opted for total political commitment (for example, Fougeron, Taslitsky, and Paul Horgan), but they were not exactly the greatest of their age, and the political tribulations of Picasso are well known. The author makes the interesting point that the choice of the medium matters in this context. The most famous radicals such as Daumier, Steinlen, and Grosz were all essentially graphic artists; when they turned to painting their art was not of the same quality.

Professor Egbert's book is a mine of information, and it will offer something of interest to every reader. His judgment on major issues is sound, and the conclusions that emerge are unstartling: that many artists were to a varying degree alienated from society; that being individualists, they tended toward some form of libertarian anarchism rather than collectivism and strict party discipline; or that artists are more likely to be radical in their youth than in their old age. The author notes that in recent decades the alienated avant-garde has become in the noncommunist world the artistic mainstream with the active help of the establishment; as a result there is no longer an avant-garde.

If on concluding this massive volume one feels nevertheless a sense of dissatisfaction, this has to do to some extent with the intangible and inchoate character of the subject: to deal adequately with the many topics raised, a dozen volumes would be needed. At the same time the case for studying in detail the artist's political ties is not proven; if the political orientation of the artist, as Egbert says, does not account in the deepest sense for the artistic merits of his work, they cannot possibly provide a major key to the understanding of his work. The class interests and the political aims of the peasantry are obvious, those of the artists are not. The answer provided by Herbert Read (that there has never been before the modern epoch an art without social significance) is not quite convincing, for "social" is not a synonym for "political." True, the artists'

politics are of sociological interests, but then a third-rate artist may well reflect the *Zeitgeist* more accurately than the lone, isolated genius. Among the shortcomings of the study is the fact that it reads in parts like a catalog of artists and their work, which does not always clarify their respective importance or relevance. The ideological impact of the writings of Francis Klingender and Christopher Caudwell is surely not equal to that of the leading Continental left-wing theoreticians in this field. The terms "radical" and "Marxist" are used rather freely and sometimes indiscriminately; to call Wilhelm Hausenstein or Walter Benjamin Marxist is at least questionable. William Morris is alternatively called a "Marxist" and a "revisionist"; but even the revisionists did not reject, as Morris did, the class struggle. On Romain Rolland it is said that he "died a convinced communist although he had never entirely shed the influence of East Indian mysticism," which means of course that, with all his admiration for Stalin, Rolland was not a Marxist. Again, the fault is not entirely the author's, for artists have been (and are) notoriously insistent in their ideological beliefs. But this makes it all the more important to differentiate between Marxist and *Marxisant*, between Communist and *Salon Kommunist*, between socialist and antiestablishment attitudes—or to refrain from such classification unless it can be done with a minimum of confusion.

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JONATHAN BEECHER and RICHARD BIENVENU, translated, edited, and with an introduction by. *The Utopian Vision of Charles Fourier: Selected Texts on Work, Love, and Passionate Attraction*. Boston: Beacon Press. 1971. Pp. xiv, 427. \$12.50.

This volume, introduction, selection, translation, and notes, represents an excellent piece of work. The introduction provides a penetrating summary of Fourier's remarkable system. It both emphasizes its essential unity and treats with sympathetic understanding its diverse parts. The selections are generally brief, very numerous, and skillfully arranged to present with great richness and fidelity the strange teaching of the father of the phalanx. The exposition moves from "first proclamations" to

"commerce, industry, and work in Civilization," and "philosophy, morality, and sex in Civilization," followed by "the theory of passionate attraction," "the ideal community," "attractive work," and "the new amorous world," with "the mathematical poem," Fourier's theories of cosmology and analogy, as the conclusion. The book also contains a preface, a note on the sources, a bibliographical note, a glossary, and an index. The translation is splendid, a feat that can be properly appreciated only by those who have worked much with Fourier. The notes, that is, brief introductions to the several sections, as well as some more technical notes at the bottom of the pages, constitute the least distinguished part of the volume; but they, too, serve their purpose in helping the reader.

Some particulars, of course, can be criticized. There are, for example, the rather lame pages on whether Fourier's system was or was not tyrannical (pp. 271-73), whereas Professors Beecher and Bienvenu really know that everything depended on the efficacy of Fourier's formula: if the formula is valid, life in Harmony would be the very essence of rapturous liberty and joy, if it is not valid, there would be no Harmony, or, if something of the sort could be somehow imposed, it would be a total tyranny. Again, while the authors are completely correct in emphasizing the extreme refinement and good taste of the future society, they are nevertheless wrong when they assert that "Harmony would have no room for the Promethean dynamism or the sweating bodies and calloused hands" (p. 72). As Fourier repeatedly informed us, the marvels of Harmony would include just such dynamism, found only very occasionally in present-day life, for example, when miners dig desperately to rescue trapped comrades or when soldiers storm enemy positions in the heat of battle. But these and other possible cavils do not impair the generally very high quality of the volume.

It is good to have so much Fourier so well presented and readily available in English. Although the prophet of Harmony aimed to encompass the universe in a formula, he also had formulas for everything else, some of them a paragraph or a sentence long, and these formulas too often stop the reader. "The poor incessantly attempt to rob the rich on an individual

basis, and the rich continually plunder the poor as a class" (p. 302).

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ORON J. HALE. *The Great Illusion, 1900-1914*. (The Rise of Modern Europe: A Survey of European History in Its Political, Economic, and Cultural Aspects from the End of the Middle Ages to the Present.) New York: Harper and Row, 1971. Pp. xv, 361. \$8.95.

Two ideas, theses almost, link the many details of this book. One is that "the years from 1900 to 1914 were not simply the sunset of the nineteenth century—or the last act in a Victorian play—but rather a period that clearly belongs to the twentieth century," not an end but a beginning, "not the twilight of a golden age but the seedbed of our twentieth century problems and concerns." The other thesis is that while it was the great illusion of a progress-proud age to believe that a general war among the nations of Europe had become unthinkable, that war, when it came, was by no means inevitable, that "it cannot be argued convincingly that the First World War was predestined or prefigured in the arts and sciences, in economic relations, in the area of ideas, or even in the public relations of the peoples of Europe."

Case proven? Not quite, not on the first count, that is. There are of course some formidable obstacles in the way of showing that any given fourteen years will form a historic unit, even if they happen to occur between the opening of a century and the beginning of a war. Then, too, the nature of the book, designed as it is as a survey in the Langer series, makes the task no easier. The mass of detail tends to obscure whatever pattern may exist—the eternally carping critic: too many details on the one hand, too few on the other (sometimes, the urge to inclusiveness results in an almost private shorthand). Or will the reader really know, without further explanation, what either plural voting in Belgium or the Selden Patent were, or be able to make very much of a half-line mention of De Mun? There is the fact, besides, that on more than one occasion the discussion of early twentieth-century trends reaches back into the nineteenth, and rightly so, since the discussion of new departures in bi-

ology would be as incomplete without Darwin as the description of the automobile industry without Daimler.

On the second point, that the war was forecast neither in the stars nor in the hearts of villainous politicians, the book is very much more convincing. One reason is that the narrative finally slows down here; there is a chance for breath and for reflection. The other is the profound understanding not only of European diplomacy but of the European mood that suffuses these pages. Thus there are, to balance the obligatory accounts of friction, some excellent passages on the many equally real efforts at cooperation and accommodation. The Balkan Wars and the Moroccan crises are fully described, but so is the formation, in the same period, of several hundred international organizations, governmental and private. Thus, while the book is clearly a post-Fischer one, the judgments are full of the sort of old-fashioned good sense that may well be tomorrow's style again—on the determinist road to war pattern: "wisdom made manifest after the event"; on Bethmann Hollweg: "There were flaws and weaknesses in his makeup, and he woefully misjudged the situation in 1914. But fundamentally, he was a man of honor, sensibility, and peace." Was the writer unfamiliar, then, with the past decade's torrent of argument? No, he was fully aware "that current research on German war aims has revealed a new Bethmann Hollweg, but it should be remembered that war leadership produces unique transformations; the Lloyd George of 1909, for example, was not identical with the Lloyd George of 1918."

There may, of course, be those who will disagree still and side with the revisionists of the 1960s. But after Hale, they had better be very sure of their ground. And, in general, there is something fairly churlish about any criticism of this volume, the preceding lines included. For in essence this is a book by an author who has read widely and well, who has taken note of all recent works of importance and of much unfamiliar material as well (there cannot be many other American historians of Europe who refer quite so frequently and casually to the *Neue Zürcher Zeitung*), who has kept his own good counsel throughout it, and who has provided the reader with a path through a

thicket of facts. It would seem quite enough for one book.

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PAUL G. HALPERN. *The Mediterranean Naval Situation, 1908-1914*. (Harvard Historical Studies, Volume 81.) Cambridge, Mass.: Harvard University Press. 1971. Pp. x, 415. \$13.50.

On the eve of the First World War many European naval officers expected to see, when the struggle began, a great and decisive battle between England and Germany in the North Sea. There the main protagonists faced each other, the hostile fleets separated by a mere twelve hours steaming time; and there, at Jutland, the only large naval battle of the war did take place. As a result most research on the navies of the period has centered in the gray Atlantic. But what of the blue Mediterranean? The French, English, Italians, and Austrians, not to mention the Russians and Turks, had strategic interests and bold dreams for this area.

Mr. Halpern's book is the first comprehensive study of the prewar naval situation in the Mediterranean. He has obviously spent a number of years on the work, and it is a definitive account. The author has consulted the archives of France, Britain, Italy, and Austria as well as many private papers and relevant individuals. The book devotes a chapter to each of the main powers: France, as the dominant nation; her chief rival, the Italians; and the Austrians, whose navy grew from a coast defense force to a major contender in the Mediterranean balance of power. The fears and hopes of that bygone era are evoked. France was apprehensive about the possible union of the Italian and Austrian navies, which would seriously threaten her commanding position. She could count on little help from the British, who had withdrawn the bulk of their ships to the north. The Italians, with their technically daring and beautiful vessels, saw a chance—with their old antagonists, the Austrians, now allies—of sweeping the French from the sea. And the Austrians, seeking a wider role, spent lavishly for enormous dreadnoughts that they really did not need for their defense. These great battleships were then a symbol of prestige, and na-

tions from Chile to Turkey wanted them, whatever the cost.

This was a period of innovation in ships and innovators in naval administration: Fisher and Churchill in England, Delcassé in France, Grigorovitch in Russia, Montecuccoli in Austria, and Cattolica in Italy. They purged the deadwood in the officer corps, kept their ships at sea and away from ceremonial functions, and generally tightened up the slack conditions that had crept in during the long years of peace.

The last chapter carries the drama forward to its conclusion and shows the actual events that occurred in wartime. It is, as the author states, an anticlimax. Italy remained neutral and later joined the Allies, thus ending the French fear of an Italian-Austrian combination against them. While Austrian submarines were active, no major action was fought by that navy.

The book has no photographs—a pity, since the Imperial War Museum has such a fine naval collection for this period. The author could have spiced the story with a few anecdotes as his writing is sometimes rather unvaried. But he handles the mass of detail as well as the dominant themes in a straightforward manner. For those who wish to know the period, it is an indispensable book.

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ALBERT SEATON. *The Russo-German War 1941-45*. New York: Praeger Publishers. 1971. Pp. xix, 628. \$15.00.

Thirty-one years after Hitler's invasion of the Soviet Union, the Russo-German war continues to attract the attention of historians, military writers, and journalists. There are, still, some major problems connected with this aspect of World War II that, in its duration and ferocity, was the counterpart to the Allies' war against Japan. Why did Hitler start this campaign that proved so costly for Germany? Why, in spite of repeated warnings, was Stalin caught off guard in the beginning? Why did the fortunes of war change so dramatically? These are not easy questions, and Colonel Seaton has tried to answer them in a scholarly and readable book, which should please the specialist as well as the general reader. Based on a

broad range of German sources—official documents, journals, war diaries, unit histories, and personal diaries and memoirs—and on the available official Soviet histories and memoirs, the author concentrates on military operations and on problems of leadership and command responsibilities. The relative paucity and inaccessibility of Russian material is reflected in a somewhat unbalanced treatment, which, except in the case of war atrocities, favors the German side in breadth and detail. The summaries of strategic and tactical decisions and the assessment of responsibilities of military and political leaders on both sides are the most valuable parts of this study. The descriptions of military operations, on the other hand, are involved and complicated and not easy to follow, and although there are maps for each major campaign, they are, for the most part, poorly drawn and of little help.

Discussing the pre-1939 European diplomatic developments, Seaton believes "that Chamberlain was right and Churchill was wrong" in their assessments of the Soviet Union, which, in Seaton's opinion, was not ready to come to the aid of Czechoslovakia. He blames Hitler for starting the war in the east and for the German defeat at Stalingrad. Germany had no chance of winning the war to begin with, because Hitler was confused in his military and political aims, his intelligence service was poor, his resources were insufficient, and his general staff was inadequate. And while Stalin and the Soviet High Command made serious mistakes throughout the war, they were, on balance, superior to Hitler and his generals. In the last analysis, however, "Britain and the Soviet Union owed their survival to geography and to climate and only secondarily to their own endeavours."

In closing and with unacknowledged hindsight, Colonel Seaton faults Britain and the United States for not invading Europe through the Balkans and Scandinavia to defeat Hitler and, at the same time, to prevent the Soviet Union from dominating Europe.

GEORGE O. KENT  
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*durch die deutsche Wehrmacht.* (Studien und Dokumente zur Geschichte des Zweiten Weltkrieges, Number 13.) Göttingen: Musterschmidt. 1970. Pp. 404. DM 78.

More than half this volume is taken up by reprints of documents; the rest is an illuminating study of the last pathetic phase of Hitler's venture into the Balkans. The author's declared purpose is to set the record straight because both the 120-volume Yugoslav official documentary history of the war and the two-volume work by the Yugoslav general staff are "one-sided, have pronounced biases, and contain falsifications and exaggerations" (p. 9).

In his effort to present an objective account Hnilicka has tried to study all the documentation everywhere, and he deplores the fact that while the papers in American and British archives are readily available those in Yugoslavia and the Soviet Union are not. He makes good use of what he had access to and has indeed come up with a lot of raw material. He perused the published memoirs of participants and conducted interviews with survivors both in Germany and outside, notably in Yugoslavia itself.

The title is somewhat misleading, for the work does not deal exclusively with military affairs. Perhaps its most interesting and valuable sections, however, are the accounts of the military, SS, secret police, and Nazi political agents who were attached to the Hitlerite satellites in the Balkans—Nedić's Serbian and Pavelić's Croatian regimes. The author shows great understanding of the latter. Indeed, he declines to consider General Nedić a collaborator in the customary sense of the word so much as "an honorable Serbian royalist" (p. 47)—although it would have been helpful had he defined exactly what he meant by the customary sense of the word.

The author is concerned with a topic the essentials of which have been obscured by the partisan versions of history written by the victorious forces of Balkan communism. He is to be commended simply for attempting to tell the other side of the story in a scholarly fashion. What does become clear from his book is that had the peoples of the Balkans had their wish, the situation in the area would be very different from what it is now, for the noncommunist population had looked to be liberated

KARL HNILICKA. *Das Ende auf dem Balkan 1944/45: Die militärische Räumung Jugoslawiens*

by British and American troops rather than the Red Army (p. 48).

Hnilicka's book makes a substantial contribution to the military, international, and domestic history of the last two years of World War II in the Balkans. Any work of scholarly standard that assails the monolithic interpretation of the history of any period is to be welcomed. So is the present one.

BÉLA K. KIRÁLY  
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MAY MCKISACK. *Medieval History in the Tudor Age*. New York: Oxford University Press. 1971. Pp. 180. \$7.25.

The search for British antiquities in the reign of Henry VIII was primarily motivated by the necessity for justifying the Reformation in the English Church; by the time of the founding of the Society of Antiquaries in 1586, the Tudor establishment having long since been reaffirmed by the queen, antiquarianism appears often to have become an end in itself. Thus, Bishop Godwin in 1601 confesses that his delight in the study of history and antiquities "hath been somewhat greater than was needful for a man that had dedicated himself and his labors unto the service of God's church in the ministry." Oblivion had long since become as potent an enemy as the pope. Miss McKisack tells the story of this antiquarian urge beginning with Leland's itineraries with license to salvage books from the dissolved monastic libraries, through the more coordinated efforts of Parker and Cecil to recover and print early British historians both ecclesiastical and secular, to the patriotically oriented topographers Stow and Camden. Even the queen, on the occasion of receiving Lambarde's digest of the records in the Rolls Chapel, responded that she would be "a scholar in her age."

Miss McKisack gives generous acknowledgment to earlier studies of the movement, notably that of T. D. Kendrick (1950), but she has resurveyed the field with manifest profit, updating the scholarship and making new and extensive use of manuscript materials in the royal, diocesan, and university collections. It is therefore the more regrettable that she came so late upon F. J. Levy's *Tudor Historical*

*Thought* (1967), which had already dealt with many of the antiquarians she treats. Her chapter on general histories of Britain is the least satisfying since it is relatively derivative and, as a result, occasionally inaccurate. C. L. Kingsford's *English Historical Literature of the Fifteenth Century* (1913) still remains the soundest authority here. Value judgments are for the most part avoided. But to say that Protestant prejudice is a "major handicap" of Elizabethan historians is not merely to ignore the equally prejudiced Catholic historians of the period but to overlook the historical value of the prejudice itself.

W. GORDON ZEEVELD  
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CONRAD RUSSELL. *The Crisis of Parliaments: English History, 1509-1660*. (The Short Oxford History of the Modern World.) New York: Oxford University Press. 1971. Pp. xvi, 434. Cloth \$9.75, paper \$2.95.

This survey of English history from the accession of Henry VIII to the Restoration of Charles II incorporates the results of much recent research, but it is not merely a compendium of other historians' views. It is an intelligently constructed synthesis that is reinforced by the author's own research and enlivened by his selection of appropriate and often humorous examples and quotations. A sensible balance between factual and illustrative material makes this book more readable than many studies of its kind. Although it takes the form of a political narrative, it does not suffer from a lack of thematic emphasis or analytical insight. An introductory portrait of Tudor England and a chapter entitled "Puritanism and Fashion 1570-1640" include a variety of topics that can be discussed only within a broad chronological framework.

Although Mr. Russell does not give sufficient coverage to humanism and the problem of poverty, he describes other social, economic, and intellectual developments adequately. His discussion of the related issues of inflation and government finance merits specific praise. Russell is most successful, however, as a political historian. He offers an excellent account of the parliaments of the early seventeenth century and the best brief summary of the ship-money dispute to date. In analyzing religious develop-



ments he engages in a current debate by arguing that Puritanism, even in its Presbyterian form, was essentially conservative. It became dangerous to the Crown only when it moved into political opposition and revolutionary when fused with the secular radicalism of men like John Lilburne.

The central problem of explaining the civil war and revolution of the mid-seventeenth century Russell approaches cautiously. He argues that the war itself was unintentional and is therefore explainable only by mutual fear and distrust. The collapse of the system of government that preceded the outbreak of war he attributes to the failure of the government's religious and financial policies. He tends to be suspicious of underlying social explanations, insisting quite correctly that the revolution in its first stages did not constitute a challenge to the established social order. Yet this should not have prevented him from clearly relating the political breakdown of the early 1640s to the social changes of the preceding one hundred years. Although the author establishes a connection between the educational revolution and the growing self-confidence of the gentry in the House of Commons, he does not give proper attention to the crisis of the aristocracy or the alienation of the "country" from the "court." He is reluctant to take a stand on the gentry controversy, which he considers to be a statistically insoluble problem of rising or falling incomes. It might have been more fruitful to view the "rise of the gentry" as a complex social problem concerning the changing relationship between the gentry and the peerage rather than a strictly economic process.

Nevertheless Russell's book, which is part of an important series, deserves a wide audience. A reliable and up-to-date introduction for the student, it will serve the historian as a careful, balanced, and lively synthesis.

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J. D. GOULD. *The Great Debasement: Currency and the Economy in Mid-Tudor England*. New York: Oxford University Press. 1970. Pp. viii, 198. \$7.00.

Professor Gould has written an almost model monograph on the highly complex, technical,

and easily misunderstood subject of the Great Debasement. He leads both general Tudor and expert economic historians alike gently, but with evident enthusiasm, through the labyrinths of Tudor minting of coins and government monetary policy and resolves a number of scholarly controversies along the way.

He specifies the scope and limitations of his book at the outset: that his is the first study of the subject by a modern economic historian but that it lays no claim to be a "balanced and systematic view of all economic aspects of the Great Debasement" (p. 5). When one considers the thoroughness of this volume, however, one cannot help wondering what the author has omitted.

Other scholars will have to test the broadest significance of this study claimed by the author—that "the theory of mint affairs and of the foreign exchanges offered in this work . . . should . . . prove useful to economic historians who wish to concern themselves with monetary and allied topics in any part of the medieval and early modern periods" (p. 6). They will require a sufficient body of data such as Professor Gould had at his disposal (and he is admirably cautious in explaining the evidence he has used, its limitations, reliability, and the gaps in it). Evidently modern quantitative techniques and analytical tools may have limited applicability if, for example, the author is correct that no adequate assessment of imports and therefore no clear balance of payments position can ever be established. He pleads, moreover, for the creation of an import index, for which the essential requirement will evidently not be a knowledge of econometrics, but the kind of ingenuity in using scattered, difficult-to-interpret evidence that has always characterized the best in medieval and early modern historical scholarship.

The text is admirably printed, with a few very minor errors. Where feasible, charts have been included near the text they illustrate. Only a few extensive tables and three graphs have been relegated to the end of the volume. A few topics are discussed in appendixes, several shorter technical points in explanatory footnotes.

Professor Gould may chiefly be faulted for a tendency to protest too much his inadequacy to deal with some aspects of his subject. While in-

tellectual modesty is as commendable as it is rare, no one presupposes the historian's omniscience (and the author pays full tribute to those upon whose work he has built, while he is very generous toward scholars with whom he disagrees).

He uses rather haphazardly "I," "we," and "one." For my taste the "we" might well be eliminated altogether since the author is neither king nor pope.

One can only trust that Professor Gould's remoteness from manuscripts in England will not prevent his making further significant contributions to the study of this period. One would anticipate reading them with both profit and pleasure.

HOWARD S. REINMUTH, JR.  
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WILLIAM S. MALTBY. *The Black Legend in England: The Development of Anti-Spanish Sentiment, 1558-1660*. Durham: Duke University Press. 1971. Pp. 180. \$6.75.

At the beginning of this century, Julian Juderías, a Spaniard disturbed by his country's reputation for cultural backwardness, coined the expression "Black Legend" to refer to the writings of Spain's critics past and present. Soon after, historians picked up the term, using it primarily to describe the literature concerning Spanish maltreatment of the Indians. Now William Maltby, investigating anti-Hispanism in Renaissance England, applies the term to Tudor-Stuart works on Spain, including translations of sixteenth-century Spanish histories, religious writings of early English Protestants, literature on the revolt of the Netherlands, overseas expansion, and the Armada, as well as other miscellaneous pamphlets produced in the seventeenth century. Maltby examines these works one by one, shows how they consistently exaggerated, distorted, and misrepresented Spanish actions, and claims that they were responsible for establishing an English stereotype of the Spaniard as cruel, immoral, treacherous, and proud. This stereotype, according to the author, was reinforced by similar strains of anti-Hispanism in France and the Netherlands, so that the "legend of Spanish barbarism was allowed to grow and to become part of the intellectual baggage of Western man."

Black Legend is a loaded term; and Maltby's

decision to adopt not only the term but also Juderías's thesis that there has been some sort of virulent anti-Hispanism present in the last four centuries of Western thought was an unfortunate one. It led him to oversimplify Tudor-Stuart conceptions about the Spaniard and to ignore the real ambivalence in the Englishman's attitude toward his enemy. Renaissance Englishmen may well have hated the Spaniards, but, in writers such as Raleigh, there is also a certain awe, which sometimes even borders on admiration, for their power and accomplishments, particularly in the New World.

Maltby's preoccupation with the Black-Legend thesis also impels him to devote an inordinate amount of space to exposing the biases and inaccuracies of Tudor-Stuart writers without bothering to develop in any systematic or meaningful way the nature of the relationship he admits exists between these biases on the one hand and the Reformation and incipient nationalism on the other. For example, his book contains no discussion of the recent scholarship on Tudor-Stuart millenarianism, even though it is obviously pertinent to his subject. As a result, his study adds little to what is already known about the ideology behind the Protestant nationalism of England in this period.

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K. R. WARK. *Elizabethan Recusancy in Cheshire*. (Remains Historical and Literary Connected with the Palatine Counties of Lancaster and Chester, Volume 19, Third Series.) Manchester: [Manchester University Press] for the Chetham Society. 1971. Pp. viii, 200. £3.60.

This is a monograph of superb scholarship and painstaking research, judicious, and, on the whole, well balanced in its analysis and conclusions. K. R. Wark has added another important study to the growing literature on recusancy in Elizabethan and early Stuart times, providing an in-depth survey of the sources for Cheshire, thus completing the work of T. S. Willan, who surveyed recusancy in Lancashire in the reign of Elizabeth I some twenty-five years ago. Wark continues for yet another county the able work done by Dom Hugh Aveling for Yorkshire in recent years. If such stud-

ies could be forthcoming for all the counties, a much needed major investigation of at least this area of Catholicism in Tudor and Stuart England might be undertaken.

The study begins in 1559 with the enactment of the Elizabethan religious settlement and traces the activities and fortunes of recusant Catholics in Cheshire up to 1603, as well as the efforts of authorities to curb it. Utilizing a wide array of materials—including the most important single source for understanding Cheshire recusancy before 1580, the Metropolitan Visitation Book of 1578, as well as subsequent Diocesan Visitation records, Parish Registers, Bishops' Transcripts, Recusant Rolls, Plea Rolls, Crown Books, Quarter Sessions' Records, Sessions' Depositions and Examinations, Mayors' Letters, and others—Mr. Wark has assessed statistically and through personal vignettes the strength or weakness of recusancy as a serious governmental problem in Elizabethan times. The author succeeds admirably in this while still providing occasional glimpses of the human drama, the excitement of prison escapes, and the poignancy of young boys being whisked out of the country for years of exile and forbidden education abroad (pp. 81, 103, 108–10).

Mr. Wark offers excellent testimony to the impact of the gentry as the largest sustaining group, while noting that the lack of clear leadership from great families whose influence was predominant in the country was decisive in limiting the extent of recusancy in Cheshire, which Mr. Wark puts at a "known" 302 out of an estimated population of 63,865! More interesting is his examination of those social classes least studied in this matter, that is, business, professional or craftsmen, husbandmen, laborers, and servants. As would be expected, the largest number of known recusants were women. This he found to be true even though he indicates that before the 1590s the importance of women went largely unrecognized, so that until that date they were not indicted to the extent that the men were. Mr. Wark also supports with clear evidence the accepted view that as the reign progressed Elizabeth's government resorted less to imprisonment and more to enforcing the laws for severe fines and loss of lands and goods, hoping to impoverish the

recusants into obedience to the laws as a more successful means of obliterating the group.

While Mr. Wark offers little in the way of startling or new ideas, his meticulous research helps immeasurably to substantiate earlier general interpretations. There is, however, a curious inconsistency in his abrupt, and from his own evidence unproven, conclusion that before the end of the reign of Elizabeth I recusancy had developed a vitality no penal law could crush. All his evidence and intermediate analysis had pointed to recusancy as a minor, insignificant problem in the body politic as far as Cheshire was concerned.

The study is invaluable for its lengthy appendix containing a list of Cheshire recusants with a brief summary of what is known about each (pp. 138–73) and in his excellent and thoroughly up-dated bibliography. No scholar in the field can afford to ignore it.

SISTER JOSEPH DAMIEN HANLON  
*St. Joseph's College, Brooklyn*

RICHARD L. HILLS. *Power in the Industrial Revolution*. [Manchester:] Manchester University Press. 1970. Pp. ix, 274. £4.00.

HAROLD CATLING. *The Spinning Mule*. (David and Charles Library of Textile History.) Newton Abbot: David and Charles. 1970. Pp. 207. £2.75.

J. GERAINT JENKINS. *The Welsh Woollen Industry*. Cardiff: National Museum of Wales, Welsh Folk Museum. 1969. Pp. xviii, 410. £2.75.

KENNETH G. PONTING. *The Woollen Industry of South-west England: An Industrial, Economic and Technical Survey*. (Origins of Industry.) New York: Augustus M. Kelley. 1971. Pp. x, 214. \$20.00.

These four volumes reflect the sustained interest of English historians in the British textile industry at a time when it acted as a major catalyst of economic and technological change. The first two studies in the group are confined to technological advance, mainly in the cotton industry. That by Dr. Hills now becomes the best single account that we have of the development of cotton manufacturing equipment between the 1730s and 1820. The author offers no widesweeping reinterpretation of earlier work by Miss Julia Mann and Mr. Walter English. Rather, in a well-documented monograph he probes into the details of technical develop-

ment and especially relates manufacturing equipment to power sources.

Particularly enlightening is his investigation of the work of the inventors John Wyatt and Lewis Paul. Although they partially or fully conceptualized several advanced spinning devices (for example, their intention of placing drafting rollers inside a revolving tube implied tube twisting), it is most unlikely that they ever spun by roller drafting, the major innovation sometimes attributed to them. Mainly because, as Dr. Hills observes, they failed to establish the relationship between fiber length and roller spacing. Besides demonstrating the well-known fact that waterframes and mules, in exhausting traditional power supplies, stimulated the development of the steam engine as a source of regularly transmitted power, Dr. Hills carefully plots the installation of Boulton and Watt engines in textile mills before 1800 and reconstructs the problems facing early engine builders and their textile mill clients. The study seems most impaired by Dr. Hills's neglect of much of the artifactual evidence, the early machinery that may be found in museums in Lancashire and London.

Dr. Catling's examination of the development of the spinning mule is a fascinating, though unavoidably technical, history of a single highly significant machine. In it the author nicely utilizes his rare combination of experiences as a teenage mule piecer and as a long-time textile research engineer. Several aspects of the book struck me as of particular value: the lucid explanations, supported by helpful diagrams, of the techniques and advantages of intermittent spinning on spindle wheel, jenny, and mule; the equally clear accounts of the mule's working parts and their modifications; the results of engineering research into Richard Roberts's quadrant, which the author concludes could achieve "almost perfect winding" (p. 82); the subtle distinctions between the Bolton fine-yarn mule and the Oldham coarse mule and the more familiar differences between cotton and woolen mules; and the author's own recollections and analysis of mule room operations. Inexplicably Dr. Catling has omitted the spinning-mule data found in the "Manufacture of Cotton" article in Rees's *Cyclopaedia*. Though Dr. Catling gives some figures of mule spindleage and output, the eco-

nomic aspects of his account would have come into sharper focus had he used more machine-makers' records (for example, Dobson and Barlow's order books, which run complete from 1851 to 1951). Despite these weaknesses, no one attempting to write about the British nineteenth-century cotton industry should omit reading this book for insight into the technological tastes and limitations of British cotton-mule spinning; for instance, standardized maintenance, possible with interchangeable parts, was frustrated in Lancashire mule spinning because each "gaffer" was allowed to tune his pair of mules to a high degree of individuality.

The two remaining volumes are regional histories of the woolen industry. In his definitive portrayal of that in Wales, Mr. Jenkins examines woolen manufacturing techniques and the industry's pre-sixteenth-century structure, based on home and fulling mill, before treating five manufacturing subregions in turn: Montgomery, Merioneth, north Wales, west Wales, and the southeastern counties. The picture emerges of an industry handicapped by undercapitalization, bad organization, small-scale production, strong conservatism, and a variety of depressing English influences. These last began when the Shrewsbury drapers monopolized the finishing and marketing of Montgomery and Merioneth cloths from 1562 until the 1770s. Later, concentration following factory organization was frustrated by east-west canal and railway networks that severed mid-Wales from Glamorgan's growing industrial valleys and opened central Wales to mass-produced English goods. Merioneth mills survived by processing local wool to meet local needs, until wool prices rose during the Boer War and Welsh farmers turned to English manufacturers for higher profits. But the real problem was the absence of a strong capitalist class, rather than nefarious English practices. In west Wales, the industry's sixty prosperous years ended after the First World War because of failure to modernize equipment or to follow wider fashion trends toward lighter fabrics; dependence on the south Wales workers' demand for medium to heavy woollens had disastrous results during the miners' strikes of 1921-26. Mr. Jenkins's organization of his material leads to some repetition; otherwise only

minor errors were disturbing. The carding machine illustrated on page 36, for example, is certainly not Paul's of 1748; and the water-frame never spun wool, as opposed to worsted (p. 238).

Mr. Ponting's new introduction to the West of England woolen industry consists of a historical narrative, illustrations of extant evidence and artifacts, a collection of documents, a list of surviving buildings, a glossary of trade terms, a bibliography, and an index. I consider the concept of such an integrated survey excellent but wish that the imperfections in its execution were less numerous. The historical account is unbalanced since over a third is devoted to the conflict in 1802-03 between the shearmen and clothiers over the introduction of machinery, a struggle adequately depicted previously by J. L. and Barbara Hammond and Professor Arthur Aspinall. Curiously, the earlier battles over the introduction of the jenny and the longstanding hostility between manufacturing and agricultural interests are hardly noticed. Second, there are too many slips for it to be regarded as a wholly reliable introduction; for example, mistakes in patent dates on pages 47, 48, 55, and 66. Third, the documents suffer from irregular editing; for instance, commentary appears sometimes at the head and sometimes at the foot of the gobbet (pp. 137, 139). Also two important sources have been omitted: the 1661 description of clothmaking by the economist Sir William Petty, son of a Hampshire clothier and the 1823 description by William Partridge, a Gloucestershire dyer who emigrated to the United States. Finally, the American price of this book is outrageous.

In conclusion, three specific defects found in more than one of these studies seem worth mentioning. First, it has long since been shown that neither Leonardo nor Jurgin invented flyer spinning (cf. Catling, p. 14, Ponting, pp. 30-31). Second, B. D. Jackson's invaluable dating of the Rees *Cyclopaedia* articles, the unrivalled source for early nineteenth-century British technology, seems unknown to several authors (Hills, p. 245, Jenkins, p. 59). Last, the omission of footnotes by Dr. Catling and Mr. Ponting exemplifies a deplorable economy increasingly practiced by the newer English presses.

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Merrimack Valley Textile Museum

GEORGE RUDÉ. *Hanoverian London, 1714-1808*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xvi, 271. \$8.95.

This is the first volume to appear in the new History of London series, whose promise of high scholarly character is attested to by the distinguished list of authors that its general editor, Francis Sheppard, has gathered together. Professor Rudé has written a sound, traditional, satisfying history, which places London in the context of broader national developments without ever letting it lose its identity in the process. Chapters on its physical expansion, economy, social structure, amusements, religious life, and government provide lucid and convenient summaries of the standard works on the subjects, but do not attempt to reassess their conclusions.

How differently would Rudé have written the book a quarter century ago? It is in no sense a reproach to him, but certainly one to the industry of London historians, to be forced to answer, not much. The picture of Georgian London given us by Sir John Summerson, Dorothy George, and even the Webbs remains remarkably intact. For its political history, thanks to more recent works by Dame Lucy Sutherland, Ian Christie, Francis Sheppard, and Rudé himself among others, the past few years have seen more fundamental changes in our understanding. The chapters on London politics, therefore, particularly insofar as they deal with the role of the "mob," are the most valuable in the book. But the apparent durability of the earlier studies in the social, institutional, and architectural history of the metropolis, while tributes to their sound scholarship and persuasive interpretations, suggests that the present generation of historians is neglecting eighteenth-century London.

Such neglect is hard to understand; London's libraries and archives abound in unexploited source material, while Georgian London ought to exert a peculiar fascination today: a city sizable even by our standards, precociously afflicted with every conceivable twentieth-century urban problem, from poverty, pollution, congestion, riots, and crime to suburban sprawl and alienation; a great manufacturing center hardly affected as yet by the new techniques of the industrial revolution; a city with a mass electorate long before 1832 and

with radical organizations decades before the French Revolution; a city undergoing all the most unsettling stresses of the modern megapolis, yet universally admired by contemporary foreign visitors for its wealth, cleanliness, and order. We need both more specialized studies and more re-examinations of received opinions on eighteenth-century London, to make Rudé's book, admirably though it sets forth the present state of our knowledge, rapidly obsolete.

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FRIDA KNIGHT. *University Rebel: The Life of William Frend (1757-1841)*. London: Victor Gollancz. 1971. Pp. 320. £3.00.

Other than one of five columns in the *DNB*, there was no biography of Frend until Mrs. Knight's. She discovered a trunk full of his correspondence that provides detail for the story of his life. A fellow of Jesus College, Cambridge, and an able mathematician, Frend suffered banishment in 1793 after the vice-chancellor's court and the senate judged that his book, *Peace and Union*, offended against a university statute, meaning against the temper of the times. The account of this proceeding, meant to be the high point of the book, is the most disappointing part of it. Mrs. Knight spoiled her opportunity by relying entirely upon Frend's published version of the case, by her unnecessary partisanship, and by her inadequate scholarship. Frend's tactless conduct as a university rebel obscured the academic freedom issue and made his defeat predictable. Thereafter in London he lived among political radicals, dissenters, and literary people. The discussions of Frend's personal associations at Cambridge and in London are the best parts of the book. Frend maintained these personal connections and his interest in good causes even after 1806 when he entered upon a twenty-year career as actuary for the Rock Life Assurance Company. Mrs. Knight accounts for this career in one badly garbled page. By this time the book has gotten away from her. In middle and old age, Frend had a busy and satisfying life as husband, father, actuary, journalist, reformer, and friend of many of the literati, but all of this appears in sketchy form without depth and substance. A biographer of Frend needs to

know more of the history of Frend's times than Mrs. Knight bothered to learn. Instead of writing a very good book, she only wrote a pleasant one about a man who was actually bigger than the person she presents. Still, whoever undertakes to write a more substantial biography of this interesting man will find it difficult to bring out Frend's personality and character better than Mrs. Knight has done.

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D. C. M. PLATT. *The Cinderella Service: British Consuls since 1825*. [Hamden, Conn.:] Archon Books. 1971. Pp. x, 272. \$11.50.

Described as "the first comprehensive work" on its subject, this book breaks new ground in a little known field. The British Consular Service became a government service in 1825, but consular services on a private basis had begun much earlier. The book deals with administrative and social aspects, which supply almost the only firm ground for a historical survey. This was a relatively minor government service whose miscellaneous activities, though cumulatively important, left little record. Scattered in five continents and working as individuals isolated from each other, consuls had only the most tenuous relations with London, the seat of power, prestige, and patronage. Their miscellaneous functions, poor reporting, the slow mails, the lack in London of administrative oversight of their work, and the absence of fixed policies robbed the records of threads of continuity. The book makes extensive use of archival material, including consular reports, the evidence, minutes, and reports of investigating committees (which mostly left things as they were), parliamentary debates, and consular memoirs.

To have discerned the continuities and interest in such a chaos of particulars and to have digested it into a short, tightly packed book is a real achievement. Footnote references to sources are meticulous, and there is a good bibliography. The author's eye for essentials, the economy and lucidity of the writing, and the wide and careful research impress the reader. The unifying theme of the "Cinderella Service" adds interest to the book.

Two long chapters deal with the General

Service in the nineteenth and twentieth centuries. Two more, each of special interest, deal with the Levant and the Far Eastern services—both in international frontier areas of primitive administration and weak sovereignty, where Western powers sought to regulate contact by capitulations and extraterritoriality. Each chapter contains succinct subsections dealing comparatively with such matters as functions, recruitment, training, service conditions, salaries, control, and inspection. Political and judicial functions and extraterritoriality are emphasized in the two regional chapters.

The account of the amalgamation in 1943 of the Foreign Office, Diplomatic, Commercial Diplomatic, and Consular Services into a single Foreign Service brings the "Cinderella" theme to the forefront. It is the major emphasis of the preface and introduction and recurs in each chapter. In the epilogue the author avowedly becomes "partisan," exchanging "the role of historian for that of social commentator and critic." The consul was the victim of "social injustice," kept down by "the social distinctions and snobberies" of the Foreign Office and the highly privileged diplomats. Amalgamation instead of securing "equality" left the consul weaker. He still suffered from the "contempt" in which "for a century and a half the job itself was held." There was a time when "no self-respecting Foreign Office clerk or diplomat would have been seen dead with a consul as colleague." A historian bent on defending the underdog may risk going out too far on thin ice.

H. DUNCAN HALL  
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J. F. C. HARRISON. *The Early Victorians, 1832-1851*. New York: Praeger Publishers. 1971. Pp. xix, 195. \$10.00.

For historians raised on John and Barbara Hammond's compassionate picture of England during the Industrial Revolution, J. F. C. Harrison's *The Early Victorians* will evoke pleasure and nostalgia. Harrison once again describes the social experience of the Industrial Revolution with a Hammondesque vividness and sympathy. The old landmarks are still prominent: the Industrial Revolution, population growth, urbanization, the factory system,

King Cotton, and the dramatic cycles of prosperity and depression. The emphasis, of course, is on poverty and squalor. Harrison will not allow, any more than will the Hammonds, posterity to forget the starving frame knitters, naked coal hewers, inebriated navvies, and harassed menial servants; nor will he allow posterity to overlook slums whose intense overcrowding, bad water, and lack of sewerage meant high death rates, sexual promiscuity, drunkenness, and misery. In dramatic contrast to this squalor is the life of the upper classes.

Harrison's picture of the aristocracy emphasizes the elegance of their country homes, their local power, their foxhunting, and their use of spring traps and transportation to rid themselves of poachers. His picture of the middle class underscores their nouveau riche passion for fine homes, fashionable clothing, and many servants. Both classes expected deference from the poor. In press and pulpit they preached doctrines of self help and religious seriousness and advocated the gaol and the New Poor Law's grim workhouses for the really erring and lazy.

Harrison, in drawing again this picture of exploitation, has done a service for historians. However familiar this story of harshness and inequality, it is one that deserves retelling. But Harrison has done more than redraw a picture. He has also added refinements and discriminations. His treatment, for example, of class structures is particularly discerning. He goes beyond the simple upper-, middle-, and lower-class schema to delineate the many and varied divisions that made up the social complexity of Victorian England, divisions in which a well-paid iron foundry worker lived better than a poor property-owning green grocer. It is Harrison's particular descriptions of these various classes and how they fitted into the complex network of Victorian society that, along with insights on social movements and social attitudes, give this work some originality.

It is thus all the more regrettable that he has not purged himself entirely of a Hammondesque disposition to exaggerate suffering and oversimplify oppression. On page 81 he says of the New Poor Law of 1834, "outdoor relief was to be abolished and all recipients made to enter the workhouse." To remind Mr. Harrison that the New Poor Law said nothing at all

of a workhouse test might be pedantic, but it is not pedantic to remind him that the Poor Law Commissioners (whose orders, not the law, defined the workhouse test) never imposed it on every applicant. They always included in their orders classifications for those paupers who were entitled to outdoor relief. Never did they make "all recipients" enter the workhouse. This error is not an isolated one. Harrison's work is full of exaggerations, ranging from the mistaken notion that the early Victorians were "firmly in the grip of Malthusianism" (p. 6) to pictures of sexual promiscuity in factories, seven-shilling weekly wages in Wiltshire, and naked coal hewers. There were, to be sure, Malthusians, sex in factories, seven-shilling wages, and naked coal hewers in early Victorian England, but a careful reading of Kenneth Smith's *The Malthusian Controversy* (1951) or of the government reports on factories, agriculture, and mining will show that they were the exceptions. Harrison's admirable sympathy for suffering has led him to mar his largely accurate and often perceptive study with oversimplifications. This is unfortunate since the socialist tradition in English historiography, which has done so much to illuminate the reality of misery and oppression, has always been attacked for its biases and distortions. To free that tradition from those charges, historians as important as Harrison and as famous as E. J. Hobsbawm, general editor of this series and a reader of the book in manuscript, must be much more accurate and balanced.

DAVID ROBERTS  
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C. H. S. FIFOOT. *Frederic William Maitland: A Life*. (Studies in Legal History, published in association with the American Society for Legal History.) Cambridge, Mass.: Harvard University Press. 1971. Pp. x, 313. \$10.00.

Mr. Fifoot has put the whole historical world in his debt by publishing an edition of Maitland's letters in 1965 and now by writing the best life of Maitland we could ever have. Miss Ermengard Maitland asked him to write it and made available to him all the family papers and a rich yield of family memories. Maitland was born in London in 1850; two sisters were born in 1849 and 1851. The family had al-

ready, by marriage, acquired land in Gloucestershire. Maitland's grandfather Samuel Roffey Maitland came from a Scottish family with strong Nonconformist leanings, so that, although he went to Cambridge, he did not subscribe to the Thirty-nine Articles and could not take orders, but by the Easter term of 1816 he became a barrister. Scholarship and music were both more attractive to him than the law. He abandoned both the law and the Church but gave himself to the critical study of Evangelical writers. Maitland himself said "that we still have to do for legal history what S. R. M. did for Ecclesiastical history. His *Facts and Documents* is the book I admire most. . . . It teaches us that a statement does not become true because it is constantly repeated. A chain of testimony is never stronger than its first link."

Samuel Maitland had one son, John Gorham, born 1817, who had a distinguished career at Cambridge, third classic of his year, seventh wrangler, second Chancellor's medalist, and a member of the Apostles. He was called to the bar but, like his father, did not practice. He wrote a few pamphlets on income tax and became secretary of the Civil Service Commission. His wife was a daughter of John Daniell; she died while her youngest child was still a baby. Gorham died in 1863, but he and Samuel (who died three years later) had both added to the family money so that after providing for the two girls in the usual fashion of the day, it sufficed to give F. W. Maitland both a good education and "the sense of stability and independence which in itself justifies private income. But he inherited from his paternal grandfather more than land and money; a keen and original vision, intellectual powers, both wide and deep, moral courage and a resolve never to do less than his best. His was a rich inheritance."

But Maitland was not a prominent Etonian either in play or work; he hated the classics and felt cricket and football rather slow, but loved the river and walking. The same tastes dominated his Cambridge years. He was elected to the Chitchat club and in due course to the Apostles and to the Sunday Tramps, where he met Vinogradoff.

During the ten years that followed his success in the tripos in 1872 Maitland came to re-



alize that his future lay with the history, not the practice of the law, but had not Henry Sidgwick provided £300 a year for four years for a readership in law, nothing might have been done to help him. That Sidgwick meant Maitland to benefit seems certain although Mr. Fifoot does not actually say so. The 1880s were the years of decision for Maitland's public and his private life. At Leslie Stephen's house he met Florence Fisher, a sister of H. A. L. Fisher. She and her sisters could not have much spent on their educations with seven brothers to be launched on the world. She had had a country upbringing, which was to stand her in good stead in the Grand Canary. She was, moreover, a lover of music, and "a splendid player on the violin, viola and piano." When they were married Maitland was thirty-six years old and Florence was twenty-two. After a Devon holiday they set up house in Cambridge for the autumn term. In November the Selden Society came into being, largely through the energy of Maitland, but it was unfortunate that P. E. Dove, a barrister who was struggling unsuccessfully to collect work, was left too much power over the funds of the society. In 1894 he found that he owed the society £1,000 and that the society owed a large bill to the printers. Before the society had been in existence for ten years Dove gave up hope of pulling things together and committed suicide.

• To a historian Mr. Fifoot has settled conclusively what has always been a disturbing problem: how far ought we to accept Sir Frederick Pollock as the effective collaborator with Maitland in writing *The History of English Law*? That question is now settled once and for all. This great book is Maitland's as we always felt that it was. Pollock's hand need only be traced in the section on Anglo-Saxon law where even a new edition could not entirely eradicate the unsuccessful efforts of Pollock.

The happy family life of the Maitlands was continually overshadowed by the threat of illness. After much discussion they determined on the Grand Canary as the most convenient place to go. Save for the winter 1904-05 they went there every year. Despite constant illness, Maitland never refused any challenge whether it came from the Deputy Keeper of the Public Records and the Year Books or the family of Leslie Stephen and the duty to write a book to

preserve his memory. When he died in 1906, Maitland had done far more than most men have ever done in a longer life.

†DORIS MARY STENTON

J. K. CHAPMAN, editor. *A Political Correspondence of the Gladstone Era: The Letters of Lady Sophia Palmer and Sir Arthur Gordon, 1884-1889*. (Transactions of the American Philosophical Society, New Series, Volume 61, Part 2.) Philadelphia: the Society. 1971. Pp. 52. \$2.00.

History's losers have a nostalgic fascination about them. Often good people in terms of their own time, they are overtaken by events and changes that they have little capacity to control or even understand. In public they may put up a forthright defense, but in private they reveal their apprehensions and dismay.

Lady Sophia Palmer and Sir Arthur Gordon were such people. As the editor of their letters says, they were "conservative Liberals, not themselves leading politicians." Products of the mid-Victorian political elite, they viewed with increasing alarm the democratization and radicalization of English politics in the 1880s and of their leader, Gladstone.

They are interesting not so much because they "counted" but because they knew or were related to a great many people who did. Lady Sophia was the daughter of Gladstone's Lord Chancellor, Lord Selborne, and by family and acquaintance she was connected to leading political figures in both parties. Sir Arthur was the youngest son of the fourth earl of Aberdeen. After a political apprenticeship as his father's private secretary, he became a brilliant colonial governor over a period of thirty years, always imperiously preferring the Crown colonies to the self-governing ones.

The correspondence of Lady Sophia and Sir Arthur, scarcely any of which has previously been published, extended from 1880 to 1912. Professor Chapman has made a selection from their political letters from 1884 to 1889, along with two letters of Lady Sophia on the general election of 1880 and the death of Gladstone. Her letters, because she was in England, have more detail on people and their private views, while his, mostly written from the distance of colonial assignments, are mainly reflective discussions of political issues. The letters are generously annotated, but it would help to know

which ones Gordon wrote when he was out of England.

Not much is added to our knowledge of political events here, although a good deal is confirmed. More interesting are the central themes of fear of the future and betrayal. Whether the issue be socialism, the franchise, Ireland, sanctity of property, or the Church, both correspondents reveal a sense of doom. They had little recognition of the economic and political stresses reshaping the country, and Gordon could only despair at "a large, inert mass ready, though not willing, to be revolutionized" by "the few who really desire revolution." The villains were Chamberlain, Churchill, and most of all Gladstone "whose dangerous demagogic gifts did untold harm latterly." But with a forgiveness that ironically revealed her Victorian anachronism, Lady Sophia could say of Gladstone at his death, "Yet after all the *real* power of the man lay in his faith in God."

WILLIAM HENRY MAEHL, JR.  
*University of Oklahoma*

G. R. SEARLE. *The Quest for National Efficiency: A Study in British Politics and Political Thought, 1899-1914*. Berkeley and Los Angeles: University of California Press. 1971. Pp. x, 286. \$8.50.

G. R. Searle's study of the political ideal of "national efficiency" attempts to transcend the political historian's conventional concern with party, institutions, and policy. But Searle is most persuasive as a traditional political historian successfully tracing the complicated course of Lord Rosebery's bid for national leadership from 1900 to 1902, the Education Act of 1902, the Webbs' Poor Law Minority Report, and Lloyd George's abortive efforts toward a national coalition government in 1910. To identify a "national efficiency group" Searle argues that a reaction to Britain's decline as a great power in the last quarter of the nineteenth century, exacerbated by disillusionment with Liberal political forms, led to a "corporate identity" among such disparate political figures as the Webbs, H. G. Wells, G. B. Shaw, R. B. Haldane, A. J. Balfour, Lord Rosebery, Alfred Milner, Robert Morant, L. S. Amery, and Lord Esher. To these people Searle attributes a common ideology of imperialism, Broad Church traditions of social thought idealizing the Ger-

man state, reliance upon experts in government, and state-aided science and technology.

Searle's criteria for membership in the "national efficiency movement," evidently derived from the Webbs' analysis of inefficiency and their bureaucratic and suprapolitical remedies, are applied only to those eager to promote national strength. This treatment of ideas qua programs introduces substantive and methodological problems. An individual's use of politically popular rhetoric in the decade following the Boer War is not sufficient evidence that he belonged to an "efficiency group," especially since the same priorities and goals were advocated by liberals whom Searle puts in opposition to this group. C. F. G. Masterman and Herbert Samuel within government and Alfred Marshall, J. A. Hobson, and L. T. Hobhouse without had been serious and influential critics of wasted human resources before the Boer War made national reform a popular issue. The political activities of men like Balfour and Haldane, furthermore, cannot be discussed apart from the intellectual and spiritual assumptions that governed everything they did. Searle's exploration of politics and political thought exclusively in terms of the machinery of government ignores the development of political thought and conduct as part of a larger revolution in social, economic, and political theory and practice occurring from the 1880s to 1914 in the closely related communities of scientists, men of letters, members of parliament, dons, bankers, and businessmen.

REBA N. SOFFER  
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State College*

P. F. CLARKE. *Lancashire and the New Liberalism*. New York: Cambridge University Press. 1971. Pp. ix, 472. \$18.50.

DAVID AYERST. *The Manchester Guardian: Biography of a Newspaper*. Ithaca: Cornell University Press. 1971. Pp. 702. \$15.00.

The center of Clarke's argument is (1) Lancashire was one of the critical areas of political geography (Greater London was another) where the post-Gladstonian Liberal party had to make gains in order to win a general election; (2) the party succeeded here, not only in 1906 but in the 1910 elections; and (3) its suc-

cess in working-class constituencies here showed how strong and healthy the party was on the eve of the 1914 war. So much for all those from George Dangerfield to Roy Douglas who have argued the inevitability of Liberal decline by 1914.

The case for Liberal strength is not made simply in terms of the number of seats held by Liberals through the elections of 1910 (indeed Clarke shows that by December 1910 the Conservatives had doubled the number of seats they had in 1906 in the northwest). It is the fall in the Conservative poll that indicates their relative weakness; another sign is the marked Liberal success in the "more highly enfranchised" seats (that is, those with over sixty to sixty-five per cent of the adult males on the register).

All the same, the author concedes that what would have happened in a 1915 election, without a war, is "an open question." By restricting himself to Lancashire and the northwest he skips the fact that seven Liberal seats lost to Conservatives from 1910 to 1914 were the result of three-cornered contests. And surely it would have surprised the Webbs to hear that the term "progressive" has been "consigned to . . . American history." This is a serious, if very expensive book, with an excellent bibliography.

The chronicler of Manchester's *Guardian* proceeds with unhurried, affectionate detail from the aftermath of Peterloo to the Suez crisis of 1956. He relies upon the files and archives of the *Guardian*, including letters by its editors, and especially the letters of C. P. Scott. Over three-quarters of the book is devoted to the age of Scott (editor 1871-1932, or effectively 1871-1926) and after. The golden age began after the Boer War but before Scott's purchase of the paper in 1907.

This is a labor of love without any easily incapsulated thesis. There are fascinating anecdotes: Admiral Lord Fisher wanted Redmond made prime minister in 1916; before the Balfour Declaration the leader-writer Herbert Sidebotham saw the advantage of a Jewish Palestine (as a British dominion) safeguarding the Suez Canal; on Captain Walter Lippmann's advice, Scott tried to hire first Gilbert Murray and then his son-in-law Arnold Toynbee as an American correspondent in 1919.

Laurence Scott, grandson of C. P., found out in 1950 that *Guardian* readers all over tended to be well-educated people who cared much more for international politics than for local cricket. In 1959 the masthead read simply *The Guardian*; in 1960 the first London printing was run off. This is a good book about an admirable journal. Unhappily the footnotes are buried in the back, under chapter numbers.

BARRY MCGILL  
Oberlin College

ARTHUR J. MARDER. *From the Dreadnought to Scapa Flow: The Royal Navy in the Fisher Era, 1904-1919. Volume 5, Victory and Aftermath (January 1918-June 1919)*. New York: Oxford University Press. 1970. Pp. xxiii, 416, 7 charts. \$12.00.

In this concluding volume of his magisterial series on the Royal Navy in the Fisher era, Professor Marder has to handle three different themes: the triumphant conclusion of the war at sea; the readjustment of the navy from war to peace; and a summary, not only of the five volumes of this series, but of a scholarly lifetime devoted to the senior service. All are handled with the precision, clarity, and careful analysis that have marked the preceding volumes.

The most dramatic episode in the first part of the book is the surrender of the High Seas Fleet to Sir David Beatty's Grand Fleet on November 21, 1918, but the chapter that will be read most attentively is doubtless that which describes the final defeat of the submarine campaign. Marder's analysis and the statistical tables accompanying it demonstrate clearly the point made in volume 4: convoy was the answer, and in the circumstances of 1917-18, the only answer to the U-boat. Other expedients, such as the vast northern mine barrage and the spectacular Zeebrugge raid, had only the most fleeting and marginal effect. Yet the belief that convoy was defensive and therefore not consonant with sound principles of maritime strategy died very hard. The battle between the advocates of convoy and hunting groups had to be fought out again in 1939-41. The entire story of the convoy system and its vicissitudes is an instructive example of the weak staff work at the Admiralty as well as of the deficient intellectual grasp of strategy and

tactics by many senior naval officers—two characteristics that Marder highlights in his concluding chapter, "Reflections on an Era," as some of the worst failings of the Royal Navy during the First World War. It also illustrates another of Marder's conclusions: the unfortunate effect on the navy of the belief that the Nelson tradition required constant offensive action. In terms of the U-boat menace this translated into futile offensive sweeps a policy that, if persisted in, would have led to disaster. It was unfortunate that Sir James Saumarez was not given a small tithe of the attention that went to Nelson.

During the period between the armistice and the signing of the Versailles Treaty, the naval settlement with Germany occupied a great deal of Admiralty attention, as did demobilization, reorganization, and the naval aspects of the allied intervention in Russia. The two events of greatest significance for the future, however, were the "sea battle of Paris"—the clash between the British desire for primacy at sea and the American demand for a "navy second to none"—and the Admiralty's recognition that Japan was the most likely naval threat in the future. The story is taken up from this point on (with somewhat less clarity) in Captain S. W. Roskill's *Naval Policy Between the Wars: The Period of Anglo-American Antagonism, 1919-1929* (1969), but by the time the High Seas Fleet scuttled itself at Scapa Flow on June 21, 1919, enough had happened to make it clear that that event was the end of an era rather than the prologue to a new age of British supremacy at sea.

Marder's conclusions about the Royal Navy in the Fisher era lead the reader to reflect on Marder's achievement. It is not very likely to be bettered or supplanted for a long time to come. The recent publication of the third volume of the official Churchill biography by Martin Gilbert (*Winston S. Churchill: the Challenge of War, 1914-1916* [1971]), with copious extracts from the Churchill papers, does not substantially modify Marder's evaluation of Churchill's tenure of the Admiralty in 1914-15. On the *Goeben* episode, for instance, Marder's discussion is considerably fuller than Gilbert's. In fairness, Mr. Gilbert's declared intention is to rely exclusively on contemporary documents. An eight-hundred-page book largely devoted to

Admiralty affairs that does not once mention Marder's work, however, will strike many readers as remarkable. If this partial opening of the most important remaining private archive dealing with naval affairs during the First World War does not necessitate any serious modification of Marder's work, it seems safe to assume that nothing else will. Not everyone will share his great enthusiasm for "Jacky" Fisher or his more restrained enthusiasm for Jellicoe. Additions and corrections to the record will continue to be made. The work as a whole, however, is indispensable to our understanding of the last era of British supremacy at sea and a splendid monument to the ships and men who sustained it.

RAYMOND CALLAHAN  
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MAURICE COWLING. *The Impact of Labour, 1920-1924: The Beginning of Modern British Politics*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1971. Pp. ix, 570. \$17.50.

This book could have been the most notable contribution to our understanding of modern British politics. With all its faults it must still be considered one of the truly important studies in the field. The first to discuss the response to Labour from 1920 to 1924 and to show the extent to which Labour became "the major problem" for Liberals and Conservatives, it is also the first to explain that, with respect to Labour, the struggle within the Conservative party concerned tactics rather than policy and that the Conservatives who defeated the Coalition in 1922 were just as opposed to socialism as those who wished to remain allied with the Lloyd George Liberals. So, it seems, were the Asquithian Liberals who wanted to organize a center party in order to provide an alternative other than Labour to Lloyd George. Indeed, once the Coalition fell all Liberals—particularly Lloyd George—wanted to establish themselves as the alternative to the very same threat.

Why they failed and the Conservatives emerged as the party of constructive resistance forms a second, almost parallel theme. One might say themes, except that they are linked through the person of Stanley Baldwin whose political genius and studied moderation Cowl-

ing freely admits. In doing so he tacitly questions some of the sentimental myths surrounding the Conservative leader. More explicitly, he anticipates Roy Douglas, *The History of the Liberal Party* (1971), in suggesting that the Liberals' decline, far from being inevitable, was actually the result of astute tactics on the part of Baldwin as well as internal divisions and a series of miscalculations culminating in Asquith's decision to put Labour into office in 1924.

The story of this decision, like the rest of the book, is drawn from many private papers. Interspersed are amusing, if sometimes oblique, descriptions of the politicians involved. Yet the detail can be overwhelming and difficult to follow because *The Impact of Labour, 1920-1924* is poorly constructed and because its parallel themes—Labour's threat and the success of the Conservatives in meeting it—unfold in confused, uneasy tandem. The second yields long lists of feuding factions whose relevance to the problem of how best to deal with Labour is not always made clear. The basis of the problem is itself unclear and, for this reason, so are the grounds for the tactics adopted by the victorious moderates.

Take Baldwin's conscious effort to destroy the Liberals in 1924. One might have thought that in rejecting coalition as an answer to Labour, he would have looked to a revived Liberal party. His decision to adopt a protectionist platform in 1923 helped to revive it. But Cowling denies that the effect was intended without saying when Baldwin determined on Liberal collapse or when he came to accept either the eventuality or the possibility of Labour rule. At times he suggests that the leadership of Ramsay MacDonald after 1922 made the difference; at other times he suggests that Black Friday and the collapse of direct action in 1921 counted most. Either way there are problems. Leaders of the Labour party had never endorsed direct action and, contrary to what Cowling believes, their acceptance of parliamentarism did not begin in 1921. Whatever changes in policy occurred after that date cannot be ascribed to new methods of achieving them: if the changes were as great as Cowling implies, then it is hard to see why everyone was so fearful in 1922, and if the significant change came after 1922, then Cowling must prove that

MacDonald differed substantially from previous leaders of the party.

This he cannot do. His emphasis on the threat of Labour may be a useful corrective to Robert Skidelsky, *Politicians and the Slump* (1967), and others who minimize the differences among parties. But in the end he does not explain how great those differences were, whether Labour's threat was as real as it was perceived to have been, or why the conclusions drawn from the same perception differed so widely. Such omissions are serious in a book about the impact of Labour. Even more serious, perhaps, is his failure to discuss the constitutional implications of all the proposals for some sort of coalition or center party to keep Labour permanently out of office.

BARBARA MALAMENT  
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NOREEN BRANSON and MARGOT HEINEMANN. *Britain in the 1930's*. New York: Praeger Publishers. 1971. Pp. x, 358. \$11.00.

The excerpts are the best part—Sir John Boyd Orr speaking the truth on poverty and malnutrition, three angry Conservative M.P.'s reporting the brutalities of a Mosley rally, and a Rhondda Valley miner recalling how he and his mates stole coal and timber from the mines. When the authors let the contemporaries speak *Britain in the 1930's* comes alive. Nearly all else is disappointing.

The social and industrial topics skimmed in this discursive book have absorbed and continue to absorb the energies of dozens of specialists; yet for recent findings one searches in vain. For detailed accounts, meaty and authoritative, one must still turn to monographs long known. For synthesis one does far better with Charles Mowat's *Britain Between The Wars*, published in 1955.

Best done, I thought, are chapters on "The Unemployed and the Means Test," and on malnutrition ("Eating and Not Eating"). But the chapter on "The Radical Trend in Culture" remains a catalog of names. The subject of "Industrial Graveyards" presented an opportunity to show, in vivid human terms, what chronic unemployment meant; but the opportunity went by default, buried among generalities and statistics. Surprisingly weak is the

chapter on "Class Structure and Class Outlook," which one would hope to be particularly strong in a series designed "to bring together . . . what we know . . . about the structure and changes in British society" (p. viii). The chapter on "Homes, Landlords, and Building Societies" does not begin to suggest either the complexities of housing or the ferment of ideas through the decade. Two excellent chapters discuss industrial unions and their organizational efforts. The authors succeed in highlighting several themes: slowness of mass organization, lethargy of union leadership, the impact of the unemployed marches, the continued decline in workers' share of output despite rising productivity, persistence of stratified inequalities in income and spending power, and official apologetics, now evasive and now callous, for inaction.

Professedly "social history" concerned "primarily with the everyday lives and attitudes of the British people" and the "ordinary person," this book veers to the political often. The process is never haphazard. Attention focuses on the more striking efforts of Communists at the grass roots or on the more egregious (and, by implication, typical) profascist comments from the upper class. One result is to leave virtually unexplained the government's eventual liberalizations or modifications of policies originally callous or miserly. The book's credibility gains nothing from this partisanship.

PAUL B. JOHNSON  
Roosevelt University

FRANKLIN REID GANNON. *The British Press and Germany, 1936-1939*. New York: Oxford University Press. 1971. Pp. x, 314. \$10.50.

Gannon's first chapter may be described as an essay in minor revisionism devoted to a subject, appeasement, that has largely escaped such treatment. This substantial study is the product of strenuous effort. The revisionism is minor in scale because the author is not primarily concerned to impugn the present by subverting its vision of the past. Instead the author argues that appeasement was the result of British reaction to the Nazi regime as well as the result of a complex of attitudes dividing Britain's Left and Right—and splitting each of them. Some of these divisive problems persist

"to bedevil both contemporary politics and the possibility of a detached consideration of the inter-war period" (p. 31).

The revisionism largely consists of denying the foresight of Leftist liberals and the guilty propensity of the Right for Hitler as a mighty fortress against Bolshevism. There is nothing wholly new in this except for the sympathetic presentation of the Rightist position. Gannon's background setting presents the press competing for circulation. They were therefore unwilling to worry readers severely by drawing conclusions that might call for a costly response. An understandable but quite unheroic discretion was compounded by the fear of jeopardizing Britain's frail economic recovery. Left and Right were largely in agreement—"never again"—about World War I: the former favored the League of Nations, disarmament, the expression of outrage against Hitler, and, in smaller numbers, a national and international popular front; the Right favored rearmament, the British Empire, and the prospect of a settlement with Germany. At various times parts of both groups distrusted France and larger numbers thought the Versailles Treaty illegitimate and regarded Czechoslovakia as its unloved scion.

Gannon's pages convey the journalistic and editorial thoughts and sentiments in which appeasement flourished. His account of a selected number of London papers (as well as the *Manchester Guardian*) is interesting but somewhat random. Purists of method will not accept his rule-of-thumb justification of his selection. Those with a taste for drawing parallels with our times may find reading this book a haunting experience. But for all its interest the book is disappointing in the coherence of its design, in the inadequate development of the theme that appeasement marked a crisis in the conscience of liberalism, and in its impoverished concluding section.

M. A. FITZSIMONS  
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K. THEODORE HOPPEN. *The Common Scientist in the Seventeenth Century: A Study of the Dublin Philosophical Society, 1683-1708*. Charlottesville: University Press of Virginia. 1970. Pp. xii, 297. \$7.50.

Ireland produced few important seventeenth-

century scientists; one of the more interesting ones, however, was William Molyneux (1655-98) of Dublin, a friend and correspondent of John Locke and Edmond Halley. It was Molyneux who worked to galvanize the meager scientific resources of Ireland and to establish a working scientific society for Dublin.

It is a widely held belief, and basically a sound one, that important elements of the scientific revolution of the seventeenth century are epitomized in the numerous societies for the pursuit of science that sprouted during the course of it. The organization that has justifiably commanded and continues to garner the lion's share of attention has been the Royal Society of London. In his study K. T. Hoppen chose not to re-evaluate the basic premises of the literature but rather to examine in depth an offshoot of the London group. The Dublin Philosophical Society, founded in late 1683 and early 1684, offers a striking opportunity for the examination of the concerns, goals, and methods of late seventeenth-century science. "We possess in the records of the Irish group an accessible and fully documented case of a scientific society consciously organized upon what was conceived to be the principles of the Royal Society of London. We have here, as it were, a model of the Royal Society, stripped of its complex and obscured origins, its historical accidents, and its warring factions. In short, the Dublin Society provides a clear reproduction of the public face of later seventeenth-century science."

Hoppen has diligently collected the minutes, letters, and other papers pertinent to the Society, along with an impressive bibliography of secondary works; the sum provides an image of exhaustive and painstaking research leading to what will doubtless stand as the definitive description of the structure and function of the Dublin Philosophical Society.

Yet from the point of view of the historian of science, there remains something more to be done. The heart and soul of the Dublin Society was Molyneux, who, although best known for his single political tract, devoted an overwhelming part of his efforts to astronomy, dynamics, and optics. Molyneux left enough scientific correspondence (with the astronomer Flamsteed and with Halley, for example) and other manuscripts to keep a small team of scholars busy with new and exciting material.

Hoppen makes no concerted attempt in this book, however, to dig deeply into the character and value of Molyneux's achievement. Molyneux's very interesting translation of much of Galileo's *Dialogues concerning Two New Sciences* is passed off (pp. 129-30) as the *Dialogues concerning the Two Chief World Systems*.

But if Hoppen has not plumbed the depths of the science carried on in seventeenth-century Ireland, he has produced a polished, detailed, and valuable account of the organization and development of a scientific society that will surely provide part of the groundwork for that much-needed reassessment of the character and role of cooperative scientific efforts in the scientific revolution.

ROBERT H. KARGON

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GEOFFROY ATKINSON and ABRAHAM C. KELLER. *Prelude to the Enlightenment: French Literature, 1690-1740*. Seattle: University of Washington Press. 1970. Pp. 221. \$7.95.

This book is the last of three volumes by the late Geoffroy Atkinson on French literature in the period from 1690 to 1740. It is presented with interpretive commentary as a collection of quotations ("the quotations, and not our comments, are the meat of this book") that Abraham C. Keller has translated and selected from Atkinson's notes.

The authors stress the diversity of literary themes and eschew stuffing the Prévosts and Marivaux into categories but emphasize those themes that show a break from the classical age and anticipate Romanticism. Thus, in part 1 ("The Emotional Revolution") the excerpts reveal a penchant for wallowing in emotion that the generation of Racine would have found comical or ridiculous but that to later writers such as Bernardin de Saint-Pierre would have seemed natural. Aspects of the "emotional revolution" such as sentimentality, personal confession, and primitivism indicate how "lesser writers first cultivated the genre and prepared the public taste for the master-work of Jean-Jacques [Rousseau]." The strongest evidence of a shift in literary values was in the libertine view of love that presaged the romantic attitude of the later eighteenth century.

Themes commonly associated with the Enlightenment included toleration of intellectual

and religious diversity, freedom from dogmatism, skepticism of revealed religion, cosmopolitanism, sympathy for the poor, exoticism, belief in progress and perfectibility, social and racial equality, and the utility of science. In part 2 ("The Broadening World of 1700") Atkinson and Keller find these themes in such obvious writers as Bayle, Montesquieu, the lesser-known Jean Buvat, and the anonymous authors of the *Journal des Sçavans*. While many motifs of the Enlightenment are audible in its prelude, the authors discern more emotionalism than rationalism, more that anticipates Romanticism than the Age of Reason.

Central to the authors' purpose is the attempt to pin the literary innovations of 1690-1740 to the taste of a growing bourgeois audience. Achieving unusual acceptance by all social classes, the intellectuals wrote "quite consciously, for the enjoyment of their fellow commoners, often with open or tacit disregard of the approved values of traditional aristocratic society." Their novels, which preached a plain ethic of benevolence and personal happiness, helped to make the daily life of commoners an object of sympathy.

The principal strength of this book is the careful selection and presentation of a popular literature of sentiment, exotic adventure, revelations of the bedroom, and even social protest. The authors discover that the literature of science and reason found a smaller audience than did the literature of *sensibilité*. But herein, too, lies the book's weakness. The authors fail to analyze the audience for these books beyond the amorphous middle class. Who bought which books and in what quantity? In his preface Professor Keller has forsworn a statistical analysis of book sales. But he makes the matter of the audience so crucial to his discussion of the literature, and he so tantalizes the reader in his conclusion with the information that Atkinson possessed "considerable data about writers, publishers, and public acclaim or failure" proving that "audiences were much larger and more bourgeois in their make-up than in the heyday of the reign of Louis XIV," that one is left begging for a social analysis of this public and its taste. One is tempted to propose that Professor Keller write yet a fourth volume.

ROBERT ISHERWOOD  
Vanderbilt University

CHARLES COULSTON GILLISPIE, *Lazare Carnot, Savant*. A monograph treating Carnot's scientific work, with facsimile reproduction of his unpublished writings on mechanics and on the calculus, and an essay concerning the latter by A. P. YOUSCHKEVITCH. Princeton: Princeton University Press. 1971. Pp. xi, 359. \$17.50.

This handsome volume offers a fresh perspective on Lazare Carnot, the "organizer of victory" in the campaigns of 1793 and 1794 and the only member of the Comité du salut publique to continue in power in the Directory. From the archives of the scientific academies of Paris and Berlin, Dr. Gillispie and Professor A. P. Youschkevitch have recovered forgotten papers that exhibit Carnot in his youth as a military engineer who wrote on mechanics and the calculus in the effort to win recognition in academic competition. His career in politics and government is better known than his real significance as a scientist whose work led to the creation of the laws of thermodynamics by his son Sadi. From the present point of view, his part in public affairs and military administration appears as an interlude; his role in the Terror and his directorate lasted about five years, 1792-97, and his service to Napoleon consisted of the defense of Antwerp in 1812-13 and the ministry of the Interior in 1815. On returning from exile to France in 1800 he resumed public scientific activity, serving on various commissions of the Institut de France, première classe, and publishing in the fields of geometry and mechanics. After Waterloo he went into exile again and died at Magdeburg in 1823.

In 1778 the Académie des Sciences of Paris offered a prize for the most precise statement of "la théorie des machines simples en ayant égard au frottement et à la roideur des cordages," with the proviso that the laws of friction and the effect of the rigidity of ropes be determined by new experiments applicable to naval machinery such as pulleys, capstans, and inclined planes; as none of the essays were satisfactory, a similar topic was announced for 1780. Carnot entered both competitions, and the theoretical section of each paper now published emphasizes the mathematics involved in raising weights, "the purpose of the greater number of all machines actually used." The context of the competition was clearly the three-decked man-of-war, with its numerous



heavy guns and elaborate rigging, requiring powerful hoisting gear, whether in action at sea or under construction or repair in arsenals. Summarizing these essays, Dr. Gillispie writes that Carnot generalized from the principles of action of certain machines to the principles of machines collectively, thereby "making physics out of the industrial reality of the age."

The third essay, presented in full, was entered for a competition established in 1785 by the Académie royale des sciences of Berlin calling for a theory of what is called *infini* in mathematics. Professor Youschkevitch's commentary relates this "Dissertation" to Carnot's later mathematical works as well as to earlier studies of calculus and points out that "a number of the remarkable ideas that Carnot failed to include in the final [published] version of his work have continued to be unknown." Taken in chronological order the three papers show a development in clarity, in the enunciation of principles, and in the definition of quantities; in short, a power of exposition and penetration that was not recognized by the examining committees.

Dr. Gillispie's work illustrates the maturing of the discipline of the history of science from an inclusive gathering of information concerning science and scientists to concentration on specific theoretical advances in the light of immediate circumstances. These three previously unknown documents reveal the widening generalizations that may be expressed, as Carnot himself puts it, in progressively simpler formulations, a kind of language that advances understanding even as it limits such insight to a narrower professional public. This book permits the reconstruction of one man's contribution to the science of mechanics and applied mathematics and thus adds notably to the intellectual history of these critical years. The effect of the whole book is enhanced by the photographic reproduction of crucial texts just as Carnot wrote them, without transcription, translation, or editing. There can be little doubt of what he meant, so clear is his language and his handwriting.

HARCOURT BROWN  
Brown University

GONZALO REDONDO. *Las empresas políticas de José Ortega y Gasset: "El Sol", "Crisol", "Luz"*

(1917-1934). In two volumes. (Colección Rialp, de cuestiones fundamentales, Number 15.) Madrid: Ediciones Rialp. 1970. Pp. 476; 608.

Ortega y Gasset was the dominant intellectual of Spain in this century; the translations of his works were best sellers in many countries. A professor of metaphysics, a journalist, publisher, parliamentarian, philosopher, and humanist, his influence was far reaching. Redondo deals with Ortega as a journalist whose role was inseparable from that of a politician, in a period of change from an oligarchic, liberal, semiconstitutional monarchy to a military dictatorship and then to a republic. Thus the author contributes not only to Spanish history but to the role of the intellectual in politics.

Ortega did not influence events only through his newspaper but was involved in the founding of two political groups. Unfortunately the book does not tell much about these organizations; it centers on the history of three newspapers (*El Sol*, *Crisol*, and *Luz*) that served Ortega and his friends as platforms and reveals the interaction among the editor and collaborators of a journal of opinion, the financiers who owned it, and the government that used subsidies and tariffs to pressure the owners and through them the editorial policy. The conflict between the independent, liberal intellectual and Azaña—the intellectual turned politician—and the realities of party politics in a divided society are analyzed. Ortega attacked the old politics of restoration Spain, hoping that the socialist would play the role of a labor party. Despite his Spanish nationalism he looked sympathetically upon Catalan regionalism. Criticism of the oligarchic two-party system led *El Sol* to find hope in the intervention of the army and Primo de Rivera, only to be soon in opposition to the dictator. The *dictablanda* that succeeded leads to the *Delenda est Monarchia*, the call for a republic, the loss of *El Sol* under the financiers' pressure, and the creation of a new paper, *Crisol* (soon transformed into *Luz*), four days before the proclamation of the republic. Ortega intervened constructively but critically in the constitution making and policies of the Left bourgeois socialist coalition, but seven months later he wrote "No es est," which expressed his disillusion. After giving up his regular journalistic activity he came out with a most insightful analy-

sis of the failures of the Left republicans but also with a prophetic warning to the Right of the mistakes it would make.

Liberal but elitist, conservative but committed to serious reform, opponent of clericalism but also of demagogic anticlericalism, Ortega was soon disillusioned by party politics, pettiness, and demagoguery. The call for a great national party, the appeal and faith in youth, and the critique of party politicians were elements in Ortega's thought reflected in the nascent fascism whose leaders expressed their admiration for him. With his "Viva la República" he still thought that the new regime remained the only alternative. A mixture of economic and political intrigue deprived him of *Luz*; defeated in his efforts to create a new party and to arouse those Spaniards he believed ready for a different style of politics, he returned to academia. This intensely political man, insightful and a perceptive critic, turned silent when he felt that he could not play a responsible and effective role. Less than three years later the Civil War started.

Anyone interested in the contradictions and tragedy of the sincere intellectual in politics, the relationship between economic and political power and the mind, and the difficulties of creating a civil society and democratic politics in Spain will find this book interesting, although long-winded. He will, however, miss an analysis of the relationship between Ortega the thinker and the journalist-politician, the response of others (except Azaña) to his endeavors, and his relationship to those who joined him in his efforts.

JUAN J. LINZ  
Yale University

EDUARDO BRAZÃO. *Présence du Portugal en Belgique (De Philippe d'Alsace à Léopold I.<sup>er</sup>)*. Companhia de Diamantes de Angola (Diamang), Serviços culturais: Dundo—Lunda—Angola. Publicações culturais, Number 80. Museu do Dundo, Estudos de história (ultramarina e continental). Lisbon: the Companhia. 1970. Pp. iv, 18—198.

To those acquainted with Professor Bailey W. Diffie's stimulating study, *Prelude to Empire—Portugal Overseas before Henry the Navigator* (1960), one of the more absorbing aspects of pre-Columbian Europe's developing interest in

maritime commerce was Portugal's role in establishing trade connections with the Low Countries. This subject, usually given but cursory treatment in existing accounts of the late Middle Ages and Renaissance, is now competently and thoughtfully explored by Senhor Eduardo Brazão, the former Portuguese ambassador to Belgium.

This work, originally published in Portuguese in 1969 and now in a French edition, traces the close and continuous commercial and cultural relationships between the Portuguese and Flemish from Portugal's rise under Afonso Henriques in the twelfth century to the ambassadorship of the famed Portuguese poet, Almeida Garrett, to the newly independent Belgium of the 1830s. Its purpose is to underline the strategic importance of Portugal's geographic position as Europe's Atlantic gateway and to emphasize the essential thrust given by early Portuguese-Flemish commerce to the eventual expansion of maritime discoveries sponsored by Henry the Navigator. Carefully researched, fully documented, and excellently illustrated with both photographs and color portraits, the study describes early Flemish interest in Portugal during the Crusades, the formalizing of ties between the two areas in the marriage of Philip of Alsace with Teresa, the daughter of Afonso Henriques, and the subsequent evolution of maritime trade between the Portuguese ports of Lisbon and Oporto with the Flemish ports of Ghent, Bruges, and Antwerp. Originally the midway point on routes from Italy to the North Sea ports and subsequently by virtue of its own much-desired products, Portugal established itself as a major entrepôt in Northern Europe's early trade network. Both in Bruges and in Antwerp its *feitorias* or trading posts became prominent foreign commerce centers that weathered the wars of the Counter Reformation. Their representatives were likewise appreciated foreign residents who developed friendly relationships between the two nations that have endured seven centuries.

Brazão's study attests admirably to the adage once expressed by Belgium's Leopold II that a nation is never small as long as it borders the sea. It also provides much valuable new information on the curious yet significant contribu-

tion of Portugal to the growth of European maritime commerce.

C. J. KOLINSKI  
Florida Atlantic University

VALDEMAR ANDERSEN. *Den jyske hedekolonisation* [The Jutland Heath Colonization]. (Skifter udgivet af Jysk Selskab for Historie, Number 24.) Aarhus: Universitetsforlaget. 1970. Pp. 246. 37.95 D. kr.

Reclamation of swamp, bog, waste, and forest lands in Europe matched rising populations from the 1750s through the whole of the nineteenth century. Its conceptual origins and progress denote, likewise, economic and social changes. Throughout Europe these efforts brought a mixture of populations; Spain, Prussia, Russia, and others sought the unwanted, the dissenters, and refugees from wars or repression, and these colonization efforts exceed the small example of the Jutland case. Denmark reclaimed lands, resettling both its own and German migrants by reason of economic factors alone, for the members of the Rente-kammer (tax office) thought that reclamation of the sandy wastes of a small portion of the west-central Jutland coast would profit the state. Immigrants recruited by J. F. W. Moritz from Pfalz, north of Heidelberg, were promised twenty years of freedom from taxation and military service, "day money" of varying sums, monetary assistance for the trip, and provisions to keep them alive. Many harassed Pfaltzers accepted, but the bleak heath of Jutland proved too forbidding and most colonists went elsewhere—to Schleswig-Holstein, Prussia, or other beckoning states.

The persistent colonists were mostly Danes and not Germans, and heath reclamation continued well into the last quarter of the nineteenth century under both private and public auspices. The area reclaimed in the eighteenth century was hardly proportionate to the costs; the assembled sum of 1,046,667 *rigsdaler* of 1783, while reasonable, averaged an enormous amount per acre. Yet these colonizations inspired a willing movement by Danes where costly effort, delayed rewards, and meager living were balanced only by promises.

The most conspicuous weakness of Andersen's book is that the concluding "European perspective" ought to have been a prelude to

explain and place in context this Danish example. It would also have been prudent to examine more fully cameralist and free-trade economic views of such writers as Erik Pontoppidan or J. H. von Justi to test the claim of mercantilist inspiration for the project, for the similar views of its chief supporters—A. G. Moltke and J. H. E. Bernstorff—cast doubts on Andersen's generalization. Likewise, the amount expended on the colonization would deny its value in mercantilist terms and would point to cameralism as its inspiration. The narrow confines of the work within the eighteenth century exclude the next century's major effort and even overlook the agricultural changes transpiring within Denmark simultaneously with the colonization scheme. In other respects the work is competent; an excellent bibliography, lists of immigrants, and reproductions of original documents enhance its value, even if the index is a sort of afterthought.

RAYMOND E. LINDGREN  
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JOSEF SEUBERT. *Untersuchungen zur Geschichte der Reformation in der ehemaligen freien Reichsstadt Dinkelsbühl*. (Historische Studien, Number 420.) Lübeck: Matthiesen Verlag. 1971. Pp. 72. DM 15.

Nothing of great moment marks the history of Dinkelsbühl in the late Middle Ages. An average city of about four thousand with a modest *contado* in the Swabian-Franconian region, comfortably wealthy from textiles and metal wares, and possessing a stable mixed government of patricians and guildsmen, the little imperial city entered the Reformation era with typical ecclesiastical conditions suggesting the need for change: too many clerics, an arrogant monastic provost exercising the *jus patronatus* over municipal parishes, and general dissatisfaction with the Church's spiritual guidance. By 1523 Luther had followers among leading tradesmen and politicians, and in 1525 the council began to legislate for the clergy. Widespread citizen support for rebelling peasants brought a brief backlash in 1525 (as it did in nearby Rothenburg), but under the influence of the Lutheran preacher Bernhard Wurzelmann the new religion was firmly established by government action in the 1530s. From then

until the Schmalkaldic War Dinkelsbühl was officially Lutheran, although the old religion never ceased to be practiced. In the reaction following the emperor's victory the government was lodged in the hands of conservative Catholic patricians, and in 1555 parity was established in religion but not in politics, from the exercise of which the Lutherans were excluded. Their ecclesiastical ordinance of 1572 was a unique example in Germany of an evangelical church organization independent of political authority.

Seubert's little monograph tells all this in efficient prose but offers little beyond the recital of familiar events. The "investigations" of his title are nothing more than descriptions of incidents in the Reformation in Dinkelsbühl. At the most interesting junctures in the story (the active support given to the besieging peasants by substantial numbers of burghers, reasons for religious choice among citizens, the impact on society of the new marriage court, etc.) the author avoids analysis or confesses inability to explain. Beyond a certain usefulness as an outline of events and a handy reference to the sources, the book seems to serve no real purpose.

GERALD STRAUSS  
Indiana University

CARLA KRAMER-SCHLETTE. *Vier augsburger Chronisten der Reformationszeit: Die Behandlung und Deutung der Zeitgeschichte bei Clemens Sender, Wilhelm Rem, Georg Preu und Paul Hektor Mair.* (Historische Studien, Number 421.) Lübeck: Matthiesen Verlag. 1970. Pp. 95. DM 18.

Carla Kramer-Schlette presents a highly schematic, comparative study of four Augsburg chroniclers living at the outbreak of the Reformation. The work is conceived first in terms of the chroniclers' presentation of objective material bearing on the social, political, and constitutional relations of the city and, second, in terms of the subjective cultural worth of the sources according to the consciousness of the authors. Whatever historical value the chronicles possess lies in the latter realm.

With a single exception there are no surprises here. The chroniclers ignore motives and context. One seldom obtains a glimpse of the internal power structure and its relationships.

Even the new religious ceremonies and doctrines, although supported by all but Clemens Sender, are reported vaguely. Our questions are obviously not theirs. Their nominalistic perception usually fails to penetrate beyond the externals of an event: for Georg Preu the *reformacion* is principally the destruction of idols. Occasional vignettes emerge: the aged Emperor Maximilian on his knees at mass; Charles V, bare headed, in the Corpus Christi procession; the description of a brutal execution. It is still the world of Huizinga. And yet the relieving influences of humanism are curiously absent. The chronicles ignore Augsburg's culture and Conrad Peutinger is only mentioned to be attacked for self-serving. Preu, himself a painter, delights in the iconoclasm. In the bibliography of the monograph one notes the absence of Paul Joachimsen's study of Sender's predecessor, the Benedictine monk-humanist and Augsburg chronicler Sigismund Meisterlin, as well as Joachimsen's great work on humanism's impact upon German Renaissance historiography. The problem of humanism's presence or absence at least warrants formulation. Excellent in its close analysis, the monograph yet suffers from being too narrowly conceived.

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GERHARD MASUR. *Imperial Berlin.* New York: Basic Books. 1970. Pp. vii, 353. \$7.95.

Of all the capitals of modern Europe only Berlin leaped suddenly to the pinnacle of fame and power. A disunited Germany had known many cities of eminence and cosmopolitan elegance; Berlin was not always one of them. In the 1850s Berlin's economic growth quickened, and after 1871 her new place as the capital of Imperial Germany sealed her importance in the world.

Berlin was a superb example of the old and the new existing side by side; the court remained, the bourgeois plutocrats arrived, as did tens of thousands of immigrants from the country seeking employment. Jews struggled to establish themselves in the wake of full emancipation. Here was a city of conflicts and problems, of cultural ambition and garish ostenta-

tion. Here is a topic that might attract historians of different genres: a modern historian would explain this strange amalgam from a new perspective and with new techniques, while an older type of historian might try to recreate the atmosphere of Berlin, to sketch the conditions of society and the rise of its artistic and intellectual life. We need studies of all kinds.

One would have expected Professor Masur, who spent his early years and university days in Berlin, to give us an elegant version of the older type of history. His work, however, is marked by such serious faults of organization, substance, and style that it possesses few of the virtues and many of the weaknesses of this earlier genre.

Professor Masur has written a short book based on published sources; he did not turn to archival or statistical material. He endeavors to cover all facets of Berlin's life, and his book is a useful introduction to the city's economy, society, and high culture. He offers a kind of generalized Baedeker, but his scholarly aim and method are not clear. "This is not an economic history of Berlin," he writes, but neither is it a social history of the city, for it says little about how different classes lived, worked, played, or thought. He makes the important assertion that "in the second empire Berlin became an a-religious city," but he does not elaborate on this and in this apologetic epilogue merely reiterates his assertion and contends "that in consequence that phase of Berlin life could be ignored without loss." Can it? It is an *aperçu* that should have been bolstered by fact and illustration and enriched by reflection. Even the Youth Movement—born in Berlin—is dealt with in one inadequate paragraph. Much space is devoted to a conventional summary of German political history, especially during the Great War. Masur's judgments in this realm lack freshness and imagination. To conclude, for example, that "it is difficult to see that Ebert had a choice unless he had wanted to become another Lenin," is to be content with Ebert's own perspective on his predicament. Have we learned nothing in the intervening half century?

As one would expect, Professor Masur, the author of *The Prophets of Yesterday* (1961), dwells on Berlin's varied cultural and artistic

life. As in the rest of the book there is much here that is informative and pleasantly anecdotal. Theodor Fontane certainly was the most interesting Berlin writer of the time, but Masur's fifteen pages on him constitute a mixture of biography and plot analysis not an interpretative essay on Fontane's role and work. It is surprising that someone so familiar with Ranke would have devoted but one—conventional—page to him. Finally the reader misses a treatment of Berlin's popular culture. What did the different groups or classes read? The book is consistently idiosyncratic without being consistently interesting.

*Imperial Berlin* is written with ease, but occasionally an imprecise phrase or stylistic lapse ("the emotional subjectivism of the romantic movement now somersaulted and embraced the collective order") mars the text. The best passages of this book are anecdotal and autobiographical, and one regrets that Professor Masur did not choose to write the recollections of a cultured, upper-class Berliner; perhaps he could have done for Imperial Berlin what Stefan Zweig in *The World of Yesterday* (1943) did so poignantly for prewar Vienna.

FRITZ STERN

Columbia University

GIUSEPPE TALAMO. *La formazione politica di Agostino Depretis*. (L'età del Risorgimento. Studi e testi, Number 7.) Milan: Dott. A. Giuffrè Editore. 1970. Pp. 246. L. 2,400.

Until recently Italian historians have rarely regarded the writing of biography as an important scholarly endeavor, and this work by Giuseppe Talamo gives cause to fear that the art may not yet be understood in Italy. The subject, Agostino Depretis, virtuoso and founder of the practice of *trasformismo*, overshadowed an entire decade of Italian history. Much is known of his parliamentary dictatorship from 1876 to 1887, but little has been written of the preceding sixty years of his life. Talamo seeks to fill this gap, but the result falls far short of the goal.

This is a set of five crudely linked and badly structured essays, each with appended documents, on as many segments of Depretis's life and family background that carry us to the turn of the century when he was thirty-eight years old. The first study is on the establish-

ment of the Depretis family (Agostino and his father Francesco) as landowners of growing wealth, and the second purports to treat Agostino's administration of the lands of the wealthy Piedmontese family of the Gazzaniga Arnaboldi. Essentially what emerges is that Depretis was of Lombard origin, not Piedmontese as has long been believed, and that in 1843 he conceived a modern scheme for the management of the Gazzaniga holdings. But these are needles to be dug from Talamo's haystack. The author has presented information germane to the formation of the future statesman, but it is in the form of ponderous, raw statistical material, the type of data readout one would get from a computer programed to report on the family's economic and business activities. Such information is little more than a tiny insight into the minutiae of agrarian economy in one small region; on the future prime minister we have precious little. While Depretis's birth registry is faithfully reproduced in the original Latin, he appears again only as an adult, an illustration of the author's tendency to omit biographical information. Why have we no account of his formative years? Not one word has Talamo provided on Depretis's education, or *cultura*, something Italians have always regarded as a *deus ex machina* in their leaders. Was he indeed a lawyer as is generally believed? If so, more than incidental mention of the fact is crucial to his political formation.

In the next two essays on Depretis in Parliament in 1848-49 and as mayor of Stradella in 1849-50, Talamo veers a bit closer to his subject. We are told, almost incidentally, that Depretis's experiences in the decade prior to 1848 slowly matured him and that he fought for broader application and more liberal interpretation of the *Statuto* in the Kingdom of Piedmont, that he was a democrat. In short Depretis is quite clearly delineated in his Left convictions of the moment, but Talamo concludes here with a tantalizing mention of the *connubio* and nothing more. Hence, apparently on the threshold of allusion to Depretis's reactions as a deputy to that first form of *trasformismo*, the author leaves his reader to speculate.

The chapter on Depretis as mayor of Stradella is actually a three-part essay in which only the last section deals directly with the subject. The remainder is, first, a socioeconomic

study of the town with pedantic lists of its "zoo-technical patrimony" and, second, a brief account of Depretis as city councilman. The material is neither uninteresting nor unimportant, but Talamo's handling of it lacks coherent narrative.

The last essay treats the founding and brief life of the *Progresso*, a Turin newspaper in which Depretis was actively interested in 1850-51. Here again Talamo knits ideas together a bit better, but he fails to tell much that is not already known about an obscure subject. The *Progresso* had links with Mazzini, we learn, but so did various other contemporary periodicals. Would we not be more aided by a study of Depretis the publicist that is broad enough to include also his work with *La Concordia* in 1848 and after?

Talamo's work is excessively fragmentary; depth studies of moments do not elucidate decades. The writing is frequently turgid, and the author often yields to trivia and cannot resist engulfing his reader with oppressive documentation that hinders an already leaden text. Often he drifts into digressions from his already inappropriate title, and there is one supporting document of no relevance (a memoir on agricultural conditions [pp. 70-76] in Depretis's home region dated 1879, more than thirty years beyond the most recent reference to the subject).

Making fair allowance, as indeed we must, for the author's promise in his preface of a later evaluation of this material, the volume still remains a frustration. It is frustrating because so much painstaking research has gone into the task, but it smacks of the assiduousness found occasionally in an industrious but unimaginative graduate student. Talamo has taken a figure who still appears stodgy in Italian history, and so he has left him. Even if Depretis did govern in a period of "dry-as-dust," his stature was such that someone should be able to bring him alive. Yet Talamo leaves all this to the reader's fantasy.

In his introduction the author notes that parts of his book originally appeared in scholarly journals, but he does not inform us that chapters 1, 2, and 5 were first published as *dispen- se* or university lectures (*La formazione di Agostino Depretis* [1968]). Indeed what is wrong with this book is that it maintains the

classroom format. Comparison with that first edition is sadly enlightening, for little has been changed beyond an occasional word or, worse, paragraphs and entire pages shifted from one chapter to another. At least the addition of an adjective to the book's title clarifies matters slightly, but all remains raw and pedantic. Yet Talamo is listed as the author of a forthcoming biography of Depretis in the *Unione Tipografico-Editrice Torinese* series. Perhaps, as the subtitle of the 1968 edition suggests, this is nothing more than notes for a biography. If Talamo's research continues in this depth, a polished complete biography might be outstanding; if he publishes it in the present form, one volume will not suffice and Depretis will still await his biographer.

Another aspect of this work is ironic and tragic. Produced as lectures at the University of Rome in the disastrous year 1967-68 and required for examinations in Risorgimento history, these pages are a document that illustrates and perhaps justifies complaints of students rebelling against the present curriculum.

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University of Kansas

LUCIO AVAGLIANO. *Alessandro Rossi e le origini dell'Italia industriale*. (Università degli Studi di Salerno. Collana di studi e testi, Number 2.) Naples: Libreria Scientifica Editrice. 1970. Pp. 554. L. 6,000.

Alessandro Rossi (1819-98) was the founder of one of Italy's greatest establishments of the nineteenth century, the woolen manufactory of Lane-Rossi, which employed on the eve of the twentieth century over five thousand persons. Only a small portion of this volume is, however, devoted to a business history of this famous concern or to the early industrialization of Italy. Consequently in the scant fifty pages devoted to the business, attention to economic questions is both sketchy and elementary. To an economic historian this is a pity, the more so because the author apparently had access to the pertinent information, some of which he put in that half of the work given over to documents and tables.

Mr. Avagliano was clearly more interested in the public career of Rossi than in his business affairs, and his choice of emphasis is under-

standable. Rossi was one of those early entrepreneurs who, like Robert Owen, was deeply concerned with the welfare of his workers and of society in general. Thus one is not surprised to find that he was an exponent of a guild organization of the state, Catholic social welfare movements, model housing for his workers, the education of youth, and mutual aid societies. In politics he was at first a Cavourian liberal who opposed the old Destra, but he later supported Crispi and worked for the protective tariff of 1887. He was active in local politics at Padua, served as a deputy, and was then appointed to the Senate.

This biography touches on a multitude of questions that confronted Italy in the last third of the nineteenth century, but it does not take a hard, clear look at any of them. Its virtues are that it has a great amount of detail, which will make it valuable as a reference book, and a wealth of leads to business archives.

SHEPARD B. CLOUGH  
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C. DAICOVICIU *et al.*, editors. *Acta Musei Napocensis*. Volume 7. (Comitetul de Stat pentru Cultură și Artă.) Cluj: Muzeul de Istorie. 1970. Pp. xvi, 668.

This most recent collection of writings on Transylvanian history by the scholars of Cluj is, like previous volumes in the series, weighted toward the pre-Roman and Roman periods and various aspects of the national movement between the latter part of the eighteenth century and the end of the First World War. This emphasis reflects, on the one hand, both the interests of the editor, the distinguished classical archeologist and historian, Constantin Daicoviciu, and the intensive study of ancient history at Cluj and, on the other, the increased nationalist orientation of Romanian historiography in the last decade. Only brief mention may be made here of a few of these and other articles.

In the first group, Valentin Vasiliev describes pieces of jewelry discovered recently in Scythian graves that suggest the need for a fresh look at the problem of the Scythians in Transylvania; Iudit Winkler describes the chronology and types of the earliest Geto-Dacian coinage; Hadrian Daicoviciu makes a number of interesting observations on Trajan's

first war with the Dacians; and I. I. Rusu continues his series of "epigraphical notes" on Greek and Roman inscriptions. In the second group, Aurelia Bunea presents a well-documented study of how the Romanian question in Hungary was treated in the Romanian parliament between 1892 and 1899. She concludes that the nationality policy of the Hungarian government helped to turn Romania away from the Triple Alliance toward the Entente. Acațiu Egyed has written a brief but enlightening article on the emigration of peasants from Transylvania between 1900 and 1913. He analyzes their social status, reasons for leaving, destinations—the great majority went to the United States—and the effects on Transylvania of this sizable exodus. Finally, Dumitru Suciuc discusses critically the attitude of the French press toward the union of Moldavia and Wallachia in the period from 1856 to 1859.

There are, in addition, two short but important articles dealing with the period of the *Völkerwanderung*: a description by Kurt Horedt and Dumitru Protase of a late fifth-century treasure discovered near Cluj that may be Ostrogothic rather than Gepid and may, therefore, necessitate a reassessment of Germanic settlements in the area and an analysis by Ștefan Ferenczi of two jars dating from the end of the sixth or the beginning of the seventh century that may be the oldest traces yet discovered of Slavic settlement in Transylvania. Mention must also be made of Magdalena Bunta's extremely interesting and original contribution to the history of the Anabaptists in Transylvania in the seventeenth and eighteenth centuries and Marius Porumb's study of selected sixteenth-century icons in Maramureș and northern Transylvania.

The volume also gives some attention to more recent historical developments, but in these articles the influence of current political trends is clearly evident. Gheorghe Bodea, for example, provides much useful information on the establishment of a democratic regime in northern Transylvania in 1944-45, but he is mainly concerned with stressing the leading role of the Communist party and the cooperation of Magyars and Romanians. The result is, inevitably, a one-sided piece that ignores diversity of opinion and controversy and never gets

to the heart of the age-old nationality problem in Transylvania.

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DAVID JORAVSKY. *The Lysenko Affair*. (Russian Research Center Studies, Number 61.) Cambridge, Mass.: Harvard University Press. 1970. Pp. xiii, 459. \$13.95.

If one wonders why this study by a historian on the genetics controversy is reviewed by a political scientist in a historical journal, the answer is quite simple: the Lysenko theories, purported to be genuine, turned out to be a quack science supported primarily by Soviet authoritarian politics that, in turn, have been subject to historical inquiry.

American scholars who study closed societies are always searching for cracks or holes in the political walls through which they might observe and analyze political cultures manipulated by authoritarian elites. The Soviet Union has such a society, and Professor David Joravsky is an American scholar who found a "hole," the Lysenko affair, through which he chose to observe and analyze "the evolving interaction of agriculture, natural science, ideology, and political power" in the Soviet Union. The prime rule of this case study, according to Joravsky, "is to determine the political unknown by its interaction with strategically selected knowns, as they are evidenced in the public record." Joravsky did so, and as a result he tried to answer two basic questions: "How can we distinguish Soviet ideology from other types of thought, and, how can we be precise and factual in analyzing ideological influences in Soviet history?"

Unlike some of the earlier writers on this subject (for example, J. S. Huxley, Conway Zirkle, and John Langdon-Davies) Joravsky tries to refute the allegation that Lysenkoism was a direct outgrowth of Marxist theory in the 1930s. According to Joravsky, "the most well-known Lysenkoite writings lay overwhelming stress on agricultural practice as the chief source." And since the criterion of practice is the chief principle of Soviet ideology, Joravsky equates Lysenkoism with Soviet ideology rather than with Marxist theory. While it is plausible



to argue that Soviet ideology was and still is geared toward "the criterion of practice," it is equally justifiable to claim (as Joravsky does) that Soviet agriculture suffered for thirty-five years because of Lysenkoite practices. If so, why did the Soviet leaders, with the exception of Brezhnev, permit this setback in Soviet agriculture? The answer, at least to me, is obvious. When the Lysenko controversies reached a high plateau in Soviet scientific circles, the issues involved in the controversy were so close to the sensitive ideological pole of communist dogma that it was imperative for the Soviet elite to render interpretations based on Bolshevik morals. The question raised by the issues was not which theories would lead to greater agricultural output in the Soviet Union but which theories were more compatible with the dogma when applied to agriculture and genetics. In the final analysis, the issues were reduced to Soviet socialist science versus "decadent bourgeois science." Of course, as in many other cases, the interpretation rendered by the party was consistent with Soviet hypocrisy. While many Soviet agricultural scientists were paying lip service to Lysenkoite theories, at the same time they were practicing "bourgeois science" in agriculture and genetics. Soviet leaders tolerated this situation because the increased yield of corn, wheat, and other crops in such areas satisfied their ideological interests.

The creator of "agrobiolgy," Lysenko persuaded Stalin and his successors to accept his approach to Soviet agriculture because it could render the desired increases in farm yields at little or no cost. Thus, from 1929 to 1964, with a few ups and downs, Lysenko had the support of the party and its auxiliaries to impose his "scientific methods" not only on collectivized peasants but also on scientists, first in the preparation of seeds and in the science of plant physiology, then in plant breeding and genetics, and finally in a wide range of agricultural techniques and related sciences—from afforestation to the use of fertilizers, from cytology to soil science. As a result the damage to Soviet agricultural science was enormous. This aspect of Lysenkoism, however, is only of secondary interest to Professor Joravsky. His primary concern is the political repercussions that followed the Lysenko controversies under Stalin and Khrushchev. Therefore, he examines the volu-

minous public record that abounds with evidence of change in policies but offers little insight into the secrecy of high Soviet politics. It is perhaps for this reason that Professor Joravsky does not contribute much to our knowledge of the period of the struggle for power after Stalin's death. Although he cites the V. S. Dmitriev case, its political significance is not fully explained. Nor does he explain the criticism of Lysenko launched in 1954 by Malenkov's protégé, Khrushchev. Professor Joravsky claims that from the end of 1954 until his ouster in 1964, Khrushchev had supported Lysenko in all his endeavors. The evidence, however, suggests that it was not until December 1958 when Khrushchev, *Pravda*, the presidium of the Soviet Academy of Sciences, and *Izvestia* assailed the editors of the *Botanicheskii zhurnal* for criticizing Lysenko. From 1952 until 1958 Lysenko was systematically refuted by the editors of the journal. Although we have no proof that such criticism was instigated by N. S. Khrushchev, we do know that he did not stop this criticism until 1958.

From a historical point of view, one can draw at least two valuable lessons from Joravsky's study of Lysenkoism. One is that there has been a tendency in this country to oversimplify the relationship of Soviet science to Soviet ideology. In this respect Lysenko did almost as much harm to American space science as to Soviet biology, for he encouraged skepticism in the United States about scientific achievements in the Soviet Union. A second point is that the basic task of Soviet science is to facilitate the universal development of the productive forces of the country. It is a dutiful and fully subordinated tool of the elite in power. Thus, when the Soviet leaders felt the need for the services of the scientists, they were willing to rehabilitate those having committed "treason."

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#### NEAR EAST

G. E. VON GRUNEBaum, editor. *Theology and Law in Islam*. (Second Giorgio Levi Della Vida Biennial Conference, May 9-10, 1969, Near Eastern Center, University of California, Los Angeles. Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Wiesbaden: Otto Harrassowitz. 1971. Pp. 105. DM 48.

The occasion for *Theology and Law in Islam* was a conference held in May 1969 to mark the bestowal of the Giorgio Levi Della Vida medal upon the late Joseph Schacht, and the six papers published here are the proceedings of that conference: Schacht himself on "Theology and Law in Islam" (pp. 3-24); W. Montgomery Watt, "The Great Community and the Sects" (pp. 25-36); Walther Braune, "Historical Consciousness in Islam" (pp. 37-42); Gerhart Ladner, "Reform: Innovation and Tradition in Medieval Christendom" (pp. 53-74); George Makdisi, "Law and Traditionalism in the Institutions of Learning of Medieval Islam" (pp. 75-88); and Fazlur Rahman, "Functional Interdependence of Law and Theology" (pp. 89-97).

The papers of Schacht and Makdisi taken together summarize the interrelationship between jurisprudence and Islam's dialectical theology (*kalam*) from the beginnings down to the twelfth century, and it is immediately apparent that comparisons are difficult. Jurisprudence reached maturity in the early ninth century when *kalam* was approaching adolescence. It was the law, too, that dominated Muslim institutions of higher learning, and theology came into them, as Makdisi points out, by a side entrance—through the academic sermon. In this connection Makdisi gives an instructive summary (pp. 77-79) of the fundamental differences between the Muslim *madrasah* and the European university.

Rahman's approach to the same question is not so much historical as ideological. Jurisprudence is a science of the particular act and *kalam* of the universal principle. There never developed between them, Rahman regrets, a system of moral philosophy. In my opinion, the conclusion appears to render something less than justice to both Muhasibi and Ghazali.

Watt is interested in the heresiographies and their presuppositions, and the greater part of his paper is given over to tracing the history of some of the names put upon various sects, generally by their opponents but occasionally, as in the case of the Shi'ites, by the partisans themselves. The example of the Shi'ah, though offered only in passing, is particularly interesting, but it is the only place in the volume where the issue comes up. The notorious problems of Shi'ite theology and Shi'ite traditions

have been addressed by very few Western scholars, and that unhappy fact is painfully illustrated in this collection.

One of Watt's theses is that Western scholars look for "progress" in the matter of intellectual history while the Muslim more often finds reprehensible "innovation." This may be true of the traditionalist Muslim lawyer, but Braune attempts to nuance Islam's view of history by adducing the contrasting attitudes of the pre-Islamic poets, the Prophet, the Mu'tazilites, and the Sufis. Braune makes the case that the Mu'tazilites were Islam's chief apostles of man's historical self-realization and consequent reform, while Sufism took men out of history.

Ladner's portrait of medieval Christendom is quite different, with the monastic orders in the van of reform movements. Ladner does not make a point of them, but the differences are provocative for an Islamicist, and it is to be regretted that the editor, as so often in the past, did not in this instance publish the participants' comments on each other's papers.

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GEORGES I. BRATIANU. *La mer Noire: Des origines à la conquête ottomane*. (Acta Historica, Number 9.) Munich: Societas Academica Dacoromana. 1969. Pp. 394. DM 38 (\$9.50).

This volume, written some two decades ago by Professor Bratianu and now published for the first time, represents an important addition to our understanding of the Black Sea during ancient and medieval times. It also makes us regret even more the tragic end of its author after he fell into the hands of his political opponents following World War II, when he was still in his prime.

The book itself is divided into four parts or sections. The first begins with a description of the special physical and geographical characteristics of the Black Sea and the areas that surround it and then surveys its history in ancient times until the end of the early Roman Empire. The second portion deals with this region from the third through the twelfth centuries A.D.—which might be called a late Roman-Byzantine age. The third section concerns itself with the years between the time of the Fourth Crusade and the mid-fourteenth century, which the author calls a Mongol-Italian period and

with which he has dealt extensively in an earlier book. The last portion tells the story of the two centuries between 1300 and 1500 A.D., which saw an intensive rivalry for Black Sea mastery between Venice and Genoa and the first conquest of the sea and its shores by the Ottoman Turks.

As one would expect from a knowledge of the author's earlier work, there is a heavy emphasis upon social and economic factors in the history he relates, although political developments are not slighted. It is also worth noting that this remarkable work of synthesis is placed in a world setting that relates events taking place in the Black Sea region with those affecting the wider world of Eurasia and Africa. One is also struck by the breadth of Bratianu's knowledge and understanding of the historical process and the wide range of his scholarship, which makes this book as fresh and timely as if it were written yesterday.

In short here is historical synthesis at its best, beautifully written by a scholar who is able to explain his own chosen area of Europe and Asia so that it not only throws light upon other regions during ancient and medieval times, but at the same time deepens our understanding of history in general.

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MICHAEL CURTIS, editor. *People and Politics in the Middle East*. (Proceedings of the Annual Conference of the American Academic Association for Peace in the Middle East.) New Brunswick: transaction Books; distrib. by E. P. Dutton, New York. 1971. Pp. 325. Cloth \$8.95, paper \$3.95.

The motivation of this volume and of the conference at which most of the papers were first presented is the belief that analysis of various aspects of the Arab-Israeli conflict will contribute to its peaceful resolution. Some of the contributors approach the objective somewhat indirectly. S. G. Haim's contribution is a useful summary of recently available Arab information concerning the origin of the Ba'th party. Two studies, one by H. H. Smythe and Sandra Weintraub and the other by Yochanan Peres, treat ethnic and cultural groups in Israel. The former views the conflict between

Orientals and Europeans more seriously than does the latter. The Peres study uses survey data to deal with sophisticated and important theoretical questions. Eliyahu Kanovsky challenges most current estimates of recent economic developments in Egypt, Israel, and especially Jordan.

The approach to the objective is more direct in the remaining eleven essays and in the accounts of the three discussion periods. Several subjects recur throughout, even in essays whose chief concern lies elsewhere. Doubts of the Palestinians' claim to be an entity are raised by Ben Halpern (whose chief thesis is that Israelis and Arabs have borrowed each other's ethical arguments), Marie Syrkin, Samuel Merlin, and Yehoshafat Harkabi. Their aim is to deny the need of the Palestinian Arabs for all of former Palestine—that is, the elimination of Israel and the reconstitution of Palestine—in order to realize their national existence, since the Palestinians, as Arabs, can enjoy national existence in a large Arab territory. Don Peretz argues for the legitimacy of a Palestinian entity. We may add that the present insistence on the Pan-Arab nature of Palestinian nationalism seems strange in view of the former objection to the involvement of non-Palestinian Arabs in the Palestinian question. Furthermore, the Palestinians today are giving the same answer to the question of local patriotism and Pan-Arabism, which are not regarded as contradictory, that other Pan-Arabs did from the 1920s on. Syrkin and Merlin, as well as Peretz, believe a "Palestinian State," that is, the Jordanian west bank, preferably with the addition of the east bank, can facilitate the ultimate settlement. Most of the contributors with Israeli sympathies believe that Arab refusal to recognize the existence of Israel is independent of Israeli policy or action. The exceptions are Peretz and even more Merlin, who is critical of aspects of Israeli internal politics and argues that Israel cannot claim to be willing to negotiate with the Arabs in good faith and without preconditions. Jon Kimche thinks, erroneously in my opinion, that the British from 1918 on deliberately prevented Arab-Jewish cooperation. Amos Perlmutter offers an up-to-date version of the familiar criticisms of Dulles and American cold warriors in general. He is often penetrating when dealing

with particulars, but his treatment suffers from the usual combination of snippets of historical information with an interpretation that is no more convincing than was the theory of the cold war social scientists he criticizes. Shlomo Avineri explains how New Left critics can be won over to Israel by showing that Israel, not the Palestinian Arabs, is the analogue of North Vietnam. Peretz probably would disagree. Avineri's discussion ignores the vital effect of British military force on the relative positions of the Jewish and Arab communities in Palestine. In any event Arab, not Israeli, membership in the universal church of peoples' national liberation has been certified by the prelates—the bishops of Havana, Peking, and Hanoi.

Representing Arab views, Abdul Aziz Said believes that the post-1967 Palestinian movement is a new and truly revolutionary phase in Arab history, and F. J. Khouri argues that Israel could agree to the return of the Palestinians without endangering its security. F. H. Hinsley believes, with good reason, that only the powers can bring about a settlement of the Arab-Israeli problem and, with less apparent reason, that there is an emerging agreement among the powers.

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R. HRAIR DEKMEJIAN, *Egypt under Nasir: A Study in Political Dynamics*. Albany: State University of New York Press. 1971. Pp. xvi, 368. \$10.00.

At first glance this bulky volume may appear to be a rather forbidding exercise in the application of concepts from sociology and political science like the "routinization" and "legitimation" of the charismatic leader's rule. But the reader who perseveres soon finds that Professor Dekmejian has written a lucid and readable analysis of interest to many besides the professional social scientist. Drawing both on Arabic materials and on studies by Western scholars, he exposes the shallowness of supposing that Gamal Abdel Nasser was a second-rate fascist dictator or the dupe of the Kremlin. He argues persuasively that Nasser was the greatest Arab hero since Saladin and Muhammad and that

his popularity among Egyptians survived almost unscarred a long list of failures, from the defeat by Israel in the Suez War of 1956, through the secession of Syria from the United Arab Republic in 1961 and the disastrous involvement in Yemen's civil war ("Egypt's Vietnam"), down to the Six Day War of 1967. Nasser's offer to resign at the close of that humiliating conflict was rejected by acclamation, and his death three years later was the occasion for mass mourning of an intensity matched in recent history only by Indian grief for Gandhi and Nehru.

How are we to account for this extraordinary record of nothing succeeding like the absence of success? Because, Professor Dekmejian contends, Nasser endowed the abstractions of Arab nationalism with a new psychological reality and still more because his reiterated stress on the need for dignity gave the Egyptian peasant a chance to identify with the Nasser government in contrast to his traditional hostility toward his rulers. Yet, as the author also observes, "charisma can only inspire men, it cannot organize them" (p. 250). He argues that the year 1961 marked the watershed of Nasser's domestic policy, as the Syrian secession led him to complete the organization of a sweeping program of "Arab socialism," which owed as much to Islamic traditions and democratic European examples as it did to Marxism. At the same time more determined efforts were made to create a viable mass party, the Arab Socialist Union. Its vicissitudes and the shifting roles of the military and academic elite in Egyptian cabinets (many ministers had Ph.D.'s) are followed in considerable and informative detail.

Throughout the book Professor Dekmejian (who is of Armenian stock) is no uncritical hero-worshipper. He notes, for example, that Nasser made General Neguib an "unperson" as soon as he had maneuvered him out of the presidency in 1954. And he blames the fiasco of the Six Day War in part on the backfiring of Egyptian "brinkmanship" based on Nasser's underestimation of the intensity of the Israelis' nationalism and their disposition to seize the boldest of the options open to them.

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## AFRICA

J. DESMOND CLARK. *The Prehistory of Africa*. (Ancient Peoples and Places, Volume 72.) New York: Praeger Publishers. 1970. Pp. 302. Cloth \$10.00, paper \$3.95.

Since the publication of H. Alimen's *The Prehistory of Africa*, translated by A. H. Broderick (London, 1957), archeologists have taken great strides in Africa. A new survey was overdue. But this book is not merely a survey brought up to date; it is a work that shows a bold change in conception over all former textbooks. No longer is this a record of man's tools but a history that weaves the physical and cultural evolution of mankind into a single story. Hardly anyone but Desmond Clark could have succeeded in this venture. And no story could be more stirring than man's emergence from the apes and his slow but accelerating evolution ever since.

The book opens with a chapter on methodology and then follows the main chronological sequences: the australopithecine and *Homo habilis*; *Homo erectus* (the unspecialized hunter); modern man (*Homo neanderthalensis*, *Homo rhodesiensis*, *Homo sapiens*); the specialized hunting and gathering societies; the farmers and present-day people. The most crucial step in man's evolution is the emergence of modern man to whom the author attributes the invention of speech and ritual and the discovery of fire between 70,000 and 35,000 years ago.

One admires Clark's virtuosity and breadth of knowledge that cover many disciplines in the natural, physical, and social sciences. Here and there are some weaknesses—in linguistics, perhaps, or in psychology—but even so there are no real lapses. Truly *Homo mirabilis* wrote this account!

For the earlier periods a few remarks must suffice since the historian is most interested in the last chapter of this book. The first warning must remain that this is a case where there are too few skeletons in the closet, especially when one deals with modern man and later ages. Human remains are too few to allow us to term as "racial" the subtle variations found. These variations are not gross anatomical differences as they were during the early evolution of man. They can fall well within the range of variability of modern populations.

Also, there are not yet enough excavations to feel confident at all points that the distribution maps given will not undergo major changes as time goes by. A splendid example is the case of the channeled and dimple-based potteries associated with the coming of iron to Equatorial, East, and Southern Africa. Since this book was written, there has been at least one stylistic study that shows a dozen subvarieties in this "co-tradition." But more important, sites are now known on the lower Congo, on Lake Leopold II, and at least a few shards have come from Kinshasa so that the whole distribution is altered. At all times, therefore, the reader must remember that Africa's archeological exploration is still in its infancy or adolescence. He must also remember Clark's own words in his first chapter about the need for interpretation of data and therefore the possible fallibility of some interpretations.

The last chapter deals with the initial stages of food production, the spread of domestication (especially in East Africa), the spread of the Bantu, metallurgy, and Iron Age societies. One cannot agree with the statement on page 193 that pharaonic government is very reminiscent of the rule of the despots of later times on the upper Nile and in West Africa. This is in a moderate form a revival of the "Sudanese kingship" theory, which will not hold water. The pharaohs remind one just as much of Javanese monarchies or early Indian states.

Attention is drawn particularly to Clark's treatment of the early date for cattle in the Sahara and to his ingenious proposition that this may indicate either independent domestication in North Africa or, perhaps more likely, an introduction by sea from Italy to Tunisia by the middle of the fifth millennium. Only further data will allow us to decide whether either of these two theories is correct or whether we should stick to the theory of diffusion from the Middle East.

On the spread of agriculture a few major points must be mentioned. First, of all the domesticated crops (see the map on page 212 and the table on page 213), only the sorghums and millets domesticated in the eastern Sudan or in Ethiopia spread into East, Equatorial, or Southern Africa. This virtually implies that agriculture had spread before the Bantu came. The date for the spread from Ethiopia to

Kenya (p. 208) is put at the end of the first millennium B.C., but this assumption really rests on a single carbon-14 data, which may well (thirty-three per cent probability) be wrong. To me it seems wrong because we find that by 1800 B.C. African millet had already reached India, from Ethiopia presumably. So one should not be surprised if new sites show that animal domestication and agriculture reached East Africa much earlier than 1000 B.C.—in fact not long after 2000 B.C. In this whole discussion one might as well make abstraction of all the physical anthropology that is correlated with cultural movement simply because we do not have enough skulls and bones to be certain that we are dealing with different populations, “newcomers,” “blends,” or simply normal variation within a single population. On the same general topic the date assigned (p. 210) to Lanet by its excavators (the middle of the sixteenth century A.D.) can also be a freak, and it looks like one. But one must remember that linguistics, physical anthropology, and the study of culture show that in the first millennium B.C., the first millennium A.D., and later very complex human movements have taken place in East Africa as well. That puzzle will not be unraveled in the near future.

The author shows convincingly (pp. 205–06) that agriculture was almost certainly practiced in the southern Congo at an early date, before the coming of the Bantu. This fact must be accepted, and every interpretation of the processes that led to the spread of Bantu-speaking people and the domination of their languages over all others in the African “peninsula” must take this fact into account along with the probability that Bantu-speaking people may have moved before iron had spread among them.

The description of the spread of the Bantu is merely a rephrasing of Malcolm Guthrie’s hypothesis. It is not more than a hypothesis. Many linguists, while admiring much of Guthrie’s monumental work, disagree with his historical implications.

By and large one agrees with Clark’s view about the spread of iron in Africa. Perhaps, however, iron reached East Africa first on board the ships of south Arabian traders; and it reached Daima (p. 215), not from Meroë, but from the bend of the Niger after having crossed the Sahara. Graham Connah feels that

this is the only likely explanation for the sites in northern Nigeria (see, for example, Thurstan Shaw, *Lectures on Nigerian Prehistory and Archaeology* [Ibadan, 1969], 33). Since the Iron Age civilizations belong to the historical record in many cases, the short summary given here seems very general indeed to most historians and needs no discussion.

When all is read, one can only admire the book and learn from it. It will influence and even shape any interpretation in African archaeology for years to come. Some dates may be wrong, some assertions may become obsolete, but the approach will endure. That is what makes a great book.

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ROBERT I. ROTBERG. *Joseph Thomson and the Exploration of Africa*. New York: Oxford University Press. 1971. Pp. 360. \$10.00.

ROBERT I. ROTBERG, editor. *Africa and Its Explorers: Motives, Methods, and Impact*. Cambridge, Mass.: Harvard University Press. 1970. Pp. 351. \$12.95.

One cannot often review two books with such unreserved pleasure. By virtue of Professor Rotberg’s sensitivity and his brisk but restrained style, *Joseph Thomson* is rescued from both the tedium and the hyperbole often encountered in tales of trial and tribulation in darkest Africa and is transformed into one of those infrequent biographies that can be read for sheer enjoyment, should one’s interests happen to go no further. Rotberg, like Thomson, knows how and when to be “cool in print.”

Thomson’s portrait, however, is not altogether endearing. His approach to Africa was essentially romantic, his observations were often naive. He hungered for fame, could be prudish and hypocritical, was unstable and restive—a professional wanderer—and was plagued by fits of melancholy. His obsession with Africa drove him to illness and an early death. But despite his faults he is remembered for the sturdy gentleness, boundless goodwill, and gregariousness that got him through many a scrape and made the peaceful progress of his expeditions almost unique in African exploration. One of the book’s themes is Thomson’s conversion to Darwinian beliefs and imperial-

ism. Arriving in East Africa as an idealistic youngster prepared to admire and respect Africa on its own terms, he left the continent eleven years later convinced by observation, bad experience, and the fashionability of racial theories (and, one wonders, by the demoralizing effects of sickness?) that Africans spoiled by European contact could be redeemed only by long apprenticeship under benevolent European rule.

Thomson's achievements were limited. Coming as he did at the end of the era of exploration, only his first and third journeys were scientifically productive. Professing disdain for commercialism, three of his trips were nonetheless sponsored for commercial motives, while one can only be called recreational. A man of erratic insight, he realistically foresaw that Africa's trade potential was limited but assumed that the Kenya highlands were wholly unsuitable for white settlement. Always in a rush to cover the ground, his journals and published works proved to be disappointingly impressionistic and inaccurate. The total impression is that Thomson was perhaps the most important of the second rank of explorers.

The book is fully equipped with the proper scholarly paraphernalia: index, notes, a good bibliography of Thomsonia, and appendix noteworthy for the texts of Thomson's African trade treaties. Two technical negatives can be registered. The voluminous footnotes, parenthetically fascinating as they are, are somewhat distracting. The most significant should have been absorbed into the text, the rest discarded. Many place names found in the text are frustratingly missing from the maps.

*Africa and Its Explorers* is focused on nine men who, with one or two omissions, contributed most to African exploration and made the deepest public impression. The contributors were charged with a twofold task: to assess their subjects' historical contributions and to place them in an African context by a careful examination of all available indigenous accounts. The editor undertook a third task in the introduction: a comparative study of the explorers' motives and methods and of their individual responses to Africa and Africans. The outstanding merit of this collaborative effort is the achievement of the second task, of an African perspective that serves as a most useful

corrective to the prevailing Eurocentrism of studies of exploration. The book is a series of neat vignettes that furnish the reader with an *Überblick* of preimperial Africa through the eyes of the men on the spot, African as well as European.

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RUTH FIRST. *Power in Africa*. New York: Pantheon Books. 1970. Pp. xiii, 513. \$10.00.

JON WORONOFF. *Organizing African Unity*. Metuchen, N. J.: Scarecrow Press. 1970. Pp. x, 11-703. \$15.00.

T. PETER OMARI. *Kwame Nkrumah: The Anatomy of an African Dictatorship*. With a foreword by NII AMAA OLLENNU. New York: Africana Publishing Corporation. 1970. Pp. xix, 229. \$8.50.

PETER GEISMAR. *Fanon*. New York: Dial Press. 1971. Pp. 214. \$6.95.

The first two works, *Power in Africa* by Ruth First and *Organizing African Unity* by Jon Woronoff, are studies of Africa as a whole; while the latter two are studies of specific figures—a pioneer of the African revolution, Nkrumah, and a major philosopher of both the African and Afro-American revolutions.

In *Power in Africa* Ruth First has made a real contribution to African studies through her searching analysis of the role of the military in nation building among the African states. A statement in the introduction reveals the nature of the work; the author says, "This book is about army intervention in politics, but more about politics than armies." It is not, accordingly, a book about the mechanics of armies and coups d'état, their logistics and command structures, but about the way army interventions in politics reveal the nature of political power and its areas of failure in Africa.

The opening section of the book deals with the African political and economic situation in the period that followed independence. A brief but well-done analysis of the various problems of Africa is included. The role of the army in the new African states and the "contagion of the coup" are especially effective. The second section of the book entitled "The Colonial Sediment" is required reading for all who would

understand or attempt to understand independence in the African continent. The third, fourth, and fifth sections study the "successor states," the breakdown of authority, and the role of the coup. The author selects three nations as the focus of her study of the coups of Africa—Ghana, Nigeria, and the Sudan. The last portion of the work, and a most potent one, seeks an analysis of the army and its future role in nation building in Africa. *Power in Africa* is more than a summary of the various African coups; it is an effective presentation of the role of the military and its interrelation with other elements in the political and general life of the new African nations.

*Organizing African Unity* is a rather voluminous study of the Organization for African Unity. While some of its materials are duplicated in such works as V. T. Thompson, *Africa and Unity*, it nevertheless makes a contribution to the study of the African search for unity. It is regrettable that the format of the work leaves much to be desired in the matter of explanatory materials. The casual student of African studies would, I fear, experience difficulties in following the presentation of the materials and the views of the author.

T. Peter Omari, in his *Kwame Nkrumah: The Anatomy of an African Dictatorship* (1970), has added a valuable link in the understanding of Nkrumah. The student of African history, especially Ghanaian, now has available a rather extensive group of books—Nkrumah's own writings; A. A. Araf, *The Ghana Coup* (1966); John Phillips, *Kwame Nkrumah and the Future of Africa* (1960); Bob Fitch and Mary Oppenheimer, *Ghana: End of an Illusion* (1966); and Henry L. Bretton, *The Rise and Fall of Kwame Nkrumah* (1966).

Mr. Omari's introductory chapter seeks to present an overview of the Nkrumah period. While he presents Nkrumah's guilt, he finds that others, principally the judiciary, share the blame. The succeeding chapters trace the Nkrumah period; especially noteworthy is the chapter "The Many Sides of Nkrumah." Included in an appendix is a presentation of "Nkrumahism-African Socialism: Ghana's Conception of Socialism." This work adds a balanced account to a controversial period of African history.

The last work, *Fanon* by Peter Geismar, is

the latest effort to capture the mercurial personality of Frantz Fanon. While this study of Geismar's gives a real insight of Fanon's psychiatric theories, it fails to capture clearly Fanon as a philosopher. In all fairness to Mr. Geismar, the implications and nuances of Frantz Fanon will never be easy to capture. A bibliography of Fanon's writings, both political and medical, is appended.

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VINCENT M. BATTLE and CHARLES H. LYONS, editors. *Essays in the History of African Education*. (Center for Education in Africa, Institute of International Studies, Teachers College, Columbia University.) New York: Teachers College Press, Teachers College, Columbia University. 1970. Pp. ix, 123. \$3.50.

MICHAEL ANTHONY SAMUELS. *Education in Angola, 1878-1914: A History of Culture Transfer and Administration*. (Teachers College Studies in Education.) New York: Teachers College Press, Teachers College, Columbia University. 1970. Pp. xiii, 185. \$7.95.

Five theses and seminar papers submitted to the Center for Education in Africa, Teachers College, Columbia University, comprise the volume edited by Battle and Lyons. The authors and essays are as follows: Charles H. Lyons, "The Educable African: British Thought and Action, 1835-1865"; Henry John Drewal, "Methodist Education in Liberia, 1833-1856"; Vincent M. Battle, "The American Mission and Educational Development in the Southern Sudan, 1900-1929"; Priscilla Blake-more, "Assimilation and Association in French Educational Policy and Practice: Senegal, 1903-1939"; and Richard Heyman, "The Initial Years of the Jeanes School in Kenya, 1924-1931." These essayists have produced commendable studies, and their coverage of the sources is adequate, despite an overreliance in the first two papers on the admittedly excellent book by Philip D. Curtin, *The Image of Africa* (1964).

Several themes stressed by the essayists are familiar to the historian of Africa. They are the close relation between the development of educational and administrative policy, the impact of the missionary influence on education in sub-Saharan Africa, the parallelism in the civi-



lizing and evangelizing objectives of most missionaries, and the American influence upon African education.

In contrast to the diversity of topics in the essays, Samuels concentrates in his volume on the study of Angolan education in the 1878-1914 period. Consideration is given to the numerous forces that resulted in contemporary educational organization. The educational story also depicts the highlights of an important era in colonial history. Educational development is surveyed in accounts of the growth of governmental administration and Christian missions. Rivalry between Protestant and Catholic missions; the relative value of "civilizing" Africans through labor, language, and other cultural influences (*educação*) as opposed to traditional schooling (*instrução*); and the slow progress of the Portuguese and Angolan local governments in building an educational system are other themes. In both books the similarity in the interests of government and the missions, and, in the long run, the subordination of the latter to the former are obvious.

The Samuels study is based on an impressive array of sources as reflected in an exhaustive bibliography, and illustrative maps and an index are helpful. In modesty and understatement, the author displays a seasoned scholarship and masterful treatment of his topic. Samuels and the essayists emphasize the need for research on African education and suggest many topics awaiting the attention of historians. These volumes are recommended to specialists and those with a general interest in Africa.

GARLAND G. PARKER  
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ALAN SCHAM. *Lyautey in Morocco: Protectorate Administration, 1912-1925*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xi, 272. \$8.75.

Attention! The student of Moroccan affairs is likely to read the jacket copy, which does this book a disservice, and perhaps the first fifty pages, then lay this book aside as yet another rationalization of French colonialism (called *mission civilisatrice*, or "white man's burden" in English). It would be a mistake. The introductory chapter relies on French sources, in-

cluding the writings of Lyautey himself, and repeats all the old clichés concerning roads, ports, dams, mines, and the necessity of treating the "natives" with understanding, respect, and generosity.

Even in the introductory pages, however, the author (almost unwillingly, it seems) exposes the paradox between Lyautey's protestations and his actions, and in the final chapter provides a critical assessment of Lyautey's failure to live up to his noble words. He points out that the French took over in Morocco to protect the Algerian border and French dominance in North Africa. The other ascribed reasons only supported that primary goal.

Lyautey, a royalist, was pledged by the Treaty of Fez to protect the authority of the sultan; he systematically destroyed it. He acknowledged the presence in Morocco of "cultured men who dealt as equals with European statesmen, who are skilled politicians and diplomats" (p. 29), but "owing to their lack of education in the modern European sense they were not regarded as capable of taking part in the French administrative mainstream" (p. 48). He never failed to insist that "one thing we have achieved: that is the knowledge of how to win the sympathy of the native. . . . Despite everything, these races have remained faithful to us," but he did not live to see the conquest (he called it pacification) of these natives despite the tremendous military effort. Whatever may have been the excesses of his successors, Lyautey created the institutions and set the policy that led to the overthrow of the protectorate.

There is little new in the story of the French failure in Morocco. But the author has provided in *Lyautey in Morocco* something that is new: a detailed description of the governing institutions of the Sherifian Empire before and after the protectorate was established. Here lies the great value of this book. The author also has provided nine appendixes of reference material, a glossary of Arabic terms (many with meanings peculiar to Morocco), and an extensive and valuable bibliography.

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E. A. AYANDELE. *Holy Johnson: Pioneer of African Nationalism, 1836-1917*. (Africana Modern Library, Number 13.) New York: Humanities Press. 1970. Pp. 417. \$11.50.

Reverend James Johnson, a Sierra Leonean who became an assistant bishop in the Anglican diocese of Southern Nigeria, was tormented by a cultural schizophrenia reminiscent of Sun Yat-sen's on the other side of the world during the same early twentieth-century period. On the one hand Johnson was a man of his own people, passionately espousing African "purity," working for African self-government, and rejecting the materialism, the arrogance, and the whiskey-soaked immorality of Europe. On the other, he embraced European religion, including temporal forms and organization, with equal passion; he favored European science and technology and urged agricultural improvements and education in basic engineering for Africans; and he bitterly denounced much that was characteristic of African societies. With some of the British in Africa, on whom he looked with an increasingly complicated ambivalence, Johnson imagined that it would be possible to design and carry out a rational plan whereby Africa kept what was wholesome in her own traditions and adopted from Europe only those ideas and techniques that would harmonize with native ones. Inevitably he experienced disillusion and frustration. His sincerity, energy, and remarkable perseverance, however, brought him great fame throughout West Africa and in England. His accomplishments in proselytizing and in making Africans more aware of their own worth were more than enough to justify the subtitle of this book, even if the nationalism that finally emerged was as different from James Johnson's ideal as the kingdom of Italy was from Mazzini's.

Professor Ayandele of Ibadan has not produced an intimate portrait because, as he points out, he did not have access to personal papers. But this account, based mainly on prodigious research in the files of the Church Missionary Society, the Aborigines Protection Society, and the Public Record Office, is an authoritative and valuable rendering of the official side of Bishop Johnson's work. The author deserves high praise for his scholarly objectivity in an age when politics have intruded

into academic life rather more than one would wish.

ROBERT HEUSSLER  
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RICHARD WEST. *Back to Africa: A History of Sierra Leone and Liberia*. New York: Holt, Rinehart and Winston. 1971. Pp. 357. \$6.95.

Richard West, an English journalist, has attempted a comparative study of the "Westernized" settler communities of Sierra Leone and Liberia. (The book's subtitle inaccurately suggests a broader purview.) Parts one and two, together comprising nearly one-half of the volume, are interesting and well-written accounts of the colonization movements in Britain and the United States and of the founding of the settlements at Freetown and Monrovia. West takes a largely biographical approach, with numerous quotations from the writings of Granville Sharp, Zachary Macaulay, Jehudi Ashmun, R. R. Gurley, and other participants. He has a flair for selecting apposite quotations, and he explores a number of topics passed over or given bare mention by other writers. Parts three and four survey the histories of the settlements during the last two-thirds of the nineteenth century and the early years of the twentieth, with a chapter on Marcus Garvey and his Back To Africa movement. West incorporates copious extracts from foreign observers, including Richard Burton (inevitably), Harry Johnston, and Mary Kingsley, from Edward Wilmot Blyden, and from James Africanus Horton, Benjamin Anderson, and a few other Creoles and Americo-Liberians. The selections on Sierra Leone include choice samples of European condescension and defamation balanced to some extent by critiques of the authors and some colorful passages on the drinking habits and other excesses of resident Europeans. West's presentation has serious limitations: quote—commentary—quote is no substitute for systematic historical reconstruction and analysis. Part five is a brief fifty-page account of developments in the past half century, concluding with some surprisingly shallow and trivial reminiscences from the author's two-month trip to Sierra Leone and Liberia. The reader has cause to wonder what perspectives

West, the journalist, gained from doing historical research.

In sum *Back to Africa* is a very uneven piece of scholarship. West fails to provide more than superficial descriptions and analyses of Creole and Americo-Liberian societies. Many topics are poorly researched, and there are numerous errors of fact or interpretation that could easily have been avoided. The method of footnoting is haphazard: the reader wants to know where all the quotations were found and to whom the author is indebted for opinions and interpretations. Nonetheless some sections are very good, with excellent use of quoted material coupled to stimulating commentary. The illustrations are well chosen to complement the narrative. If the specialist will be disappointed—or annoyed—by West's treatment of some topics, it is only fair to acknowledge that the general reader will find *Back to Africa* a highly readable and frequently entertaining introduction to Sierra Leone and Liberia.

GEORGE E. BROOKS  
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S. A. AKINTOYE. *Revolution and Power Politics in Yorubaland 1840-1893: Ibadan Expansion and the Rise of Ekitiparapo*. (Ibadan History Series.) [New York:] Humanities Press. 1971. Pp. xxi, 278. \$10.00.

This is an important study, adjusting as it does the perspective on nineteenth-century history of the key Nigerian group known collectively as the Yoruba. For the marginal effects of European colonialism in Africa have included not just the Euro-centered view of history, now finally vanquished, but the more subtle tendency in restudying and revising to deal with those Africans with whom Europeans were first preoccupied. Thus in Nigeria students of African history have looked either at the coast and its immediate hinterland, or at the Islamic empire of the Fulani and its predecessors in the north.

Akintoye takes us further inland than have previous studies in Yoruba history (the notable exception is Johnson's *History of the Yorubas*, written in the 1880s and as much a source as a historical account). Himself Ekiti (that is, from one of the Yoruba subgroups central to his study), Akintoye has that invaluable insider's ability to obtain and utilize oral evidence. Nor has he neglected written material, including

British sources. He also handles with detachment the role of Ibadan, that most formidable Yoruba power and opponent of the Ekiti and other subgroups of northwestern Yorubaland; he presents, indeed, the most valuable analysis of nineteenth-century Ibadan's impact available in print to date.

As the title and subtitle indicate this is a study in military and diplomatic history. It is mainly a book for specialists; the novice in African history may lose his way amid the many names of people and places and positions (one could wish for more and clearer maps, particularly for one that put Yorubaland in Nigeria).

Nonetheless, the book is of more general interest. It gives an excellent and concise introduction to the whole of eighteenth- and nineteenth-century Yorubaland. It treats interestingly the question of Yoruba unity or lack thereof, a question that continues to matter in twentieth-century Nigerian politics. Further, historians with interests elsewhere on the continent and beyond will find valuable material for comparative purposes. For, always with extensive evidence carefully examined, Akintoye tackles such problems as how urbanization took place; how a state whose expansion was military incorporated refugees and the conquered; and how related African groups, threatened by signs of European advance, did or (as in this case) did not perceive the threat—or did not regard it as of sufficient importance to set aside differences. Here, too, are examples of an African provincial, or even perhaps colonial, administrative system; of a confederal organization formed for military purposes; and of the continuing role of "domestic slavery" in politics, economics, and the army.

A synthesizing conclusion as illuminating as the introduction would have been welcome. Still, each of the author's analyses emerges with clarity throughout the book, making it a valuable and stimulating contribution.

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Purchase

HARRY A. GALEY. *The Road to Aba: A Study of British Administrative Policy in Eastern Nigeria*. New York: New York University Press. 1970. Pp. x, 184. \$7.50.

Professor Gailey is concerned with four linked

topics: the establishment of British rule in southeast Nigeria; the imposition of an apparatus of local government inspired by Lugard's Northern Nigerian system; its manifest unpopularity, culminating in the women's riots; and, tentatively and briefly, Sir Donald Cameron's reforms. The book is briskly written and clearly organized, no slight achievements given the multicentricity of groups involved.

Gailey confesses that conditions in Eastern Nigeria in the spring of 1967 led him to abandon the idea of interviewing the "old men and women in the villages" in order to analyze the riots. Consequently he has written an essentially administrative history, with the rioters seen mainly through administrators' eyes. Yet it is administrative history of a remarkably rich kind. Lugard, after dismissing the anthropologist employed by the Southern Nigerian government, succeeded in making his own views prevail, but there was no monolithic unity of opinion within the bureaucracy. Disagreement was sharp and articulate, allowing Gailey to reveal the issues at stake in the dialectic of policy making.

He shows how alien district officers, parvenu warrant chiefs, usurious clerks and court messengers usurped the traditional authority of the village elders. When the riots revealed the unpopularity of this regime to all but the most complacent administrators, Cameron set about establishing institutions of local government more in accord with the precolonial pattern.

Gailey gives tantalizing glimpses of the social history he was forced to abandon. The Dancing Women's Movement, the Spirit Movement—which attacked non-Christians, especially chiefs and their families—and the remarkable leadership and discipline of the Oloko women, all deserve extended examination while some of the "old men and women in the villages" still survive. When the social history of protest in southeast Nigeria is studied in depth and compared with similar movements elsewhere in Africa—and in other continents—the significance of the Aba riots, and the British bureaucratic mistakes precipitating them, will be better judged. Meanwhile, Gailey has written an extremely useful case study of British colonial officialdom in crisis.

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J. S. LA FONTAINE. *City Politics: A Study of Léopoldville, 1962-63*. (African Studies Series, Number 1.) New York: Cambridge University Press. 1970. Pp. xiii, 246. \$13.50.

Few cities have undergone such sudden and profound change as Leopoldville at the time of La Fontaine's study in 1962-63. In the three preceding years, the city had not only become the capital of an independent Congo but had doubled in population, since it was a rare island of security in a country torn by civil war. To understand the effects of these phenomena the author conducted her own surveys of two of the city's communes, which she then integrated with other available political and social studies creating a general synthesis.

The strength of La Fontaine's book resides in its examination of the network of personal contacts between ordinary Zaïrois and their leaders. Discussing the fundamental problem of housing, she sensitively analyzes the relationship between tenants and landlords. In the realm of politics, she specifies the qualities necessary for success in both national and local office, attributes she calls publicity and patronage.

La Fontaine's broader interpretation, however, is gravely contaminated by her reliance on the distinction, so dear to colonial social anthropologists, between traditional and urban Africans. According to this view migrants to the city arrived conditioned by the static organization and kinship network of their villages, and they only gradually learned to subordinate these "tribal" characteristics to more modern political behavior. The argument carries with it the implicit assumption that rural Africans remained virtually unaffected by colonial rule until they moved to a city.

This viewpoint seriously distorts the colonial reality. Africans migrated to the cities after their old way of life had been substantially modified by the demands of colonial governments. Thus, colonial urbanization was not so much "a means of repudiating the traditional" (p. 149) as the most important in a continuous series of changes that transformed African society as it had existed before the European conquest.

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DOUGLAS L. WHEELER and RENÉ PÉLISSIER. *Angola*. (Praeger Library of African Affairs.) New York: Praeger Publishers. 1971. Pp. ix, 296. \$11.00.

Though several works on various aspects of Angolan history have appeared in recent years, *Angola* is perhaps the first attempt at a general synthesis within a single volume. It is possible that such an attempt is premature. As Wheeler himself observes, "The pre-European history of Angola is one of the least known histories of tropical Africa." Monographic studies of the early colonial era (1575-1800) have thus far raised as many questions as they have answered and do not lend themselves to a brief and uncontroversial summing-up. Consequently specialists in central African or Portuguese colonial history may find *Angola* much less satisfying than will the nonspecialist public for which it appears to have been written.

The abortive 1961 rising against Portuguese rule is clearly the focal point of the book. Approximately a quarter of the text is devoted to the rising and its aftermath. This is Péliissier's contribution. It is a painstaking and dispassionate examination of the evolution of the major nationalist factions, their respective achievements and failures, and the Portuguese response to the kinds of threats and opportunities that a divided nationalist movement offered. Péliissier's assessment of the contemporary situation is that the nationalist guerrilla campaigns have, ironically, provoked "an unprecedented stimulus" to Angola's economic development since 1960. He believes that the future of Angola will depend more upon the generation of Africans now enrolled in the greatly expanded secondary school system than upon the nationalist activists presently engaged in the liberation movement.

Wheeler's main contribution to the volume takes the form of an extended prelude to the section written by Péliissier. This format has the advantage of providing thematic continuity, though it runs the risk of seeing the Angolan past largely in terms of a sort of evolutionary movement toward the 1961 rising. Thus he repeatedly alludes to massive Bakongo popular resistance to Portuguese influence ("the African jacquerie") beginning as early as the sixteenth century. As presented, Wheeler's case for widespread proto-nationalist "jacqueries" rests more

upon assertion than upon evidence. On the other hand his treatment of the emergence of *assimilado* protest during the nineteenth century is well documented and is appropriate to an interpretation that focuses on the growth of African political consciousness and on the rising of 1961.

As a general survey of those aspects of the Angolan past that help illuminate the crisis of 1961 and as an analysis of events between 1961 and 1971, *Angola* is genuinely useful. And it is in this period—the almost contemporary past—that the bulk of its prospective readers are most likely to be interested.

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ROBERT HEUSSLER. *British Tanganyika: An Essay and Documents on District Administration*. Durham: Duke University Press. 1971. Pp. xii, 154. \$6.75.

The colonial district officer deserves the attention of historians, be they students of modern Africa or of the imperial process. Local administrators enjoyed their "heroic" era, in general, before the First World War: that is, before the crystalization of central bureaucratic structures, a time when initiatives were idiosyncratic and responsive to local political currents. Robert Heussler is not mistaken, therefore, in choosing to focus upon district officers; but he has failed to understand the contexts, regional and temporal, that defined the scope of the individual's activity.

By turning a blind eye to the German period, the formative colonial years in Tanganyika and surely the heroic time of district administration, Heussler makes an inauspicious start. He then takes up the various aspects of an official's task, with an extraordinary disregard for periodization and chronological sequence that will frustrate those who look for clues to the development of administration in the country. The essay illustrates a methodological trap well known to historians and others dealing with oral tradition, for it is only an enlargement of the "actors' model" of events. District officers often felt isolated at their posts and out of sympathy with dictates from the capital, preferring to see themselves as local barons. To be persuaded that these men did in

fact have great influence, we must be told far more about local politics and the historical circumstances of the respective districts.

The occasional merits of this short essay and the accompanying illustrative documents hardly compensate for the stereotypes of African society and politics it retails. A thoroughly researched study of the district officer will, among other things, draw on African evaluations.

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GERALD L. CAPLAN. *The Elites of Barotseland, 1878-1969: A Political History of Zambia's Western Province*. Berkeley and Los Angeles: University of California Press, 1970. Pp. xi, 270. \$8.50.

This is the history of the Lozi, the dominant grouping of Barotseland, centered on the flood plain of the upper Zambezi in present-day Zambia. Its ruling class traditionally held ties with the white establishment in Southern Africa; because of this and by virtue of their strategic location, the Lozi have constituted a political nexus of far greater importance than numbers alone would have warranted.

One should not take this book as yet another parochial study by an expatriate. This is a significant achievement, of merit equal to Ajaye's *Christian Missions in Nigeria* (1965), or Ogot's *History of the Southern Luo* (1967), in both methodology and synthesis. Although it breaks some new ground, its real benefit remains as an analytical and interpretive history, balancing both previously explored and new sources, affording insight into any number of historical issues claiming universality far beyond Barotseland's borders. Here is played out the vain attempt of a great African kingdom to prevail by accommodating rather than resisting European power, battling (but never stemming) the relentless erosion of its powers by the "indirect rule" that ensued. We see the pivotal role of "Western" (mission) education in the formation of new elite groups, whose destiny the missions—and governments—ultimately failed to control. We are shown how structural underdevelopment in a colony, customarily interpreted as imperial absent-mindedness, is often a conscious policy to further imperial aims and how

"tribal clashes" are class conflicts by another name. Finally, we see the sorry consequences of a traditional ruling class allying with European interests to suppress the rise of the new elite and in the process secessionist movements being born.

The book essentially depicts the interaction between European power (mission, chartered company, and colonial administration) and two African elite groups: initially the Lozi establishment, and subsequently the mission-educated elite. The latter, which was to become the backbone of the United National Independence party, ultimately tested the Lozi traditionalists, and after the Europeans departed, prevailed. Professor Caplan masters a variety of sources, oral and written, to chronicle these highly complex themes with a sure touch. He details King Lewanika's symbiotic relationship with the missionary Coillard, leading to the signing of the fateful (and fraudulent) Lochner Concession. The king's subsequent realization of what he had done and his efforts to turn aside the consequences contain all the elements of a Greek tragedy. Lewanika's aims of fashioning an educated elite to modernize and protect his lands were dashed as the British South Africa Company reneged on its commitments, arrogated his powers unto itself, and forced him into a final, groveling capitulation. In the process his "modernizing" elite developed such a stake in preserving itself and the monarchy that it became entrenched as the royalist party, utterly reactionary and manipulated by the Europeans, until its downfall on their departure.

Caplan might well have detailed more thoroughly the educational process by the missionaries, which produced the new middle class, and he might have exposed the questionable relationship between missionary and company. Some of the main figures, again particularly the missionaries, remain shadowy. But no matter: this is an important book, carefully constructed, painstakingly researched, and packed with detail. For any graduate student embarking on African field research, Caplan's discursive passage on the nature of his evidence ought to be required reading.

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LEONARD THOMPSON, editor. *African Societies in Southern Africa: Historical Studies*. (Published under the auspices of the African Studies Center, University of California, Los Angeles.) New York: Praeger Publishers. 1969. Pp. vii, 336. \$8.75.

Since World War II African history has become a "growth discipline," and its methods and perspectives are now being applied in Southern African studies. Leonard Thompson, who wrote a major work well within the traditional mode of South African history, has been eager as teacher and scholar to adopt the new approaches. In 1968 he organized a conference at the University of Zambia; this volume contains thirteen papers from that conference, six of them by former students. The papers are: Leonard Thompson, "The forgotten factor in southern African history"; D. W. Phillipson, "Early iron-using peoples of southern Africa"; Brian Fagan, "The later Iron Age . . ."; Monica Wilson, "Changes in social structure . . . : the relevance of kinship studies to the historian"; Martin Legassick, "The Sotho-Tswana Peoples before 1800"; Shula Marks, "The traditions of the Natal 'Nguni': . . . the work of A. T. Bryant"; Gerrit Harinck, "Interaction between Xhosa and Khoi . . . 1620-1750"; Alan Smith, "The trade of Delagoa Bay as a factor in Nguni politics"; William Lye, ". . . Sotho peoples after the Difaqane"; John Omer-Cooper, "Aspects of political change in the Mfecane"; David Hammond-Tookes, ". . . a model of Cape Nguni political process"; Richard Brown, "The External relations of the Ndebele kingdom . . ."; Anthony Atmore, "The passing of Sotho independence 1865-70"; and Colin Webb, "Great Britain and the Zulu people 1879-87." Two of the authors are archaeologists, two anthropologists, and ten historians. Although the papers vary in readability, the standard is uniformly high.

Volumes of papers whose only common interest is in the past of a particular region inevitably invite the criticism that the papers could have been dispersed as articles in journals. The point is well taken, but it can be partially met by arguing that this is a book for Southern African specialists. More seriously, these papers, on the whole, lack succinct endings to make clear the authors' precise conclusions and their significance for a historian, though the reader

is given real help by the editor's valiant introduction. Second, in describing migrations that lack a unifying thread of some sort and that involve a large number of names in addition to those of places, clear exposition in relation to adequate maps is essential. The maps included are excellent, but they are placed at the end of each paper, and the narrative and analysis are not explicitly connected with them.

These papers raise general issues; there is space to consider only two of them. Political pressures, to which all Southern African scholars are subject, give to the phrase "from an African point of view" a treacherous ambiguity. The detachment of the authors of these papers is impressive though it is due in part to the fact that the subjects avoid political issues. However, Atmore's conclusion, in which the end of Sotho independence is seen in terms of "white control," "interwhite conflicts," and "white interference" (pp. 300-01), overlooks the fact that it was hardly unimportant—either historically or to the Sotho—whether the Orange Free State, Natal, Cape Colony, or the High Commissioner were responsible for the governance of Lesotho. It would be a pity if the older South African history, which ignored variety among Africans, were to be succeeded by one that lumps all whites together.

Second, there is the question of the role of narrative and its relation to interpretation. Historians are not concerned only with event, but also with process and event. But what is the value to the historian of the study of the migration and subsequent distribution of peoples or the impact of languages on each other? What story is the Southern African historian trying to tell? Are these studies of languages, migrations, and consequent distributions to become the very stuff of history, or are they aids to interpretation?

The balance between narrative and interpretation can be seen in two of the papers dealing with the Mfecane. Smith's paper on trade is ingenious and careful, but it involves large assumptions; for example, ". . . it is improbable that this caravan could have originated in any place other than Zululand" (p. 187). Furthermore, so many statements have to be qualified that the paper becomes suggestive rather than conclusive. Unfortunately, the paper ends abruptly, and the material is not exploited.

Omer-Cooper, working on the basis of his earlier pioneer essay, *The Zulu Aftermath* (1969), is frankly interpretive, but rather than qualifying his statements, he appears to adopt a major thesis on continuity in the Zulu kingdom on the basis of an assertion that certain "innovations" were not "radical" (pp. 216, 227). Although sorting out events and interpreting them must go on together, do we yet know enough about the Mfecane itself—about the sheer flux of events—to justify interpretations for which it may be difficult to find evidence?

Professor Thompson and his colleagues raise an important set of issues. Many Southern African historians have turned away from the traditional questions because the approach was too political and too preoccupied with whites. But what central theme is to replace the old one of the development of the state and the impact of its policies? In other words, are the new historians concentrating sufficiently on problems of narrative, dealing with events in which Africans were the sole participants, or others in which only the "white side" has been subjected to detailed examination?

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R. RAVEN-HART, translated with notes by. *Cape Good Hope, 1652-1702: The First Fifty Years of Dutch Colonisation as Seen by Callers*. In two volumes. Cape Town: A. A. Balkema. 1971. Pp. xv, 222, xi, 223-527. R. 30.00 the set.

This is a paste-and-scissors work, but excellent of its kind. The subject is a pretty well-worn one, as books by visitors to the Cape in the colonial period form a favorite quarry for South African historians. For example, the travel literature relating to the Cape has been extensively used by the editors of the Van Riebeeck Society's publications and in works like Edward Strangman's *Early French Callers at the Cape* (1936). However, Raven-Hart has exhumed some unpublished accounts from the archives at Cape Town, The Hague, and London, which he has dovetailed with the better known published ones.

The arrangement is strictly chronological, thus involving the separation into two parts of accounts that describe the Cape on the writer's homeward and outward voyages. Each account is accompanied by a briefing (if I may use the

term) on the historical background relating to the individual's visit. A very full index, combined with a bibliography and glossary, facilitates consultation of the work, once the reader understands its rather complicated arrangement. The accounts themselves vary greatly in extent and in interest. Some of them, like those on pages 412-16 and 447-51, are such small beer as to be hardly worth printing. The hitherto unpublished accounts do not add anything material to what we already know; but the editor-translator judiciously evaluates them all and indicates where they confirm or contradict each other. The great majority of the callers of all nationalities were far more interested in the Hottentots than in the white inhabitants of the Cape. This interest is reflected in the index, which has thirteen and a half columns of references to the Hottentots and their activities.

Judging from the preface this work is perhaps chiefly intended for persons living in South Africa who have only limited knowledge of seventeenth-century history. The two volumes are richly illustrated; but unfortunately the reproductions have been made from photographs of the original engravings and illustrations, thus making many of them rather smudgy and inferior. Raven-Hart's enthusiasm for his self-imposed task is infectious; but although this work may well make fascinating reading for *aficionados* of early South African history, it is dispensable for anyone else.

C. R. BOXER  
Yale University

RAYMOND K. KENT. *Early Kingdoms in Madagascar, 1500-1700*. New York: Holt, Rinehart and Winston. 1970. Pp. xvi, 336.

In this seminal study Kent accomplishes a major reinterpretation of Malagasy history. Previous interpretation, which generally rejected the African contribution to the state-building process in Madagascar occurring after 1500, had been conditioned by the massive scholarship of Alfred Grandidier and his son Guillaume. Nevertheless, there were serious flaws in the work of the Grandidiers and their successors, which Kent demonstrates by a careful analysis of the written sources they used, and by an equally searching analysis of an im-



pressively wide range of oral, linguistic, and other nonarchival evidence.

Kent's conclusion is that early in the first Christian millennium migrations from the Indonesian archipelago brought many varying groups of these islanders to the western Indian Ocean world where they spread widely into east central and southeast central Africa, this migration taking place before the arrival of the Bantu-speaking peoples into these regions. As the Bantu migrations continued, an Afro-Malagasy "race" resulted, of varying cultural composition due to the process of contact in each locality. This pressure also led to Indonesian settlement in Madagascar, where previously only a limited number had gone, thus establishing the lasting Indonesian language base for the peoples of the island. The process was largely completed by the tenth century. Then early in our second millennium, when Indonesians on the African continent were confined to regions on the eastern coast and in the south, new pressures from Bantu-speakers and from the east-coast Muslim trading communities caused the last important movement of Indonesians to Madagascar. The migrants had by this time absorbed much from the Bantu cultural milieu, and some spoke only Bantu languages. Their movement ended by the end of the fourteenth century, and they set the stage for future developments on Africa's greatest island.

In this painstaking study, with its extensive references demonstrating the widest use of all forms of evidence relating to Madagascar's past, Kent has made a lasting contribution to the island's history. His conclusion is now established: "In short, the traditional past of Madagascar makes absolutely no sense without Africa" (p. 265).

N. R. BENNETT

Boston University

#### ASIA AND THE EAST

PAMELA NIGHTINGALE. *Trade and Empire in Western India, 1784-1806*. (Cambridge South Asian Studies, Number 9.) New York: Cambridge University Press. 1970. Pp. xv, 264. \$11.50.

OLE FELDBÆK. *India Trade under the Danish Flag, 1772-1808: European Enterprise and Anglo-Indian Remittance and Trade*. (Scandi-

navian Institute of Asian Studies Monograph Series, Number 2.) [Lund:] Studentlitteratur. 1969. Pp. 239. 45.75 S. kr.

The years from 1757 to 1818 can be characterized as the period of consolidation of English power in India. Throughout this period British policies were molded by the internal situation in India, the actions of other European powers, and the commercial interests of both the East India Company and the servants of the company who traded privately. The role played by British commercial interests in the extension of British control in western India and in the activities of the Danes is examined by Pamela Nightingale and Ole Feldbæk respectively.

Dr. Nightingale's thesis is that British imperialism in western India was not imperialism primarily for political gain and that the economic considerations of both the company and the private traders led to the consolidation of British authority over Malabar and Gujarat. With clarity and style Dr. Nightingale discusses her findings and in so doing not only has accomplished the affirmation of her thesis but has differentiated two types of economic imperialism: imperialism for the creation of a commercial monopoly and imperialism for the creation of stable conditions that allow for the pursuance of commercial goals. The transition between these two commercial philosophies by the English East India Company in western India has been accurately depicted in the examination of British policy toward Malabar and Gujarat.

Mr. Feldbæk in his examination of "the state of things behind the Danish facade" (p. 7) has shown the effect of British policies on the fortunes of the Danish Asiatic Company and private Danish traders. His findings regarding the official commercial activities of the Asiatic Company are not new and in essence reaffirm the notion that as British power increased, the commercial prospects of the Asiatic Company declined. What is unique, however, is the detailed account of the private Anglo-Indian commercial activity carried on under the Danish flag. This is a story that has not been told before with such thoroughness. From this account an appreciation can be had of the magnitude of these Anglo-Indian remittances as

well as the important role that the Danes played in the continuance of clandestine English private trade.

The advantage that Dr. Nightingale had, which Mr. Feldbæk did not, was the availability of private correspondence and commercial documents through which she was able to reconstruct the function of both the collective interests of the private commercial community and individual relationships in the formulation of British policy in western India. Mr. Feldbæk, on the other hand, had to glean his information regarding private trade chiefly from English, French, and Dutch sources, as few of the private Danish papers are extant. Whatever Mr. Feldbæk's account lacks in intimacy is compensated for by the volume of data cited and the number of examples given.

The only noticeable deficiency in Dr. Nightingale's work is the absence of a glossary that would have removed any possible vagueness regarding the meanings of such terms as *adawlat* and *chauth*, which are used but not defined in the text. The principal weakness in Mr. Feldbæk's study is likewise not one of substance but one of form. Within the body of the text he has included so much data that at times the flow of the discussion is disturbed. Some of this data should have been added to the excellent appendixes, which include valuable information on currency equivalents, values of cargoes, and the profitableness of Indian cargoes.

Within their defined areas of inquiry, these two works should be considered essential secondary sources for those interested in understanding the motives, means, and consequences of the consolidation of British power in India.

ANN BOS RADWAN

University of Pennsylvania

H. I. LONDON. *Non-White Immigration and the "White Australia" Policy*. New York: New York University Press. 1970. Pp. xv, 318. \$6.95.

EGON F. KUNZ. *Blood and Gold: Hungarians in Australia*. Melbourne: F. W. Cheshire. 1969. Pp. xviii, 301. \$7.50.

ALBERT A. HAYDEN. *New South Wales Immigration Policy, 1856-1900*. (Transactions of the American Philosophical Society, New Series, Volume 61, Part 5.) Philadelphia: the Society. 1971. Pp. 60. \$2.25.

Although these books are concerned with Aus-

tralian immigration, their particular topics necessitate individual appraisal. Dr. London's book describes how a population-hungry Australia rationalizes the exclusion of most non-white people. He reports that in 1968 there were 41,000 non-Europeans in Australia, of which group only 20,000 were Australian citizens. He feels that Australia may have to terminate its policy of "Australianizing" all immigrants and to accept some newcomers who may not be socially assimilable. Most Australians, he admits, prefer their homogeneous society—even long-resident Australian-Chinese seem to prefer "White Australia" to a nation open to major influx from Asia.

The author notes that the policy has been moderated to a degree during the sixties. Reform groups have developed. Politicians have grown sensitive to the criticism of excolonial nations in the UN. The increased number of Asian students at Australian universities has triggered student activism against the White-Australia policy. Polls and the press show some change in public attitudes—wide sympathy, for example, toward the Asian victims of such *causes célèbres* as the Nancy Prasad case. There is a feeling that Australian population needs can no longer be provided wholly from Europe and that the present policy of exclusion may imperil national security by isolating Australia from her Asian neighbors.

The Australian rationale against admitting large numbers of non-Europeans emphasizes the point that unassimilated pockets of immigrants endanger social harmony, might lead to racial violence, and might even embroil Australia with Asian nations. There remains some belief that nonwhites are, in certain respects, inferior to white Europeans. There is a decided sentiment that Australians have the right to decide who shall dwell among them. The major political groups tend to reflect this view although the Roman Catholic Democratic Labor party, the Communist party, and some Christian churches condemn the exclusion policy. The Liberal-Country administration also carried out some modifications in 1966, which have increased immigration opportunities for nonwhites, though still favoring the degree-holding, Westernized elite of this category. The Australian Labor party's traditional opposition

to "cheap colored labor" continues but may be liberalized under the new leader, E. G. Whitlam. All parties have taken note of the racial problem in the United States and in Britain and of the racialism and exclusivist legislation by Asian and African states.

The author, who believes a more liberal policy and, ultimately, an end to racially defined exclusion must come, has presented a readable, interesting account. He seems, at times, a bit unfair in his criticism of Australians for occasional boorishness toward nonwhites or neglect of social contact with such immigrants. Australian standards are no worse than those of any other nation with an immigration problem. And relations between Australians and white "New Australians" suggest that Australia may not be ready for more complex large-scale immigration problems.

Dr. Egon F. Kunz presents a somewhat romantic view of Hungarian immigration to Australia, a very small element of its human influx. We see the ebb and flow of a basically reluctant immigrant group, pulled toward Australia by gold, liberty, and sanctuary or pushed by poverty, war, injustice, and rebellion. The waves began with humble, largely Jewish arrivals in the 1830s, to be followed by the larger Magyar groups of refugees from the 1848 revolution, by gold seekers, and by refugees from the impact of the two World Wars, from the advent of communism, and finally from the invasion of Hungary by Soviet Russia in 1956.

The Hungarians seem always to hope for a return to the Central European homeland and so remain quietly Magyar in life-style and rather neutral in politics, although very active, Dr. Kunz reports, in all other fields of endeavor, especially the arts, sciences, and university teaching. Hungarian Protestants have moved into the Anglican fold in Australia, but Catholics have adjusted only with difficulty to the peculiarities of the Irish-Australian Catholic community. The rise of "Little Budapests" should be balanced against the record of considerable intermarriage with Australians of British stock. The author's style is at times repetitious, and there are too many lists in the text that really belong among the footnotes. There are occasional puzzling statements such as a reference to an 1854 voyage of the ship

*Golden Age*, "the first ship ever to go from Australia via the Panama Canal to England" (p. 69).

In contrast to Dr. Kunz's book, Professor Hayden's short study is excessively compressed. It takes up the Australian immigration question for the period 1856-1900, following approximately on the heels of R. B. Madgwick's book covering 1788-1851 and treating only New South Wales. Detailed accounts of successive governments of rough-edged politicians are presented, showing where the continually improvised policies for assisted or nonassisted immigration were influenced by the pastoralists' fear of recruiting more free-selectors, and by labor's suspicion of immigrant competition, and by the fluctuations of economic conditions limited by the rather earth-bound decisions of local political warfare. The author believes that while economic forces played a large part in shaping immigration legislation, the politics of land and of the wider franchise created political priorities that sometimes overrode or smothered economic requirements.

CHARLES S. BLACKTON  
Colgate University

#### AMERICAS

CARL ORTWIN SAUER. *Sixteenth Century North America: The Land and the People as Seen by the Europeans*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xii, 319. \$10.95.

Sauer uses sixteenth-century accounts of the North American continent to reconstruct the conditions before Europeans came to stay, to indicate the incipient change induced by their contact, and to obtain "an overview of what this country was." This is not, however, a source book. The geographer dominates the page, narrating the context of experience in which the observations cited were made. For example, Hernando de Soto is properly introduced as a brutal predator, the sources for his expedition are briefly described, and its organization and its progress rehearsed. We follow the march accompanied by a guide who continually cites contemporary report with a running commentary approving, questioning, or correcting it. The method is to observe observation;

the style is dry, clipped, and packed with fact; the tone is neutral. Thus, Sauer has attempted here for North America what he achieved so brilliantly for the Caribbean in *The Early Spanish Main* (1966). Once again we find the grasp of physical geography and occasionally the insight into the mental process of discovery that marked the earlier book, though here the material, one feels, is less familiar to the author and the subject less confined in area and period. As one might reasonably expect, the sections on Spanish exploration of the south are the best—happily indeed for they make up the greater part of the book. European preconceptions and attitudes, which of course shaped the explorers' impressions, should perhaps receive more attention in a round and fair view of the historical event, but Sauer's mind is quite legitimately on the object rather than the subject and on the geographical rather than the anthropological aspects of the object. He shows that object—the land with its endowment of vegetable, animal, and human life—to have been in many ways more admirable than latter-day Europeans or Americans have imagined and as yet little affected by European intrusion. We may with some confidence hope that Sauer's close adherence to the narrative records and his cool realism will prove as salutary to students as they are refreshing to a historian.

K. R. ANDREWS  
University of Hull

C. HARVEY GARDINER. *William Hickling Prescott: A Biography*. Introduction by ALLAN NEVINS. Austin: University of Texas Press. 1969. Pp. xxi, 366. \$7.50.

JOHN HEMMING. *The Conquest of the Incas*. New York: Harcourt Brace Jovanovich. 1970. Pp. 641. \$12.50.

Many years will doubtless pass before another biography of Prescott appears, so thoroughly has Professor Gardiner researched the life and times of this Boston brahmin who produced classical works on the history of Spain and its conquests in America. Certainly it will be difficult to turn up much new meaningful material on his family, education at Harvard, marriage, friends, search for a suitable topic to write on, and the life-long vigil to conserve his eyesight.

As one reads the mass of minutiae presented to document the progress of Prescott from his birth in 1796 into a prominent family until his death in 1859, "attended by several doctors," one wonders whether any historian in the United States has provided for posterity as much information on himself and his historical methods as did Prescott—with the exception perhaps of H. H. Bancroft's *Literary Industries* (1890). We see the well-to-do clubman struggling with the competing claims of social life, family responsibilities, and persistent illnesses; his constant exhortations to himself to conserve his time and energy for serious studies; his preoccupation with the way his books were published—the paper used, illustrations, color of bindings, copyright problems, royalty arrangements, and dispatch of review copies to promote sales.

We are, in fact, told too much. Professor Gardiner might well have heeded Prescott's own challenge to himself: "Do not be afraid of being too brief." The reader wearies of the almost daily health reports and the chitchat about social trivia and his children, though we learn almost nothing about his wife.

The most significant omission is any detailed or analytical evaluation of Prescott the historian. From the scholar who has made himself the foremost authority on the subject, we might well expect a more substantial treatment. Why is Prescott's *Ferdinand and Isabella* (1838) "distinctly Prescott's masterwork" (p. 145)? Is it true that "scholarly myopia attends most judgments of Prescott's histories" (p. 143)? Why did the American Historical Association in 1949 select another wealthy and socially prominent Bostonian, Francis Parkman, along with Frederick Jackson Turner, when the Pan American Institute of Geography and History requested the names of representatives and significant American historians? Was this myopia, a mature judgment, or ignorance of the true merits of Prescott's *Conquest of Mexico* (1843) and *Conquest of Peru* (1847)? Despite the labor lavished during the last decade and more by Professor Gardiner on Prescott, he gives no answer to these fundamental questions.

*The Conquest of the Incas* demonstrates once more that the writing of history cannot be left exclusively to the professional historian. It

also reflects how much has been learned about Inca history and Spanish action in America since Prescott bravely tackled the same subject over a century ago. Mr. Hemming has profited from the many contributions of widely scattered specialists, but it is the combination of his own imagination, diligence, and attractive style that has enabled him to produce the first satisfying and sound general work on the conquest of Peru that has appeared in a generation. Even this gifted writer, however, has been unable to escape the fatal fascination of the tangled genealogical story of Inca descendants as they struggled to establish their claims with the Spanish bureaucracy. And though he has missed a few useful items by Marcel Bataillon and Alfonso García Gallo, the only serious omission appears to be the works of the Swedish ethnohistorian Åke Wedin. (See Magnus Mörner and Hans Andersson, "A Reappraisal of the Sources of Inca History: The Works of Åke Wedin," *The Americas*, 25 [1968]: 174-90.)

What makes this comprehensive treatment so valuable is the author's fresh look at the conquest, from which has resulted a balanced view of this complicated process, including Inca reactions and viewpoints on Spanish rule. The narrative flows along smoothly, and there is due attention to Spanish legal and religious preoccupations. Excellent notes, maps, and illustrations plus a good index add to the value of the volume.

Mr. Hemming closes his work with an account of the modern explorers, especially Hiram Bingham, who sought for the lost city of Vilcabamba. As every true historian of Peru wants to do, Mr. Hemming made his own explorations and had adventures too. He visited one of the Inca sites at Choquequirau after a perilous trip across the Apurímac canyon by means of strands of telegraph wires: "I tied myself to a curved piece of wood that slid along these wires, with my feet dangling above the swirling grey waters, and hauled across with my arms for 250 feet along the swaying wires" (p. 481), a fitting finale for a rich experience in research and writing.

LEWIS HANKE  
University of Massachusetts,  
Amherst

EUGENIA MEYER. *Conciencia histórica norteamericana sobre la Revolución de 1910*. (Series Historia, Number 22.). México, D. F.: Instituto Nacional de Antropología e Historia. 1970. Pp. 234. \$3.60.

North American observers of the Mexican Revolution of 1910 have over several decades tortuously wrenched themselves from an inherited negativism toward everything Mexican and launched themselves into a commendable but perhaps futile search for the *realidad mexicana*. So argues Eugenia Meyer, a Mexican historian, in her analysis of changing American attitudes toward the revolution in this important, though incomplete, historiographical essay.

Trapped by their Protestant Anglo-Saxon prejudices concerning racial mixture, the Roman Catholic Church, and the Spanish tradition, most American authors writing from 1910 to the 1930s considered the revolution a logical and to-be-expected extension of Mexico's chaotic past. The rebels, they contended, heedlessly dismantled the peaceful and progressive state established by Porfirio Díaz and justified later United States intervention by proving their inability to govern themselves.

Events surrounding World War I, however, caused Americans to broaden their intellectual horizons, to tolerate the fact of the revolution, and to sift the results for an understanding of how Mexicans as individuals had been forged by their *circunstancia*.

Racism persisted as evidenced by Wallace Thompson (*The Mexican Mind: A Study of National Psychology* [1922]), but sensitive observers with social concerns, such as Carlton Beals (*Mexico, an Interpretation* [1923]) and Ernest H. Gruening (*Mexico and its Heritage* [1928]) discovered the Mexican to be a praiseworthy and creative person possessed of an appreciation for human existence that in most ways surpassed values being pursued in the so-called developed societies. This new-found appreciation for Mexican culture culminated in the work of Frank Tannenbaum (*The Mexican Agrarian Revolution* [1929]), who was undoubtedly inclined by his socialist tendencies but whose work remains among the most perceptive yet concerning Mexican character.

Professor Meyer principally treats the era from 1900 to 1940, although her epilogue ap-

proaches the present. In this final section she notes that a professionally trained corps of United States historians is producing a stream of monographs that approach the truth of the revolution—the truth being a more or less official Mexican viewpoint of its own revolution. In short, not only has the revolution itself become institutionalized, but also the manner in which United States historians view the event. Furthermore, the need to affirm United States values in light of the cold war, to contradict Soviet affirmations of a frustrated class struggle in Mexico, and, more importantly, to discredit the Cuban kind of social change has led American historians to label the Mexican model “the preferred revolution.”

The major shortcoming of the book is Professor Meyer's admitted failure to consider those United States domestic currents that have undoubtedly influenced much of the writing on Mexico. For instance, one better comprehends the interpretations of William E. Carson (*Mexico, the Wonderland of the South* [1909]) or Nevin O. Winter (*Mexico and Her People* . . . [1923]), when it is pointed out that the idea of white supremacy permeated the Progressive spirit. It also needs to be said that the Mexican Indianist movement of the 1920s coupled with the discontent of many American intellectuals toward their nation's emphasis on materialism influenced authors of the period to idealize Mexico's pastoral society. How their country's internal affairs affected the attitudes of United States writers toward Mexico is studied by Donald L. Zelman in his dissertation at Ohio State University, “American Intellectual Attitudes Toward Mexico, 1908–1940” (1969).

Despite the failure of Professor Meyer to develop fully her investigation, her book stands on its own as a pioneering effort to evaluate Mexican historiography from the Mexican point of view. Students of Mexican history can only agree that more of the same is needed.

PAUL J. VANDERWOOD  
San Diego State College

BERTA ULLOA. *La Revolución intervenida: Relaciones diplomáticas entre México y Estados Unidos (1910–1914)*. (Centro de Estudios Históricos, New Series, Number 12.) [México, D. F.:] Colegio de México. 1971. Pp. xi, 394.

Despite the cartoon on its cover depicting Tío Sam choking and kneeling *La Patria*, this is a work of careful scholarship with no discernible bias. In fact Miss Ulloa is less critical of Woodrow Wilson's Mexican policy than the North Americans Robert Quirk and Arthur Link. Her research is impressive, and she relies almost exclusively upon primary materials from Mexican and United States archives in presenting a straight, factual account of relations between the two countries between 1910 and 1914. She does not write as colorfully as Quirk nor with the impact of John Womack, but she has put together as complete a study of the events as may be found in any language.

There is probably more here of value to the Mexican scholar than to the North American. That is, most of the documents used may be found in United States archives (the National Archives, the Manuscripts Division of the Library of Congress, and manuscript collections of the University of Texas and Yale University), which, because of their availability, have already been researched to a considerable extent by U.S. scholars. Miss Ulloa does cite material from the archives of the Mexican Foreign Ministry, but the amount is slight and the ground seems familiar. Because much of the story is taken from U.S. documents, one may feel a “coals to Newcastle” effect. However, it is likely that Mexican scholars will derive a great deal more from a work of substance as this is in the original Spanish than from any comparable translated work.

Moreover, North American scholars will be disappointed if they look for a “Mexican viewpoint.” Miss Ulloa's judgment is balanced. One cannot detect even the “insidious” cultural influence that supposedly makes objectivity unattainable. On the other hand, the lack of an interpretative framework is a weakness of the book. Miss Ulloa permits herself to observe only that Woodrow Wilson wanted desperately to dominate Mexican political affairs and that he was ineffective. Further, she concludes that the military successes of the Constitutionalists, not the actions of Wilson or the Niagara Falls Conference (to which she nonetheless devotes one of her longest chapters), were responsible for the fall of Victoriano Huerta.

This work is part of the project sponsored by El Colegio de México under the direction of Daniel Cosío Villegas to write a contemporary history of Mexico. The project includes the collecting and editing of documents and the publication of individual and group research. Miss Ulloa's volume lends real distinction to the project.

CHARLES D. AMERINGER  
*Pennsylvania State University*

GEORGE WILLIAM PILCHER. *Samuel Davies: Apostle of Dissent in Colonial Virginia*. Knoxville: University of Tennessee Press. 1971. Pp. xi, 229. \$9.75.

Samuel Davies (1723-61) is best known as the "New Side preacher who consolidated the spontaneous revival in Hanover county Virginia being conducted by laymen whom Governor Gooch had christened 'Presbyterian'" and for his leadership of the successful legal battle for recognition of "the rights of dissenters under the English Toleration Act of 1689."

To these aspects of his career this biography adds little that is new. It does round out the picture of the man who during a career of less than fourteen years achieved a reputation as perhaps "the greatest preacher that America has known," whose oratory set an example followed by Patrick Henry, whose sermons (published posthumously in England in five volumes) went through more than twenty reprintings between 1766 and 1867, who was "America's first important writer of original hymns," who published poetry that was read throughout the colonies and was reprinted in 1968, who was recognized as one of the most eminent leaders of the Great Awakening, who pioneered in "the education of Negro slaves and . . . Indians," who was known as a foremost educator, and who for the last year and a half of his life was president of the College of New Jersey, for whose first permanent buildings he had spent fourteen months in Britain raising money.

Unfortunately Professor Pilcher's biography is a eulogistic extension of the funeral sermon given by Samuel Finley, Davies's successor at Princeton. The conclusion that his life "displayed a remarkable unity of inward belief and outward action" is contradicted by the evi-

dence. What emerges is the portrait of an oversensitive young man from a poor Baptist family turned Presbyterian, married into a prominent Williamsburg Anglican family, and struggled through a life beset by constant "feelings of inadequacy" and an inner turmoil that one may guess was related to his perennial ill health.

Structurally the portrait is a collage of material from and about Davies pieced together with Pilcher's uncritical observations. The resulting conglomerate calls attention to many apparent contradictions in Davies's career that are left unresolved and suggest that he erected a protective façade of pious deviousness. A few examples will make this clear.

Aside from having "seven [licensed] pulpits located in five different counties" Davies recorded that in two months he rode five hundred miles and preached forty sermons, often in the woods, while extending his work widely in Virginia and into North Carolina. Yet he staunchly denied that he was an itinerant. Pilcher agrees, saying that to Davies itineracy "meant going from place to place in a deliberate attempt to win converts," and he adds that this was something Davies "did not do," for he preached only "to congregations already converted." Yet it is also noted that he was a revivalist who always preached "to bring sinners to repentance." In this connection Pilcher seems uncritically to accept Davies's denial that he was a disruptive force in the colony, which a relative accused him of being, while also noting that he brought about "a mass defection from the established church."

Davies, says Pilcher, "was essentially a man of peace, preaching the words of a Prince of Peace" and admonishing his hearers to "cultivate a pacific temper towards one another, both as individuals and nations." Yet he preached preventive war in the name of defense, urged enlistment as a Christian duty, disobedience to which would bring God's curse, and "equated patriotism with Christianity and became, through his pulpit, the colony's best recruiting officer."

Davies's successful fight for the rights of dissenters, says Pilcher, helped "lay a firm base for the ultimate separation of church and state." Yet Davies, who favored Anglican bishops for

America, never questioned Establishment in principle and thereby threw his influence on the side of its perpetuation.

As for the education of slaves, Pilcher makes it clear that Davies's desire was to give them the ability to "read the basic texts of Christianity" in order that they might become church members, which he supposed, although he never told the slaves this, would "make them more loyal to their masters." Davies approved slavery in principle by holding slaves himself and, Pilcher weakly observes, "expressed whatever doubts he had about the institution by refraining from praising it." Actually his position suggests a Christian legitimization of slavery by separating being Christian from being free.

SIDNEY E. MEAD

*University of Iowa*

WILLIAM JAMES MORGAN, editor. *Naval Documents of the American Revolution*. Volume 5, *American Theatre: May 9, 1776-July 31, 1776*. With a foreword by RICHARD NIXON and an introduction by F. KENT LOOMIS. Washington: [Naval History Division, Department of the Navy.] 1970. Pp. xxviii, 1486. \$13.25.

I have been critical of the navy's documentary history of the Revolution in the past; if I continue to be so it is not out of sheer perversity. The project itself is of the utmost historical significance, yet it has never measured up to the high scholarly standards set by similar projects such as the Jefferson Papers, the Adams Papers, and the Franklin Papers. Its editorial practices have been faulted on many counts: the failure to state precise criteria for including or omitting documents, the tendency to reproduce documents from secondary sources when the primary sources themselves were available, the pronounced Whiggish bias in the written commentaries, and an index that has not always taken into account matters of current concern to scholars who are seeking different data as they resort to quantification techniques or to cross-disciplinary approaches in their research. Although the project has improved certain of these practices, it has yet to attain the level of competence in documentary editing that has been reached by other major editorial enterprises.

In the volume under consideration two major flaws are evident. First, there is the re-

production of numerous documents from Peter Force's *American Archives* (1837-53). Some of these sources, to be sure, have been destroyed or lost and can no longer be found in the original. But to rely so heavily upon the crude efforts of Force in these days of modern editing standards seems highly questionable. Second, there is an excessive reliance upon colonial newspapers for information. Much of this work was done by the project's former editor, the late William Bell Clark, who searched the press for shipping entries and prepared a voluminous file on the arrival and departure of vessels. Clark's file should have served as the focal point for further research into ships' logs. Colonial newspapers, after all, were no more accurate or detailed in their information than their modern-day counterparts.

Given the head start this project had with Clark's monumental collection of naval document transcripts, the financial backing of the federal government, and the availability of unemployed Ph.D.'s to serve as research assistants, scholars have every right to expect a higher standard of performance in this important historical undertaking.

GEORGE ATHAN BILLIAS  
*Clark University*

JOSEPH L. GRABILL. *Protestant Diplomacy and the Near East: Missionary Influence on American Policy, 1810-1927*. Minneapolis: University of Minnesota Press. 1971. Pp. x, 395. \$13.50.

In his 1968 presidential address John K. Fairbank challenged his colleagues in the American Historical Association to study "the missionary in foreign parts" whom he called "the invisible man of American history." Joseph L. Grabill has responded with this first comprehensive account of the influence of Protestant missionaries on the relations between the United States and the Ottoman Empire. The title is somewhat misleading. The bulk of the work deals with missionary efforts to guide the United States in remaking the Empire during and following the First World War and, more specifically, with the attempt to create an independent Armenia under American tutelage.

There is much to admire in this account. The research, at least up to 1919, is thorough and instructive, and some of Grabill's conclu-



sions are impressive. (He is probably right, for example, that the Armenian disaster could have been less had the missionaries' schools and hospitals been as open to the Turks as to their subjects.) Confessing himself in his preface to be an active Christian, he has sought to be impartial, and for a time the reader is persuaded that he has been. It appears, however, to at least one reader that the effort has cost him too much. He has leaned so far over to give the Turks their due that he has weighted his testimony against the Armenians, the missionaries, the Near East Relief, and Woodrow Wilson. The scarifying treatment of the American president in particular contrasts unpleasantly with the author's indulgence toward the bigoted Admiral Bristol, whose chief motivation was to make the new Turkey a happy hunting ground for American business.

Moreover, the very thoroughness of the research in its limited area may leave the impression that an independent Armenia guided by the United States was solely a conception of American Protestantism. The statement of the British ambassador (p. 230) that the Armenian mandate might still be put across by Morgenthau, Elkus, and Smith—two Jews and a Catholic—attests the inadequacy of such a view. Grabill has conscientiously set it down but seems not to have perceived its irony.

Worst is the account of the destruction of Smyrna in 1922, in which the statement of President MacLachlan of International College that the Turks not only committed no crimes but protected the college from destruction (by whom, one wonders) appears to have been credulously accepted. It is to be hoped that Professor Grabill will read Marjorie Housepian's excellently documented *The Smyrna Affair* and learn in harrowing detail that the Turkish army, far from maintaining order, set fire to the city and killed approximately 190,000 Greeks and Armenians by shooting, stabbing, fire, and deportation to the interior, while ships of the allied powers rode idly at anchor in the harbor; only the intervention of private individuals finally obliged them to save some of the victims. In this holocaust MacLachlan saw only that his school was spared. If Grabill had wanted to write a book against missionaries—as it sometimes seems he did—that would have been a good place to start.

The writing is generally serviceable, at times even making its points sharply, but some of it is awkward and some not even correct English. It does not appear to have profited from the kind of editing we ought to expect from a major university press.

In spite of these reservations the book should be read. Its author has ventured into unexplored territory and has done much of his research well. If he has fallen too often into error, I am persuaded he has done so from an excessive desire to be fair, of all intellectual maladies the least likely to spread.

JAMES B. GIDNEY

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ALLAN NEVINS. *The War for the Union*. Volume 3, *The Organized War, 1863-1864*; Volume 4, *The Organized War to Victory, 1864-1865*. (*Ordeal of the Union*, Volumes 7 and 8.) New York: Charles Scribner's Sons. 1971. Pp. 532; 448. \$15.00 each.

These two volumes bring to a satisfactory though premature close the most ambitious undertaking of one of the most remarkable men ever to enter the ranks of historical scholarship. Publication of the series collectively titled *Ordeal of the Union* began in 1947, when Allan Nevins was already fifty-seven years old. His general plan contemplated as many as ten volumes on the three decades of the Civil War era from 1847 to the end of Reconstruction. At his death in 1971, he had completed eight volumes, carrying the story down to 1865. To scarcely twenty years of American history he had devoted about one and one-half million words—the equivalent of perhaps fifteen ordinary books. Yet even without counting this magnum opus Nevins would still be regarded as one of the most prolific and gifted of American historians. Indeed, the list of his other publications since 1947 would probably match the lifetime production of many well-known scholars. When one also considers all of his earlier works, including two Pulitzer Prize biographies, it seems safe to say that no other American has ever produced so much historical writing of such high quality. It is a record that may never be equaled.

The first four volumes of *Ordeal*, though panoramic in their subject matter, were primarily about the coming of the Civil War.

They appeared most opportunely at a time when Civil War causation was the focus of extraordinary scholarly attention and controversy. During the years 1942 to 1950 the problem of explaining the disruption of the Union was wrestled with in significant books by Avery Craven, James G. Randall, Roy F. Nichols, David M. Potter, and Kenneth M. Stampp. Debate between the so-called revisionists and their critics over the irrepressibility of the conflict had reached its peak. The Nevins volumes were the climactic contribution to this outpouring of literature on the reason why, and they remain, in Potter's words, "the only great overall narrative based upon modern research." Aiming at historical synthesis on a grand scale, Nevins tried to assimilate conflicting scholarly views into an interpretation that seemed, as a result, to be ambiguous if not contradictory. More often than not, he appeared to place himself squarely in the nationalist tradition of James Ford Rhodes, but at times his tone became revisionist as he condemned the "failure of American leadership" in the 1850s and asserted that the war "should have been avoidable." This eclectic approach caused much confusion among reviewers who found him fair and unfair in his treatment of the South, sensitive and insensitive to the moral aspects of the slavery question.

At the conclusion of his fourth volume Nevins echoed Rhodes in declaring that the war was fought over slavery. But not slavery alone, he added. It was also a war over "the future position of the Negro race in North America." This addendum, though untenable as an explanation of why the war came, has considerable validity as an explanation of what the war came to mean, and it anticipated the trend of scholarship in the years that followed. After 1950 interest shifted from the somewhat jaded subject of war causation to the seemingly more relevant subjects of racism, slavery, the Negro and his white champions, and the problem of why the Emancipation Proclamation became largely a "deferred commitment." Only by exploiting the racial theme fully and giving it primary emphasis could Nevins have made his later volumes as timely and fashionable as the earlier ones, but he did not choose to do so, perhaps holding something back for special treatment in his final volumes on Reconstruction.

Not that slavery and the Negro are neglected in *The War for the Union*, but they are only marginally related to the principal argument with which Nevins sought to give this sweeping narrative a general conceptual structure.

The theme of the war volumes, Nevins announced in 1959, was to be "the impact of the war on national character." More particularly: "Their thesis, insofar as a single idea can be applied to a struggle so manysided, is that the war measurably transformed an inchoate nation, individualistic in temper and wedded to improvisation, into a shaped and disciplined nation, increasingly aware of the importance of plan and control." This assertion, variously phrased, appears again and again like a leitmotif throughout the rest of the work. Antebellum America was "amorphous," "limp," "formless," "protoplasmic," "spineless." The war produced a "coordinated," "directed," "organized" nation. Reiteration is bulwarked with elaborate description, most notably in a chapter of the third volume entitled "The Sweep of Organization." The transformation, it is plainly implied, was overdue and beneficent, ushering in modern America, whereas at the beginning of the whole series Nevins had declared that the war left "part of the country . . . half ruined for generations, and all of it set back by decades." Definition of the word "organized" and its equivalents is never very clear. In some passages we are led to believe that there was a wholesale shift from individualistic to collective effort during the war years; on other pages the significant change becomes the nationalizing of organizations admittedly already well developed at local and regional levels. Similarly, we are told both that war produced the transformation with revolutionary suddenness and that the war merely accentuated or stimulated changes set in motion before 1860. Furthermore, although he asserted that the transformation was measurable, Nevins made no real effort to measure it. He provided enough data perhaps to justify an initial hypothesis but developed no design of proof.

The fact is that Nevins had too much peripheral vision and flexibility of mind for the relentless pursuit of a grand thesis, and his style of writing was not suited to such enterprise. History he once defined as an "integrated narrative . . . written in a spirit of critical in-

quity for the whole truth." That the spirit was with him always can scarcely be doubted, but in his intensive search among the varieties, complexities, and ambiguities of history he found truth pluralized and seldom consistent. Even his more limited generalizations and judgments often have a two-way stretch. As for wholeness, in this series it became largely a matter of scope. One is impressed first of all by the vastness of his scale and the reach of his knowledge, but with reach there was also grasp and integrative understanding. Details are occasionally wrong and judgments are sometimes questionable, but reading Nevins gives one that sense of mastery which in public life is associated with statesmanship. The true measure of his achievement is what he made of the past, not what he abstracted from it.

Primarily narrative and descriptive, with interspersed passages of analysis, these two final volumes, like their predecessors, may be regarded as an updated, professionalized specimen of traditional literary history, and Nevins has perhaps carried that kind of scholarship about as far as it can go. To some readers his writing will also seem old-fashioned in another way. His outlook was that of a confirmed nationalist in the sense that he placed the nation at the center of his attention and associated the highest human values with nationhood. For him, as for Lincoln, the great conflict was above all a "war for the Union." He justified giving more emphasis to Northern than to Southern affairs on the grounds that the Confederacy was a temporary aberration. He had little sympathy for the Peace Democrats, labeling their 1864 platform "silly and evil." His pages vote overwhelmingly but not unanimously for Lincoln. In one long chapter, "The Tragic Lot of the Freedmen," he passed severe judgment on the Lincoln administration for its insensitivity and incompetence in dealing with this problem. Yet, on the political front he displayed a persistent hostility to Radical Republicans like Charles Sumner who were most concerned about the Negro's welfare.

As Civil War history, however, the Nevins volumes are by no means traditional. Military events, treated adequately though somewhat discontinuously and with wretched maps, are but part of the larger subject, which is the wartime nation. No doubt Nevins made his most

original contributions in those chapters detailing the effort behind the fighting, including such things as military transportation, industrial conversion, procurement of medical supplies, and the outpouring of war songs. These sections make heavy reading, laden as they are with names and statistics. But then there are other chapters on politics and diplomacy in which lively narration and deft character sketches quicken the pace again. Beginning with Vicksburg, "the organized victory," and Gettysburg, "the fumbled victory," these volumes continue past Appomattox and the tragedy in Ford's Theater to include two chapters on demobilization. It is a suitable ending, but there was plainly more to come, and one wishes, sadly, that it could have been so.

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WILLIAM N. STILL JR. *Iron Afloat: The Story of the Confederate Armorclads*. [Nashville:] Vanderbilt University Press. 1971. Pp. x, 260. \$10.00.

In his *Confederate Shipbuilding* (1969) William N. Still, Jr. gave us a brief, informative analysis of the difficulties that hampered Southern construction of warships, both wooden and ironclad. In *Iron Afloat* he concentrates upon the armor-clad vessels, telling why and how they were built and equipped, describing their battles in lively narrative style, and showing their contribution to the Confederate war effort.

Early in the war the enterprising Confederate secretary of the navy, Stephen R. Mallory, decided to offset the numerical superiority of the Union's wooden fleet by building iron-armored warships. In pursuit of this policy "approximately fifty ironclads were laid down or contracted for within the Confederacy, and twenty-two of these were commissioned and placed in operation." More might have been completed had not the Confederate shipbuilding program encountered chronic difficulties, such as shortages of iron and skilled labor, and inadequate transportation. In discussing these troubles, Still draws repetitiously upon *Confederate Shipbuilding*, reprinting a number of passages almost verbatim.

The ironclads were used mainly for river and harbor defense, and for this purpose Secre-

tary Mallory and his chief naval constructor John L. Porter favored casemated rams with inclined sides designed to deflect projectiles. Modeled after "the prototype Confederate ironclad," the *Virginia*, these warships were crude in appearance, slow and clumsy, and generally underpowered with inferior engines. Still defends them against critics who have emphasized their "makeshift" qualities. As warships, he argues, they were "potentially formidable," and Union naval commanders feared them. However, his own clear exposition of their defects helps fasten the term "makeshift" in the reader's mind.

His conclusion as to how much the ironclads contributed to the Confederate war effort is judicious. He points out that first the *Virginia* and later the James River squadron guarded stretches of the James against Union naval forces, that the *Albemarle* played an important part in the Confederate victory at Plymouth, North Carolina, in the spring of 1864, and that ironclads strengthened the harbor defenses of Savannah, Charleston, Wilmington, and Mobile.

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DAVID M. JORDAN. *Roscoe Conkling of New York: Voice in the Senate*. Ithaca: Cornell University Press. 1971. Pp. xiii, 464. \$15.00.

We have long needed a first-rate biography of Roscoe Conkling, for his political career was important not only to New York but to the nation during the Lincoln, Johnson, Grant, Hayes, Garfield, and Arthur administrations. Frustrated by a lack of relevant manuscripts (Conkling destroyed many of his personal papers), historians have been inclined to take the judgments and generalities of the haughty boss's many enemies at face value, and the result has been less than satisfactory. Donald B. Chidsey published a biography in 1935, but it was none too scholarly, and David M. Jordan labels it "a popular account."

Jordan is a practicing attorney in Pennsylvania who first became interested in Conkling as an undergraduate at Princeton. For a decade he worked, on his days off and vacations, on a full-scale biography of the senator. The

product, Jordan's first book, is an often useful but badly flawed work that, unfortunately, by its sheer bulk, may deter scholars from undertaking the thorough study that remains to be done.

The book has strengths. Jordan writes well (though the text is padded to a bewildering degree with common knowledge), he sketches the lives of a good many secondary political figures whose careers have been almost forgotten in recent decades, and he has an extraordinarily sophisticated view of James Garfield. He read widely in secondary sources, traveled to numerous archives, made admirable use of the Utica Public Library, and called attention to the importance of the Alonzo B. Cornell papers. But the volume's errors and faulty interpretations are legion, and one may wonder why the experienced editors at Cornell University Press, if not Jordan, were unable to catch them. Limited space permits mention of but a few.

Jordan's failure to examine New York newspapers in depth caused him virtually to overlook the Conkling machine's tight control of New York City Republicanism from 1871 to 1882. He did not carefully investigate the Phelps-Dodge case or the New York Customhouse controversy, and he badly misconstrues the Conklingite struggle with the Hayes administration. The account of Conkling's flirtation with Tilden during the postelection struggles of 1876 results in confusion. Jordan overlooks the significance of the Conkling-Lamar affair of 1879, and his portrayal of the senator's relationship to Katharine Chase Sprague after 1879 is unconvincing.

Part of the author's difficulties stemmed, apparently, from a hasty reading of documents—such as the Chester Arthur papers in the Library of Congress and the New-York Historical Society. Surely his amateurism hampered his effectiveness. At one point P. R. Levin's *Seven by Chance* (1948) is employed as documentation; on occasion one is soured by such sentences as: "The basic quality of American life was materially affected by the depression of the seventies" (p. 195). Horace Greeley is compared with Harold Stassen, James G. Blaine with Richard Nixon. And Jordan seems to have learned very little from several books he cites, such as Ari Hoogenboom's important *Outlawing the Spoils* (1961).

As a whole, this is the better of the two Conkling biographies, but it is hoped that historians will consider the task unfinished.

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HOWARD MUMFORD JONES. *The Age of Energy: Varieties of American Experience, 1865-1915*. New York: Viking Press, 1971. Pp. xix, 545. \$12.50.

*The Age of Energy* attempts to locate a "center" or "leading principle" in the period extending "from the age of Andrew Johnson to the presidency of Woodrow Wilson," an American era that by the author's admission "never really attained either political or cultural unity." Professor Jones's search for the meaning of the "extraordinarily diverse" forces contained within a "tumultuous republic" leads him to the principle of energy that in an extended conceit he applies to the life styles of American entrepreneurs, the restless cosmopolitanism of a new elite, and an aggressive eclecticism in the arts as well as to the creation of material abundance and the robust, unreflective tone of late nineteenth-century politics.

There are both advantages and disadvantages to the concept of energy as an organizing idea, the advantages stemming from its suggestive power and the range it affords the author's insatiable curiosity and lively concern for the cultural fact, whether it be the decor of Gilded Age sporting palaces, the menus of fashionable restaurants catering to gargantuan appetites, or the sudden rash of "How to" books signifying a renewed concern with craftsmanship. Yet if the strengths of Professor Jones's fascinating survey lie in the province of the descriptive, so too do its limitations—in the inevitable imprecision of the concept of energy as it is stretched to cover the bewildering variety of American behavior in these years. Thus to take merely one example, while it may be instructive in a limited sense to consider Walt Whitman and John D. Rockefeller as similar embodiments of the principle of energy, holding like visions of "a happy, wasteless, and plentiful society," the suspicion remains that they were, after all, two very different sensibilities with widely divergent systems of values. Something like this re-

servation writ large persists and constantly qualifies our assent to the argument—our conviction that variety, multiplicity, and idiosyncrasy ultimately triumph despite the author's case for underlying unity.

These doubts are confirmed when Professor Jones turns to the pragmatic sanction as exemplified in Theodore Roosevelt, the "magnetic center" of the age, subordinating "other and lesser conflicts" within it. The author eschews both the moralistic interpretation of "an age of excess" and the bureaucratic preferences implied in a "search for order." At its deepest level, therefore, *The Age of Energy* seems a response to personality, to that "amazing gallery" of powerful and picturesque figures—Annie Oakley, Ned Buntline, Adah Isaacs Menken, Daniel Drew, Jim Fisk, John D. Rockefeller, Andrew Carnegie, and Theodore Roosevelt—in whom various forms of energy come to concentrate. What held American society together in these years and supplied its citizens with such great vitality, Professor Jones argues, was the widely shared belief that ultimate meaning is determined by practical consequences and that "you can affirm any two things as good if they 'work' until you find in practice that they cancel each other or that something else or some other pair of opposites 'work' better." The question for the historian, however, is when and, more particularly, how such a handy rationalization occurred to post-Civil War America and whether conceiving of individual choices and decisions as comparable or even contradictory expenditures of energy really illuminates an age.

These questions enmeshed in the central concept of the study are mirrored in the form of the book, which is a series of personal essays or lectures with which Professor Jones has delighted generations of students of American culture. His presence and voice are constant, and what we see most clearly is the process of a vigorous mind at work on unshaped material, asserting and explaining a method, qualifying but making generalizations forcefully, discovering, quoting at length, cataloging, and combining. The result is a kind of "action history" of an intellectual engagement with the scaffolding intentionally left standing. If the final effect is that of a set of "notes toward a definition of culture" rather than a satisfying synthesis, we

nevertheless sense that here is much the same sort of energy that the author so admires in the people of the Gilded Age.

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LAWRENCE D. RICE. *The Negro in Texas, 1874-1900*. Baton Rouge: Louisiana State University Press. 1971. Pp. ix, 309. \$10.00.

ARLEN L. FOWLER. *The Black Infantry in the West, 1869-1891*. Foreword by WILLIAM H. LECKIE. (Contributions in Afro-American and African Studies, Number 6. Negro Universities Press Publication.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xviii, 167. \$9.50.

Although both these books deal with Negroes in the West during brief periods of the post-emancipation generation, there is no topical overlapping. They both manifest to some extent the understandable desire of the doctoral candidate to limit his coverage as much as is permissible.

The Rice volume is basically a political history of Negroes in the black belt and major cities of Texas from "Redemption" to the disappearance of the Negro as a political factor, with background attention to his economic, social, and cultural situations—a geographical emphasis that excludes not only Negro soldiers and trail drivers but also the expulsion in 1886 of the entire Negro population of Comanche County. One can sympathize with Dr. Rice's wish to avoid "the labyrinth of the Reconstruction story," but it is difficult to understand the Negro's political situation after "Redemption" without more knowledge than is available of his position during Republican dominance. The Fowler volume, too, does not begin as early as one might expect, inasmuch as black infantry regiments such as the 62nd, the 116th, the 125th, the 38th, and the 41st were represented in Texas, Kansas, and New Mexico prior to 1869. (On the other hand, unless the author intended to do more than refer to a secondary work, it was ill advised to carry the story beyond 1891 to include the grisly and tragic Houston mutiny in 1917 by a battalion of the Twenty-fourth.)

*The Negro in Texas* is a depressing narrative. The political elimination of the Negro was achieved by measures ranging from eco-

nomic pressure and the poll tax to expulsion, whipping, and murder. Not until 1966 did a Negro again sit in the Texas legislature. "Jim Crow" coaches were introduced as early as 1866. Although "Texas made greater progress in reducing Negro illiteracy than any other southern state," other cultural and economic developments were not impressive. The discussion of the Negro press might, however, have included mention of Emmett J. Scott, editor of the *Texas Freeman*, 1894-97, and later Booker T. Washington's confidential secretary. Rice's book contains no illustrations and, inexplicably, no map.

Fowler's volume on the two black infantry regiments, the Twenty-fourth and Twenty-fifth, supplements W. H. Leckie's study of the Negro cavalry in the West (1967). Their role was unspectacular but indispensable. While the dashing Ninth and Tenth Cavalries rode to the relief of beleaguered white troops and struck unexpected blows at hostile camps, the black footsloggers built roads, guarded wagon trains, and garrisoned posts; when they did see action, however, they demonstrated their worth. Their enemies were not merely Indians and outlaws but also "racial prejudice and discrimination." Ironically, it was after the Twenty-fifth was transferred from Texas to the North that it lost two soldiers by lynching. On the other hand, it enjoyed heart-warmingly friendly relations with the people of Missoula, Montana, achieved largely although not entirely through its magnificent regimental band. Two generations later, old-time Missoulians still remember this black regiment with affection and respect.

As contributions to a historical field—the Negro in the West—in which until recently little has been done, both these volumes will prove highly useful. The Rice volume, in particular, also points up how much still remains to be done.

KENNETH WIGGINS PORTER  
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KENNETH S. LYNN. *William Dean Howells: An American Life*. New York: Harcourt Brace Jovanovich. 1971. Pp. 372. \$13.95.

A major reason for Professor Lynn's new biography of Howells is to refute the conventional view of Howells as a complacent author writ-

ing about the complacent society of late nineteenth-century America. Taking "Leslie Fiedler's scornful characterization of Howells. . . as 'resolutely cheerful, progressive, and sane'" as representative of this critical perspective, Lynn attempts to establish the contradictory view that "actually Howells was suffering . . . from a personal and artistic despair that was every inch as profound as the more celebrated glooms that gripped Henry Adams, Henry James, and Mark Twain. . . . As it did to his three most gifted literary contemporaries, the *fin de siècle* sometimes seemed to Howells like the end of the world."

But Professor Lynn goes beyond the suggestion that Howells suffered from the despair of his literary contemporaries. He links Howells with the existential despair of all modern men. "Howells," he writes, "was a man of modern sensibility, whose awareness of life was rooted in radical doubt and anxiety. All his life he was afflicted by a sense of aloneness, emptiness, and the precariousness of his personal being. . . . In the climactic psychological breakdown he suffered in the mid-1880's, he was overcome by the feeling that all his life he had been playing roles, and that as an influential man of letters in Boston he was fulfilling an outlander's ferocious ambitions at the cost of cutting himself off from his deepest emotional needs."

Professor Lynn is persuasive in his argument that Howells suffered from "radical doubt and anxiety." His book examines Howells's youth in great detail. He uses this biographical context to illuminate Howells's first great novel of social realism, *A Modern Instance*. Published in 1881, it revealed Howells's profound sense of alienation as the central American experience. Howells had become a compulsive worker by the time he was an adolescent. He was willing to sacrifice friendships and the family he loved in order to fulfill his ambitions. He made this ruthless search for success the controlling motivation of Bartley Hubbard, the young hero of the novel, who abandons his wife Marcia to become a free man without responsibility in the West. Professor Lynn feels that Howells uses the figure of Marcia to express his insights into the sexual inhibitions and repressions of his America and the peculiar father-daughter relationships that developed out of this sexual situ-

ation. For Howells the setting of the novel, the New England town Equity, is representative of the absence of social identity in America that dooms Bartley and Marcia to a life without meaning other than their own selfish instincts.

By the middle of the 1880s Howells was searching for some new sense of social meaning, and he was attracted to the utopianism of Tolstoy. But Lynn finds that while Howells was able to borrow the broad social realism of Tolstoy's *War and Peace* for his great novel of 1890, *A Hazard of New Fortunes*, he could not imitate Tolstoy's commitment to peasant life.

For the historian of ideas, the methodological problem of Professor Lynn's book is that while he succeeds in relating the 'ideas in Howells's writings to his biography, he does not relate Howells's biography to American or modern society. Professor Lynn has described Howells's as one of modern existential despair. But Lynn has not defined what he means by modern. Is there a peculiar relationship of the artist to society in the nineteenth century that leads most artists to a sense of existential despair? Does the middle-class family in the nineteenth century tend to create unhealthy personal relationships? Is Howells's sense of a national identity crisis in the 1880s representative of much of the American middle class at that time? Is the American national identity crisis part of a larger crisis of middle-class identity everywhere in modern civilization? The traditional form of the literary biography does not make it possible for the intellectual historian to address himself to questions like these.

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RICHARD W. SCHWARZ. *John Harvey Kellogg, M.D.* Nashville: Southern Publishing Association. 1970. Pp. 256. \$5.95.

This is a straightforward and thorough study of the public life of John Harvey Kellogg (1852-1943), a man of unbelievable talents and energies and a man of tremendous influence on American life. A power in the Seventh Day Adventist Church for about half a century, Kellogg was a well-known and skillful surgeon, a prodigious and popular writer on religious,

medical, and popular health topics in journals and books, an innovator and educator in the health fields, a successful public speaker well known in the Chautauqua circuit, a big business man, a philanthropist, and more.

The central theme of his life was what he called "biologic living." This was a tenet that came right out of the Seventh Day Adventist movement, and the movement became the fulcrum from which Kellogg launched his many enterprises. Biologic living entailed a vegetarian diet and abstinence from alcohol, tea, coffee, and cocoa. It included also fresh air, exercise, and cleanliness. The Battle Creek Sanitarium was his monument to these ideas. But most important in the pursuit of his many ideas and fads in the dietary field, Kellogg developed the process of flaking cereal and thus not only was the dry cereal industry of Battle Creek begun (the Kellogg involved in the tremendous expansion of the industry, however, was John's brother, William), but the breakfast habits of the American population were changed. John Kellogg, too, was the inventor of peanut butter and, it is also worth noting, of a mechanical exercise horse.

All of the details are to be found in Dr. Schwarz's book, which, unfortunately, does not provide the student with documentation. Instead he is referred to the dissertation at the University of Michigan upon which this book is based. The reader might find much more convenient the comprehensive list of sources given in Gerald Carson's *Cornflake Crusade* (1957).

More important, however, is the fact that one does not come away from this book with any sense of understanding the personality of its hero or of the intellectual forces that turned him from a fundamentalist into something of a pantheist and a believer in organic evolution. We are never quite sure how much of Kellogg was showmanship, egotism, and avarice for money or for power and how much was genuine Christianity and humanitarianism, although it must be admitted that such profiles would be difficult to draw. The Kellogg story is, moreover, too often isolated from the main-streams of American life. In short, like Dr. Kellogg's "Protose"—a meat-like vegetable protein food he developed—the book is substantial, in-

teresting, filling, but somehow not quite satisfying.

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RICHARD D. WALTER. *S. Weir Mitchell, M.D.—Neurologist: A Medical Biography*. Springfield, Ill.: Charles C. Thomas. 1970. Pp. viii, 232. \$9.75.

Dr. Walter, himself a neurologist on the staff of the UCLA Center for Health Sciences, has written a fourth biography of S. Weir Mitchell, who was certainly the most celebrated and perhaps the most important neurologist and psychiatrist practicing in late nineteenth-century America. The subtitle of this book, "A Medical Biography," helps to clarify its relationship with Anna Burr's *Weir Mitchell: His Life and Letters* (1929), Ernest Earnest's *S. Weir Mitchell: Novelist and Physician* (1950), and David Rein's *S. Weir Mitchell as a Psychiatric Novelist* (1952).

Dr. Walter's introductory statement, which amounts to a description of his biographical methods and purpose, clarifies even further: "the present 'biography' resembles in many respects a volume of collected works. The biographer has served more as an editor, and the frequent quotations and excerpts from the original make this apparent. It is hoped that this direct exposure to Weir Mitchell will prove more interesting and helpful to those training in the fields of neurology, psychiatry, and neurosurgery than the more traditional strategy of paraphrase and pontification." Presumably this "biography" takes its place in a series including *Sir William Osler: Aphorisms from His Bedside Teachings and Writings*, *The Unique Legacy of Doctor Hughlings Jackson*, *Early Days in the Mayo Clinic*, and *Garrison's History of Neurology*.

Weir Mitchell's reputation was established in 1864 with the publication of *Gunshot Wounds and Other Injuries of Nerves*, which grew out of his hospital service as a Civil War surgeon. His fame came with the "Rest Cure" in the mid-1870s, and his celebrity was consummated in 1897 with the publication of *Hugh Wynne*, which Thomas Bailey Aldrich put in tandem with *The Scarlet Letter* with his pro-



nouncement that they were the "two great American novels."

Mitchell was a strikingly accomplished and versatile man in whom many of the currents and tensions of Victorian America are vividly illustrated. Women's liberation polemicists could have a field day with him (but only by side-stepping the paradox created by the obvious fact that he was eminently successful with his women patients); his series of medical handbooks written for laymen—which are, among other things, commentaries on the social bases of psychic disorder in America—are worthy of monographic study themselves; his test of William Osler with the cherry pits raises intriguing questions about the interrelationship between etiquette and professional performance at a time during which the professionalization of the professions was occurring. That merely begins a list of rich possibilities in Mitchell's life awaiting exploitation by a biographer who wishes to make some splendid triangulations between a man, his milieu, and post-Civil War American society. Should such a biography be written, quite likely it will be as much a study of social ideas and values as an account of medical and literary achievements.

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Chapel Hill*

- THOMAS PARKE HUGHES. *Elmer Sperry: Inventor and Engineer*. Baltimore: Johns Hopkins Press. 1971. Pp. xvii, 348. \$15.00.

Professor Hughes's *Sperry* is not only an intensely interesting study of a highly competent and inventive American engineer, it is also a model of the publisher's art. Both Hughes and the Johns Hopkins Press are to be congratulated upon the excellent way in which both the textual and graphical materials have been presented. The use of a large format with wide margins that have allowed the presentation of notebook pages, patent drawings, letterheads, and other memorabilia illustrating the points being made in the text is a boon to the reader. One of the advantages of the person Sperry was and of the business in which he was engaged, which required tremendous documentation in order to fight patent interference suits, was that he left thousands of letters, sketches, notebooks, and the like. Thus it would be assumed

that it would be easy to document his career as inventor, scientist, entrepreneur, and maker of machines, processes, and systems. Even so, Hughes quite often has to use terms such as "about" and "around" in reference even to years because he has not been able to find exact dates.

Elmer Sperry is one of the cluster of Americans produced by the Industrial Revolution who made their names as individuals before specialization and corporate group research and development usurped the field. Sperry lacked a college education, though he attended some lectures at Cornell. But he had an intuitive genius for seeing where the weaknesses in others' ideas lay, for examining patents to see what they did not cover, and for the conception and engineering development to production of his own solutions.

Sperry had one great inherent advantage over his rivals in that he learned early the necessity for feedback, and whether he was controlling electric current for arc lights in the 1880s or auto pilots in the 1920s, many of the same principles were applied. More than this, Sperry was a practical entrepreneur. He developed those ideas for which he could get capital and sold them, taking royalties and consultancies when they were developed. In general his interest in a field lasted about five years, and he moved out as soon as big business moved in.

Students of American history, whether they are in economic, technological, or general history, should read this book for an intimate understanding of the position of the inventor-entrepreneur from 1880 to 1930. They will find that the reason Sperry teamed up with the navy in the development of gyro-stabilizing was not that he was or wanted to be a warmonger, but rather that since the merchant marine was not willing to capitalize research and development, he followed his natural instinct for going where money could be obtained to further the work in which he was interested.

If one can suggest any criticisms of *Sperry*, they lie in the failure to integrate the story more fully into the general trend of developments in the United States and elsewhere and that the aeronautical terminology is weak, perhaps from being too close to Sperry's notes.

ROBIN HIGHAM

*Kansas State University*

GLENDON SCHUBERT. *The Constitutional Polity*. (The Gaspar G. Bacon Lecture on the Constitution of the United States, 1968.) Boston: Boston University Press. 1970. Pp. xviii, 195. \$6.50.

This book by Glendon Schubert, a distinguished professor of political science long recognized as the dean of the behavioral school of judicial analysis, well illustrates the advantages and the weaknesses of behaviorism as a technique for studying the Supreme Court. Originally prepared as the 1968 Gaspar G. Bacon Lecture on the Constitution (sponsored by Boston University), the book represents Schubert's synthesis of behaviorist findings on the role of the Supreme Court in policy making since 1921. (Conventional analysis is reluctantly accepted by Schubert wherever the behaviorists have not yet trod.) Schubert devotes separate chapters, each covering the 1921 to 1969 time span, to policy "output," ideological "input," and the responses to Court policy. Inevitably there is considerable overlap and much of the material is familiar, but the book still makes absorbing reading.

Perhaps Schubert's most valuable analysis is on the 1930s. Combining simple calculations with sound historical perspective, he underscores the great divide of 1937. He finds the Court at the commencement of the struggle with the president "every bit as devious and dissimulative" (p. 28, referring to the *Hot Oil* case) as Roosevelt would be two years later. On the switch in 1937, Schubert quite properly refuses to be taken in (unlike other constitutional authorities) by Roberts's "self-serving memorandum" that attributed his change of vote on the minimum wage cases to a legal technicality. Schubert concludes that *all nine* of the pre-Black justices, including the "Wilsonian-Progressive" liberal bloc, were too conservative for the unfolding New Deal. Roosevelt's attack on longevity thus had real point to it.

Interesting observations abound in the book, usually based on vote counting in conjunction with astute analysis. Two examples are the chance effect of John H. Clarke's premature resignation in 1922, which deprived the Court of its "very first modern liberal," and the odd consequence of the Frankfurter-Harlan desertion of the liberal bloc in 1959, which left freshman Justice Stewart in the crucial middle position until Frankfurter's retirement in

1962. On the Warren era Schubert is highly favorable, especially to the great period from 1963 to 1969, when the Warren Court gave to many and diverse civil libertarian causes a "degree of favorable support" probably unequalled by any other court in the history of the world.

The basic limitation of Schubert's book (and even more so of behaviorist studies in general, which usually lack Schubert's sense of history) lies in its one-dimensional view of the justices and their work on the Court. By derivation the "input" side of Schubert's mix must be taken with considerable skepticism as a satisfactory explanation of change in the role of the Court.

Scorning on the one hand "legalistic doctrine and opinion verbiage" and similarly patronizing, if more respectful, to the nonquantified scholarship of Alpheus T. Mason, Walton Hamilton, and the like, Schubert stays relentlessly with the typical behavioral categories of liberal and conservative, subdivided into economic and political. Within these categories the justices stand emplaced with their "policy preferences" (little more than Pavlovian responses, it would appear from some behaviorists). That a justice may have a constitutional philosophy that will lead him at times contrary to his policy preferences or that a justice, because he accepts settled law, will indeed vote many times (usually on unanimous issues) contrary to his private wishes are seemingly inconsequential alternatives to Schubert and his school. The analysis makes no pretense of even considering the constitutional legitimacy of a justice's position. He is simply scaled on the chart and his "inconsistent" votes labeled deviations.

I would be the last to deny the influence on a justice's decisions of his result preferences (my own work has certainly emphasized judicial attitudes and values). The broad syndrome of judicial conservatism versus judicial liberalism is clear enough in any term of the Court. Nor has my current association with the lawyer's approach particularly endeared me to professional legalism. But it is impossible to explain Frankfurter without serious consideration of his intellectual convictions in regard to judicial self-restraint—constitutionally wrong, pedantically dogmatic, and ultimately oppressive as I believe they were. And can Harlan be

dismissed as simply a low-ranker on the liberal-conservative scale when we consider his skillful sensitivity and occasional decisiveness in such opinions as *Yates v. United States* (emasculating the conspiracy-to-advocate clause of the Smith Act) and *N.A.A.C.P. v. Alabama* (protecting the NAACP against state harassment and establishing freedom of association as a viable constitutional right)? One would never guess from Schubert's analysis that justices make constitutional law as well as public policy.

Schubert, of course, does not deny the varied institutional functions of the justices but asserts in the preface that his concern is simply with their input on policy. The result, however, is a grossly skewed view of the Court and the individual justices, the more deplorable since behaviorism is so much the fashion in political science departments.

It is in the consideration of Hugo Black's last years on the Court that Schubert's approach really runs aground. On the positive side, he makes effective use of case-vote compilations to drive home what we all sensed—that Black had sharply plummeted on the liberal-conservative scales. In the 1968 term Black became “the Warren Court's anchorman on civil liberties, dissenting forty-one times (twenty-five of these alone) against the sixty-eight split pro-civil liberties decisions in which he participated” (p. 118). To Schubert this precipitous drop bears the mark of “cultural obsolescence” and “psychophysiological senescence” (*sic*). Refusing to accept Black's strict constitutional literalism at face value, he accuses Black of shifting his “ideology . . . to remain in touch with his explicit conservative policy goals.” In the course of his indictment, Schubert misstates an important Black opinion (p. 189, n. 52), misinterprets the 1964 sit-in decisions, and describes Black as “leader of the die-hard opposition to the Court's expansive policy of racial egalitarianism” (p. 119). To anyone who has noted Black's powerful language in 1969 as Circuit Justice for the Fifth Circuit when he set the groundwork for *Alexander v. Holmes* (the Burger Court's decision outlawing “all deliberate speed”), Schubert's charges must seem cruelly unfair.

True it is that Black's innate traditionalism, like that of many another liberal gentleman, quickly surfaced in the mid-1960s in reaction

to the shock waves of increasingly militant protest, ghetto rioting, and the dramatic polarizing of generations. Yet even then Black's opinions were still grounded in constitutional formalism (even the Vietnam arm band opinion, painful reading though it is) against a backdrop, it must be remembered, of unprecedented judicial activism. At bottom Schubert's accusations are reduced to a complaint that Black's “policy outputs” were too conservative for the times.

I have other quarrels with the book, such as Schubert's prejudicial treatment of radicalism when he contrasts it to conservatism and liberalism, and his casual labeling of Justice Douglas as a “spokesman for radicalism.” There is no index, no bibliography, no table of cases; and the footnotes are at the back without benefit of page references at the top.

Despite these criticisms, *The Constitutional Polity* is well worth reading. It has much to say, and in compressed space too, about the Supreme Court since World War I. Schubert's writing is smooth, lucid, taut; even his statistics are handled gracefully. What a shame that so perceptive and mature a scholar should feel bound by the thesis that one must make a measurable science out of human (that is, judicial) behavior.

ARNOLD M. PAUL

Stanford Law School

R. ALAN LAWSON. *The Failure of Independent Liberalism, 1930-1941*. New York: G. P. Putnam's Sons. 1971. Pp. 322. \$7.95.

The title of this study does not clarify its contents. It is an intellectual history of a diverse collection of social critics of the 1930s who operated somewhere in the unchartable area between the New Deal and doctrinaire Marxism. Lawson calls his thinkers “independent liberals” (there are ten to twelve major ones, but Lawson does not confine himself to them), and his problems begin at once. The men seem independent enough, but they were certainly not all liberals, even in the broad meaning of the term. Note the leading figures: John Dewey, Lewis Mumford, Howard Odum, Horace Kallen, Louis Adamic, George Soule, Stuart Chase, Alfred Bingham, Lewis Lorwin, Herbert Agar, Allen Tate, Archibald MacLeish, and Waldo

Frank. Lawson grapples resourcefully with their diversity. He discerns a group of "pragmatic rationalists" whose guide was experimental reason and who usually favored some sort of collectivist planning. Then there were the "liberal traditionalists," men who would have preferred a society and social policy that strengthened organic social ties; the latter are further subdivided into "regionalists," such as Mumford, Odum, and the Southern agrarians, and "cultural pluralists," such as Kallen and Adamic. Later Lawson adds the "Great Tradition" spokesmen as another subcategory—men such as MacLeish and Waldo Frank.

I do not, it must be clear, find Lawson's organizing categories successful. Categories and labels multiply until they are often a hindrance, not a help. The book is further fragmented by an episodic organization in which men are analyzed in brief sections, often several times, and there are occasional discussions of the appeal of fascism, literature in the 1930s, the cooperative movement, and other subjects whose relation to the argument is not always clear. Even the word failure does not unify these men, for, as the author admits, they failed only to secure their social goals. Actually, he finds of most of them that they "analyzed cogently, often profoundly, and were not discredited by events."

Despite these flaws, the book is valuable. The intellectuals who remained critics of the dominant liberalism of the 1930s have long needed analysis, and Lawson is a perceptive, thoughtful critic. He has given us our first historical study of the careers of Stuart Chase and George Soule, who were not only impressive social commentators but men of some intellectual influence. Lawson's extended treatments of Dewey and Mumford are substantial contributions. The parts of the book add up to more than the whole, for it was impossible in the end to make a whole out of these careers.

There are no startling conclusions here, but students of the 1930s will be interested to note, for example, the shift in mood and focus that came to all the "independent liberals" after 1936 with the popular endorsement of the New Deal and the approach of war between totalitarian states. At the end of the volume Lawson is discussing Paul Goodman, a characteristic diversion in a pleasantly meandering book. But

along the way we have had new insights into some familiar and a few unappreciated intellectuals. The Allan Nevins Prize was granted the book not for its organizational mastery, one assumes, or because it is entirely free of opaque phrase, but for the general gracefulness of its style and the balance of its judgments.

OTIS L. GRAHAM, JR.

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CHARLES F. O'BRIEN. *Sir William Dawson: A Life in Science and Religion*. (Memoirs of the American Philosophical Society, Volume 84.) Philadelphia: the Society. 1971. Pp. vii, 207. \$3.00.

John William Dawson is a significant nineteenth-century Canadian geologist, educator, and author worthy of a first-rate biography. Charles F. O'Brien's work is basically an intellectual history treating Dawson as a controversialist. Thus the reader should be forewarned that O'Brien's book, despite its title, is not a detailed account of Dawson's career either in science or religion, but of his participation in several nineteenth-century scientific and religious controversies. The disputes considered are geology and Genesis, polygenism, evolution, the *Eozoön* controversy, and the issue of continental glaciers versus icebergs in accounting for the effects of glaciation. With the exception of the last issue, O'Brien views these disputes as having a rational connection to Dawson's overriding interest in combating evolution and in presenting "his Paleyite approach to science." And O'Brien notes with appropriate insight that "the key to Dawson's career as a controversialist was his use of Canadian materials to enter the mainstream of nineteenth-century controversy." In sum, O'Brien shows an understanding of the complex scientific and religious issues in dispute and traces with care Dawson's and his opponents' views. The result is a contribution to the history of science.

In justifying his study, O'Brien maintains that Dawson's "historical relevance in carrying on the search for a synthesis of the two theologies—natural and revealed—is much greater than his contributions to either education or natural science." One has the feeling that the wish is father to the thought, and the evidence

presented or omitted nourishes a skepticism of O'Brien's thesis. While one may portray Dawson as a man applying a keen mind to a losing cause, the resulting portrait is unfinished. To diminish his contributions to geology in general and paleobotany in particular, to slight his administrative abilities and successes in the development of McGill University, and to treat less significantly his share of professional scientific honors—a not unreliable indication of his scientific reputation—requires further reflection upon Dawson's historical importance.

In his "Notes on the Sources," O'Brien wisely recognizes the quality of A. Hunter Dupree's *Gray* and Edward Lurie's *Agassiz*; it is to be lamented that he did not follow that tradition in providing a well-defined portrait of Dawson.

WALTER L. BERG

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DARCY RIBEIRO. *The Americas and Civilization*. Translated from the Portuguese by LINTON LOMAS BARRETT and MARIE MCDAVID BARRETT. New York: E. P. Dutton and Company. 1971. Pp. 510. \$15.75.

Darcy Ribeiro, a Brazilian anthropologist, might better have entitled this book *Latin America: Victim of Imperialism*. Decked out in the traditional trappings of scholarship (footnotes, bibliography, and scatterings of social science jargon), the work presents a politically partisan attack rather than a scholarly analysis. Using a historical approach within a Marxian ideological framework, the author sets out to investigate the causes of the material progress of the United States and the economic backwardness of Latin America. He concludes that imperialism is the sole cause. Even historians who subscribe to such a simplified view are likely to be disappointed in Ribeiro's ineffectual attempts at historical proof.

The general tone of the study is quickly set in the portrait of Iberian civilization, which is described in the crudest terms of the Black Legend. "Terrorism," "oppression," "torture," "salvationistic fanaticism," and "plunder" are typical of the terms applied by Ribeiro to the Latin American colonial regimes. The author's credibility in the field of history may possibly be measured by his charge that "viruses, bacilli,

and germs" were used as "one of the decisive weapons of the conquest."

The wicked empires of Spain and Portugal are contrasted to the benign imperial constructions of the Aztecs and the Incas. The author glosses over the bloody and forceful processes of Indian imperialism and would have us believe that these theocratic empires "crystallized." Slavery and serfdom in Aztec society, so obnoxious to Ribeiro in Europe, are depicted as merely parts of a "stratified social structure." The practice of human sacrifice on a lavish scale is passed off as part of a world conception in which the Aztecs were supporters of "life and prosperity for all." The author's failure to mention the Inca custom of *mitima* or *mitimaes*, by which large population groups were uprooted and moved hundreds of miles for the convenience of the empire, is curious in the light of his denunciation of European nations for allegedly using colonists as human cattle. Obviously a double standard is applied in the judgment of European and Indian empires.

The depiction of Anglo-American history in this work is an outlandish caricature that abounds in factual errors and naive interpretations. To cite only a few egregious examples, Ribeiro states that English colonists came to America because they were displaced by the expansion of cattle raising; independence was "won as a project of whites of the north, splitting the nation . . . in the fight to abolish slavery"; and the Founding Fathers, during the American Revolution, sought to monopolize the colonial exploitation of Latin America. Since the early days of nationhood, we are told, the United States has engaged in an international plot to retard the development of the rest of the hemisphere. The current feeble policy of the United States of fostering world population control is seen by Ribeiro as a sinister "power policy aimed at continuing the dependence of Latin America."

Those interested in a serious, scholarly treatment of the negative influences of imperialism on Latin America will be well advised to look elsewhere. The writings of Victor Raúl Haya de la Torre and Manuel Ugarte are more informative in many respects on the subject of imperialism. The chief utility of Ribeiro's study lies in its contemporary presentation of

the anti-imperialist views of the political Left in Latin America.

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Hayward

NORMAN NEWTON. *Thomas Gage in Spanish America*. (Great Travellers.) New York: Barnes and Noble. 1969. Pp. 214. \$4.75.

Thomas Gage's perceptive but malicious description of life in seventeenth-century Spanish America was, and still is, controversial. To read it judiciously, one must know the author well, and Norman Newton's book is adequate for this purpose. However, it is no substitute for Gage's original *The English-American: His Travail by Sea and Land* (1648), nor for its scholarly versions edited by Arthur P. Newton (1928) and J. Eric S. Thompson (1958). Rather, this is a popularization, an artful paraphrase and condensation of the original text flavored with a few direct quotations and extended with additional information on Gage and his milieu.

In this treatment the protagonist emerges as something of a scoundrel. Although trained by the Jesuits, Gage became a Dominican; having volunteered for missionary work in the Spanish Philippines, he deserted on reaching Mexico and fled to Guatemala. There, first as a college teacher and then as an Indian village priest, he amassed a tidy fortune in twelve years, but during his unauthorized return to England in 1637, he lost it all to pirates. In troubled England he renounced his Roman Catholic religion and then his Anglican faith. Espousing the Puritan cause and conspiring against his Catholic family and friends, he published his vindictive travel memoirs (as propaganda for Oliver Cromwell's "Western Design"), and served as a chaplain in the English conquest of Spanish Jamaica in 1655. There he died the following year.

Newton makes no attempt to whitewash Gage's unsavory acts, but neither does he accept the persistent allegation that Gage, while serving as a friar in the Spanish colonies, was actually a spy for the English government.

MAX L. MOORHEAD  
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CELSE FURTADO. *Economic Development of Latin America: A Survey from Colonial Times to the Cuban Revolution*. Translated by SUZETTE MACEDO. (Cambridge Latin American Studies, Number 8.) New York: Cambridge University Press. 1970. Pp. xvi, 271. Cloth \$10.50, paper \$2.45.

Celso Furtado is one of a small group of Latin American economists with a substantial academic reputation in and beyond Latin America who have held important policy-making positions and whose writings have been widely circulated in English. His best known monograph—at least to historians—is *The Economic Growth of Brazil: A Survey from Colonial to Modern Times* (1963). The similarity in the subtitles of this work and the book under review requires comment because in contrast to the Brazilian study, this book is not a survey in any historical sense. The colonial period is disposed of in pages 8–18 and the nineteenth century is covered in the following fifteen pages. The remaining 260 pages of text deal with the twentieth century, effectively with the years since 1929. A sense of the past as an integral part of the present, however, pervades the whole study, and so in introducing topics such as agrarian reform or the Cuban Revolution, Furtado provides succinct and provocative summaries of past events designed to explain why things now happen as they do and how the past conditions present-day political responses to economic pressures.

Furtado is the most prolific and one of the more persuasive proponents of the position that effective analysis of development requires special formulations to fit various historic processes if it is to be precise and pertinent. The past, however, can be viewed differently by economists as well as by historians. His readable expository style as translated by Suzette Macedo makes it possible for the noneconomist to bring Furtado's vision easily into focus. The obvious danger is that such a person might accept his arguments without any awareness of alternative economic approaches to the situation. His is a historically informed and rationally ordered, if somewhat evangelical, mind at work—the founding rock of the church in this instance being the Economic Commission for Latin America.

The gospel of this text is that external forces are primarily responsible for the dependent

state of Latin America's economy and that national planning must be in response to this situation. A few of the many examples cited in their specific national and regional historical context are unfavorable terms of trade for agricultural and mineral exports; the extent to which such traditional exports inhibit the capacity for innovation and reduce land ownership to a mechanism for extracting a surplus from an economy with a low level of production rather than serving as a basis for organizing agricultural production; inelasticity of market; import-substitution limited to nondurable goods; the braking effect of the gold standard and the structure of international finance because of the high volume of foreign exchange reserve required; the difficulties in developing technological competence as new industries are simply local subsidiaries of international groups, mainly North American; and a new and complex form of dependence on the outside world through the growing use of international credit agencies such as the World Bank as financial intermediaries for national governments. The variations in national situations are drawn from the cases of Argentina, Brazil, Chile, Colombia, and Mexico and after 1950, Venezuela and Peru.

One does not have to accept the theoretical validity of Furtado's position, but one would be ill advised to attempt an understanding of what is taking place in most Latin American nations today without a comprehension of his vision of the "wide process of social change as seen in the context of historical reality." It is more fully available in this book than in any of his earlier essays.

JOHN P. HARRISON  
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Coral Gables

FREDERICK C. TURNER. *Catholicism and Political Development in Latin America*. Chapel Hill: University of North Carolina Press, 1971. Pp. xv, 272. \$8.75.

Since Fidel Castro came to power, the social and economic crises that afflict most of Latin America have aroused expressions of concern among some of the region's lay and clerical Catholic leaders. Professor Turner's book examines and vigorously applauds these newly

emerging patterns of Catholic thought. According to Turner, who has assembled an impressive array of interviews, pastoral messages, and periodical articles to buttress his case, increasingly numerous and influential elements within the laity and the clergy have reoriented their goals and are calling for an activist, socially concerned Church that encourages participation in reformist political movements. Although he admits that powerful traditionalist elements oppose the reformers, he hypothesizes that already between one-fifth and one-third of the clergy is "progressive," and he is cautiously optimistic that reform trends will continue. Eduardo Frei, Christian Democratic president of Chile between 1964 and 1970, becomes for Turner a model of what concerned Catholic laymen can achieve. United States foreign policy, we are advised, ought discreetly to throw strong support to progressive Catholic political groups that achieve power.

The book is packed with worthwhile anecdotes and interesting information, much of it new. Career sketches of reform leaders like Father Camilo Torres and Archbishop Hélder Câmara, along with illuminating analyses of the Church's accommodations with authoritarian political leaders ranging from Stroessner in Paraguay to Castro in Cuba, make for lively reading. The book may achieve one of the author's goals, to stimulate public attention in the United States to the changes taking place within Latin American Catholicism. But Turner, in my judgment, does not prove his case that the reformers represent the wave of the future. The book unfortunately avoids studying the exercise of power within the hierarchical and bureaucratized Latin Churches. The seemingly endless quotes from progressive Catholic intellectuals that the author employs hardly add up to a convincing argument that the hierarchy in most countries is willing to make basic policy changes. Turner apparently hopes that an aroused laity will eventually force the leadership to change course, but the book's lack of empirical evidence as well as its strong reliance on the probably unique Chilean case weaken the argument. As Turner himself admits, we need much empirical research on Latin American Catholicism.

CARL SOLBERG  
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HUGH THOMAS. *Cuba: The Pursuit of Freedom*. New York: Harper and Row. 1971. Pp. xxiv, 1696. \$20.00.

This is a remarkable book—all 1,700 pages, 116 chapters, and 14 appendixes, 11 “books,” an epilogue, and five and a half pounds of it. It is not simply that it is monumental, virtually encyclopedic; it can also be fairly described as balanced, thoroughly researched, and insightful. For the narrative of the decade from 1952 to 1962 it might be considered definitive.

The author characterizes his own study: “Half of it, plainly, is history; but in the second half I enter upon contemporary politics, and by the time the Revolution of 1959 is reached I am in a no-man’s-land between history, politics, sociology and journalism.” In a more Orthodox way Thomas has set himself the task of presenting the two centuries between the British capture of Havana in 1762 and the missile crisis of 1962. The post-1962 years are treated more summarily in the epilogue, and narrative is secondary to analysis.

The initial date of 1762 is more defensible than the terminal one of 1962. Prior to 1762 Cuba had been largely stagnant; after the British withdrawal in 1763 as part of the general settlements of that year, conditions in Cuba were never really the same, even during the next century of seemingly solid Spanish control. But to end the study, in effect, with the resolution of the missile crisis is unfortunate. Granted that October 1962 was a crisis point, the author’s sources for the next half dozen years must have been virtually as good as for the preceding half dozen and, recognizing the author’s wariness of trying to write “recent history,” it still would have been most useful to have his same detailed treatment of the rest of the 1960s.

Professor Thomas (of the University of Reading) has consulted an amazingly wide variety of sources; much of the account of the 1960s is drawn from personal contacts made on a number of trips to Cuba. In very skillful fashion he weaves together, on an almost day-to-day basis for critical periods, the tangled skein of developments.

Thomas rigorously excludes incidental value judgments, so often introduced even unconsciously by writers on controversial topics in

their choice of adjectives, their introduction of casual laudatory or critical phrases, and in other ways. In certain chapters or parts of chapters, of course, he designedly enters into analysis and evaluation and in them he does not hesitate to speak his mind. Perhaps one of the severest tests of the objectivity of any writer on contemporary Cuba is how he deals with Fidel Castro. In general Thomas goes right down the middle, viewing the *líder máximo* neither through the rose-tinted glasses usually worn by Herbert Matthews nor with the passionate and pathetic hostility of, for example, Manuel Urrutia. His stance as an Englishman adds to his objectivity.

For the most part the study is oriented toward political developments, but occasional chapters digress to deal with social organization (in both a broad and a narrow sense), economic problems and activities, the cultural and literary scene, and other matters. The appendixes introduce a wide variety of materials of genealogical, ethnological, statistical, anecdotal, and other sorts. Useful capsule biographical information about some of the lesser actors in the marathon unfolding is occasionally included in the meticulous footnoting.

In not too obtrusive fashion Thomas deals from time to time with the problem of his volume subtitle, the Cuban pursuit of freedom. In his concluding pages, for example, he says: “Yet the obsession with freedom creates its own bondage, and is there not doubt whether in any real sense even Castro is a free man? He imposes his personality on Cuba but, like all Cuban rulers, he is at the mercy of the sugar markets as of the twenty-year relative stagnation in the Cuban economy which he has not arrested. In part, too, he is the creation of the dreams of Cubans for a revolutionary leader of epic stature, just as he is the articulate expression of a nation whose ‘authentic qualities’ include what is usually known as ‘gaiety.’ . . . The long shadows of past habits stretch across the most radical reforms, either blacking them out or giving them quite different colours.”

This study goes far toward clarifying the colors in the confused kaleidoscope that is contemporary Cuba.

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FRANKLIN W. KNIGHT. *Slave Society in Cuba during the Nineteenth Century*. Madison: University of Wisconsin Press. 1970. Pp. xxi, 228. \$10.00.

The hypothesis that slavery in the Iberian colonies was more humane than that of the Anglo-Saxon colonies and countries was given scholarly repute by Frank Tannenbaum in his book, *Slave and Citizen: The Negro in the Americas* (1946). Later writers developed the Tannenbaum thesis, including Herbert S. Klein who produced a book, *Slavery in the Americas: A Comparative Study of Virginia and Cuba* (1967). Contrasted with the arbitrary power of slaveowners in Virginia, Klein contends that Cuban master-slave relations were tempered by the intervention of priests and public officials. Moreover, he points to urbanization and agrarian diversification as factors influencing the lighter work load of slaves in Cuba compared with those in Virginia.

Franklin Knight has marshaled literary and statistical evidence that clearly modifies the Tannenbaum-Klein thesis as it pertains to Cuba in the nineteenth century. He finds that a high proportion of the slaves were rural and attached to sugar plantations, that a new slave-owning class emerged that was hostile toward the humanitarian attitude of Church and state, that the practice of manumission declined as the demand for labor increased, and that slave codes were not enforced. Overwork and underfeeding made the slaves susceptible to disease and premature death. Most damning to the myth of humane treatment was the net rate of slave population decrease, which ranged from three to five per cent per annum in the middle decades of the nineteenth century.

Sugar and slavery have had a centuries-long relationship whereby great wealth has accrued to Europeans and North Americans who combined their skills and capital with imported labor on tropical plantations. On successive frontiers the factories and cane fields have used up the labor of millions of Africans who were often cheaper to buy from the slavers than to breed by means of humane treatment by their masters. As a West Indian himself, Franklin Knight rightly contends that comparative study of slave systems should consider both the nature of the slave society and the fact of the sugar revolution. Moreover, he faults earlier

writers whose comparative studies of slavery neglect equivalent stages of economic and social growth and exaggerate the influence of metropolitan institutional differences.

I find a few faults in the book, though they are minor in the light of the important contribution it makes. Plantation agriculture in Cuba was not distinctive with respect to technical innovation. While it is true that Cuban planters invested in railroads and steam factories, planters elsewhere in the Caribbean had earlier been technologically progressive in adopting windmills, watermills, and irrigation systems. Since the mechanization of sugar processing coexisted with hand labor in cane cultivation and harvesting, it is difficult to follow Knight in his contention that the demise of slavery was caused in large part by the revolution in the manufacture of sugar. More important to the demise was the substitution of cheap indentured laborers from China and India.

RICHARD B. SHERIDAN  
University of Kansas

D. A. BRADING. *Miners and Merchants in Bourbon Mexico, 1763-1810*. (Cambridge Latin American Studies, Number 10.) New York: Cambridge University Press. 1971. Pp. xvii, 382. \$16.50.

Armed with the methodology of the social scientist, many young scholars of late have revisited colonial Hispanic America with profit. Dr. Brading is one of them. His book on Bourbon Mexico is an exhaustive effort based almost entirely upon Spanish and Mexican documentation.

The author's purpose was to define this crucial period in both human and economic terms. As his vehicle he chose three autonomous studies: first, "Revolution in Government," in which he presents the background and evaluates such factors as the visitation of José de Gálvez (1765-71), the expulsion of the Jesuits (1767), and the Bourbon reforms; second, "Miners and Merchants," in which he analyzes their origins, activities, and institutions; and third, a detailed examination of the prosperous Guanajuato mines and community. He expertly uses notary records, petitions, genealogy, production figures, and numerous tables and graphs, many of which are reproduced and

discussed in the text. His conclusions are sound and convincing.

Guanajuato's remarkable productivity resulted from the Bourbon reforms and incentives, the availability of merchant capital in the late 1780s, the miners guild (1783), and the investment skills of the entrepreneurs. Dr. Brading's analysis of Mexican society is another rewarding feature of this work as he traces the emergence of a new, aggressive elite consisting of immigrants from northern Spain. Favored by the government and relatives in Mexico, these moral and hard-working Spaniards soon reached the top of the economic structure and consolidated their position in society by marriages to Creole women, by purchases of haciendas that they entailed, and by securing titles of nobility. Unable to compete, Creole men suffered accordingly. The situation worsened in the days before independence.

The Guanajuato study is unquestionably an outstanding contribution. The earlier sections, however, are merely exploratory. By opting to concentrate on structures rather than the operational aspects of reform, the author limited the validity of his conclusions. Moreover, the emphasis upon central Mexico and the neglect of outlying zones—the Consulados of Guadalajara and Vera Cruz, for example—ignored a comparative element in what was also Bourbon Mexico. Considering the importance of contraband elsewhere in Spanish America, it is likewise difficult to believe that it played no substantial role in New Spain. Let us hope that Dr. Brading will continue to develop the first two themes so that they may reach the level of his excellent work on the mining community of Guanajuato.

MARIO RODRÍGUEZ

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JAN BAZANT. *Los bienes de la Iglesia en México (1856-1875): Aspectos económicos y sociales de la Revolución liberal*. (Centro de Estudios Históricos, New Series, Number 13.) [México, D. F.:] Colegio de México. 1971. Pp. xiii, 364.

JAN BAZANT. *Alienation of Church Wealth in Mexico: Social and Economic Aspects of the Liberal Revolution, 1856-1875*. Edited and translated by MICHAEL P. COSTELOE. (Cambridge Latin American Studies, Number 11.) New York: Cambridge University Press. 1971. Pp. xvi, 332. \$17.50.

Serious work on the material resources of the Catholic Church in nineteenth-century Mexico is now well under way. The present study is so far the most ambitious and adds significantly to the research of Michael Costeloe (Bazant's English translator), Charles Berry, and Robert Knowlton. Bazant has not, however, written the definitive study on Church wealth, as he readily admits. His investigation did not encompass the whole of Mexico, and he did not utilize all the major resources available, for example, the diocesan archives in the areas covered. Furthermore his economic analysis is open to question because he fails to establish a solid basis for computing the market value of property. Finally, his acceptance of certain statistics—for example, that there were 3,066 houses in Puebla for a population of 70,000 and 4,000 in Mexico City for a population of 200,000—casts doubt upon the conclusion that the Church controlled about half the real estate in these two cities. Some questions, of course, may never be answered with any precision either because of the destruction of relevant records or because of inaccurate or incomplete data from the beginning.

Bazant has settled in a general way some of the arguments raised in the bitter polemics that have raged for over a century between clericals and anticlericals. First, his best estimate of the value of all Church real estate and mortgages is between seventy and eighty million pesos (in contemporary not present-day currency). This is a far cry from the oft-repeated claim that the Church owned one-half of the wealth in Mexico, even if we accept the extraordinarily low estimate of 340 million pesos as the value of all real estate in Mexico in 1870. On the other hand the official method of computing the value of Church holdings by capitalizing the rents and the interest payments on mortgages at five per cent may well under-value the properties. Whatever their value Church holdings were divided approximately five-sixths in urban property and only one-sixth in rural.

Second, the Ley Lerdo of 1856, which required the Church to sell its real estate but permitted it to hold the mortgages, and the expropriation law of 1859 had their roots in Mexican history both in the colonial and the national periods. In a sense the laws on Church

property of the Reforma period are a culmination of historical precedents, not a sharp break with the past. Conservative as well as Liberal governments sought to acquire Church property to pay their debts in the years prior to the nationalization in 1859. The primary difference was that the Church parted cautiously but willingly with its property to the Conservatives and most unwillingly to the Liberals; but part with it she did. Ironically Conservatives (as well as Liberals) acquired Church properties during the 1850s and 1860s, became committed to the changes, and refused to consider returning the properties to the Church under any circumstance.

Finally, while vast amounts of property fell into relatively few hands (businessmen, professionals, government officials, and landowners), a substantial number of modest-income families did gain possession of their residences. Rural property largely fell to the already wealthy, and while foreigners got possession of a substantial amount of urban property they obtained almost no haciendas. Certainly a rural middle class was not created as the Liberals hoped it would be. The national treasury profited only marginally from the expropriations, largely because the government of Juárez was hard pressed for funds to fight first a civil war against domestic enemies and then against the French invaders. Under these difficult circumstances, property was at times sold off for less than three per cent of its assessed value.

The English version is a faithful translation of the original and basically parallels it except for some minor alterations. Appendix 1 in the original is included in the introduction of the English translation and four other appendixes are omitted since they have already appeared in English. There are also a fair number of changes in the bibliography and some rather substantial alterations in the English preface, which, however, is signed by Bazant.

This is a major work. It is not easy to read, especially for laymen unversed in banking, finance, and mortgage contracts. Its organization and style leave much to be desired, but its findings are often fascinating and its conclusions provocative.

KARL M. SCHMITT  
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PAUL FRIEDRICH. *Agrarian Revolt in a Mexican Village*. (Anthropology of Modern Societies Series.) Englewood Cliffs, N. J.: Prentice-Hall. 1970. Pp. xvi, 158. Cloth \$5.95, paper \$3.25.

ROGER D. HANSEN. *The Politics of Mexican Development*. Baltimore: Johns Hopkins Press. 1971. Pp. xiii, 267. \$11.00.

CLARK W. REYNOLDS. *The Mexican Economy: Twentieth-Century Structure and Growth*. (Publication of the Economic Growth Center, Yale University.) New Haven: Yale University Press. 1970. Pp. xxiv, 468. \$13.50.

The three books under review admirably demonstrate the improvement of social science research on Latin American topics that took place during the 1960s. The professional backgrounds of the authors are varied: Friedrich is an anthropologist, Hansen is (presumably) a political scientist, and Reynolds is an economist. Yet, all contribute in ways appropriate to their disciplines to a more sophisticated understanding of twentieth-century Mexican history.

Professor Friedrich's work is surely innovative among anthropological studies of Mexico. Departing from the tradition of community studies (engendered decades ago by the Chicago school of urban sociology) he explores the making of a revolutionary, Primo Tapia. In the process he calls attention to the peculiar importance of living in the United States for the returned Mexican immigrant and the exigencies of race and class conflict in the making of a revolution.

Primo Tapia led the agrarian revolt in his native village in the 1920s and achieved the goal of returning a Spanish-owned hacienda to community control in 1926—not long before his assassination by government troops. Two aspects of his personal experience were of major importance in his preparation for revolutionary leadership: his attitude toward authority and his experience with anarcho-sindicalism and the Wobblies in the U.S. As Friedrich puts it, "Primo had strong hatreds. The positive struggle for the fertile black soil was complemented by unmitigated hostility against the Catholic clergy, the Spanish landlords, and the entire network of exploiters, that is, against all who were not impoverished peasants, industrial workers, or their representatives. . . . His hostility toward authority and his learning of anarchist theory partly meshed with

the vigorous local autonomy for which the Tarascans are renowned and for which the Naranjeños are particularly conspicuous" (pp. 74-75). Tapia imbibed anarcho-syndicalism among the Wobblies of California and in 1920 even led a strike against a beet sugar refinery in the state of Nebraska. His training for revolution thus came in the U.S. During the agrarian revolt itself he succeeded in uniting poor mestizos in the neighborhood to the cause of his fellow Tarascans. He was then instrumental in turning a race conflict between mestizos and Indians into a class conflict between the large landholders and the landless, Indian or white. These aspects alone of Friedrich's study should encourage historians of the United States to examine his findings. This book should prove useful to those who seek to learn more (and to convey to their students) a better sense of the relationship between the U.S. and Mexicans.

More topical perhaps is Hansen's study of policy making for economic development. It seems an excellent summary and critique of the extant literature on the subject, and it correctly emphasizes the central problem of Mexico's economic success story: who gets (or got) what? One of his chapters is appropriately titled "The PRI and Mexican Politics: La Cosa Nuestra." Some readers will be disappointed by Hansen's reliance on already well-known secondary sources and may well ask what, if anything, is new in his book. However, they also serve who only synthesize.

Perhaps the more striking synthesis (as well as much more) is that achieved by Reynolds in his investigation of Mexican economic history in the twentieth century. It is difficult to imagine a major development problem that this author has not treated in his work. Thus one attaches considerable weight to his conclusion that "it is difficult to see how an alternative mixture of public policies could have increased the rate of growth or substantially improved the level of income of all sectors of the population since 1940, and it is easy to imagine how more extreme measures might have retarded the growth rate" (p. 310). The agrarian revolution of the pre-1940 period and, more important, the creation of the ethos of revolution were a necessary prelude to the rapid growth of urban activities and the highly efficient plantation agriculture of the North and Northwest.

Reynolds suggests that this "attitudinal" factor may explain up to forty-five per cent of Mexican economic growth in the years between 1925 and 1960 (p. 56). Agrarian reform returned some land to the tiller, moderated the rate of cityward migration, and turned the attention of the poor away from the growing wealth of the rich. The extraordinary economic growth of the past quarter century (exceeded only by Japan and some European countries) may not have been possible had the fruits of growth been more equally distributed.

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ISAAC S. and SUZANNE A. EMMANUEL. *History of the Jews of the Netherlands Antilles*. Volume 1, *History*; Volume 2, *Appendices*. Cincinnati: American Jewish Archives; distrib. by Ktav Publishing House, New York. 1970. Pp. 533; 539-1165. \$35.00 the set.

Curaçao, captured by Holland in 1634, became the New World haven for Sephardic Jews. Jewish merchants aided the island's development, achieving prominence in the eighteenth century when their 280 families comprised half of the white population. The "Portuguese Jewish Nation" of Curaçao functioned as a separate estate controlled by wealthy congregation trustees. Since their decisions were enforceable by civil authorities, trustee authoritarianism often aggravated intra-Jewish tensions and, after equal civil status had been obtained, finally resulted in the schism of 1864. Since then Curaçao Jewry has declined in importance and religious zeal. Such is the focus of *History of the Jews of the Netherlands Antilles*, which, belying its title, is actually a study of Curaçao's Sephardic Congregation Mikveh Israel, the oldest in the Western Hemisphere.

Dr. Isaac S. Emmanuel served as rabbi in Curaçao where he wrote *Precious Stones of the Jews of Curaçao* (1957), a masterful study of twenty-five hundred tombstones patterned after his genealogical research in Salonika. His latest handsomely illustrated effort is based on exhaustive archival research in Curaçao and Holland that provides an insight into the problems faced by small Jewish enclaves everywhere. Its wealth of detail would do justice to many larger Jewish communities. Some of the appendixes—marriages and Curaçao expatriates, for

example—furnish extremely useful data. Yet Emmanuel's work suffers in historical craftsmanship, probably because he attempts to narrate general as well as institutional history in the setting of a reference work. His inclusive approach to history emphasizes documentation over interpretation, cases rather than trends. The result is uneven and haphazard, with disjointed chapter development and blurred criteria in the selection of materials for the two separate volumes. Critical relationships, such as that of climate to occupation or invasion to foreign policy, are drawn sketchily if at all. Also missing are useful maps, population charts, and coinage tables. The index (4,400 items) concentrates on family names, while the bibliography unfortunately foregoes secondary sources. Emmanuel mistakenly assumes his reader's knowledge of Curaçao and Judaism. Several introductory chapters to provide that kind of background would have greatly enhanced the significance and clarity of this history.

Dr. Emmanuel's book is uncommonly candid for ethnic history. The various altercations he describes furnish the most readable sections. However, contradictions concerning the importance of the Jews to Curaçao or their relationships with Gentiles do appear. Furthermore the author is not entirely forthright with regard to mulattoes descended from Jews, who did have an advantaged position. Censorship by the trustees may have been responsible for some omissions, particularly where the 1964 merger of the reform and traditional communities are concerned. Emmanuel was visibly saddened when "the elegant, lucid Sephardic rite" was replaced by "irrelevant Reconstructionism," a substitution that, he maintains, will not stem the tide of Jewish assimilation in Curaçao.

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GERMÁN COLMENARES. *La provincia de Tunja en el Nuevo Reino de Granada: Ensayo de historia social (1539-1800)*. Appendices transcribed by MARÍA CRISTINA MURILLO. Bogotá: Universidad de los Andes, Facultad de Artes y Ciencias, Departamento de Historia. 1970. Pp. 283.

This is one of a series of archivally based mon-

ographs on colonial New Granada now being produced by a small number of able young Colombian historians of modern orientation. The emergence of this new generation represents an important development in Colombian historiography. Previous Colombian scholars had toiled through the archives, but the work now being done is more fruitfully informed by the concerns, methods, and interpretive insights of the most sophisticated and innovative social historians now working on colonial Spanish America.

Colmenares's monograph on Tunja is a preliminary exploration, more of a report from the archives than a finished work. It has interesting ideas throughout but lacks an integrating structure. Some material that may be useful to future historians is not placed at the service of an interpretive point. The work is not pulled together even in a formal sense: the chapters lack unifying introductory and concluding paragraphs; nor is there a general conclusion at the end. This is in contrast with Colmenares's sprightly book on mid-nineteenth century politics, *Partidos políticos y clases sociales* (1969), a work more in the traditional literary mode but one of structure and confidently asserted interpretation. While less satisfying esthetically, Colmenares's current tentativeness does reflect a greater sobriety in confronting the complexities of social analysis in a field in which data is still very sparse.

The Tunja monograph discusses sketchily aspects of Indian social, economic, and religious organization before and immediately after the Spanish Conquest. Later sections provide a more substantial treatment of Indian demographic decline and the process of *mestizaje*, the evolution of Indian tribute and other forms of colonial labor exploitation, and the constriction of Indian community lands. On each of these subjects Colmenares's findings correspond closely to those of his acknowledged mentors working on other parts of Spanish America—most notably those of Woodrow W. Borah, Charles Gibson, Rolando Mellafe, and James Lockhart. But while Colmenares incorporates perceptions from the better works on colonial society, he also displays his own very active historical imagination. He follows Lockhart, for example, in emphasizing the urban centeredness of the European population in

the sixteenth century. To this he adds the point that *mestizaje* is primarily an urban phenomenon, with mestizos forced out of the urban centers by Creole exclusivism, thereby coming to dominate the rural world of the Indian.

In the tricky field of historical demography, Colmenares is suitably cautious. He is sensibly skeptical of the reliability of *visita* figures, and he entirely avoids the problem of the size of the pre-Conquest population, considering it insoluble. Following the Colombian examples of Jaime Jaramillo Uribe and Juan Friede, he discriminates among tribute data from different regions and time periods. In his analysis of demographic decline, Colmenares emphasizes the role of Spanish labor requirements in limiting sexual contacts among the Indian population. He follows Mellafe in noting a decreasing proportion of tributaries in the Indian population, which he sees as indicating a disintegration of the family as adult males migrated to escape tribute obligations.

While this monograph does not tie all of its material into neat interpretive bundles, it is a useful and informative effort in an area in which very little research has been done.

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E. BRADFORD BURNS. *A History of Brazil*. (Institute of Latin American Studies, Columbia University.) New York: Columbia University Press. 1970. Pp. xii, 449. \$11.95.

This substantial text on Brazilian history joins two previous works by the author, *A Documentary History of Brazil* (1966) and *Perspectives on Brazilian History* (1967), to form a teacher's trilogy on that insufficiently studied giant of the Americas. In his introductory remarks Professor Burns promises a new approach to Brazil by means of thematic studies emphasizing the role of the masses. Readers will discover, however, that *A History of Brazil* provides them instead with a traditional political chronology into which intellectual, economic, and social threads frequently and effectively are woven. As a result this work will serve admirably as a useful reference work on Brazilian history or as a textbook for Brazilian history courses; it is

unlikely to engage or arouse the casual reader or the uncommitted student.

Unfortunately the first chapter lacks the occasional drama and skillful organization the author brings to subsequent sections. An effective sketch of Indian and European backgrounds makes up somewhat for an uninspired geographical description, but as a whole this chapter seems lost under a mass of detail. Such an impression only receives reinforcement from two unimaginative and amateurish maps on states and rivers that reflect little credit on Columbia University Press designers.

Professor Burns displays his considerable knowledge of Brazilian history in a far more attractive and convincing fashion in the remaining six chapters. The colonial experience emerges against the backdrop of world events and shows the mutually self-reinforcing nature of Brazilian and Portuguese development. The independence section starts with the ideas of the Enlightenment, although too soon it turns to political narrative. The description of Brazil's nineteenth-century transformation reaches high points with the detailed explanation of the impact of coffee, the communication-transportation revolution, the parallel intellectual changes, and the abolition movement. A chapter on the new Brazil, 1888-1922, examines the coffee boom of the early twentieth century, the rise of the cities, and the myriad and often confusing political events of the Old Republic. Professor Burns then advances the thesis of a conscious restructuring of society, emphasized by the 1922 Modernist movement and the rise of Getulio Vargas. The confusing events since 1945 are neatly summarized in a final chapter on reform, radicalization, and reaction.

In each chapter Professor Burns attempts to link literary and artistic movements to political and economic trends. He clearly attributes key roles to intellectual movements and changes in attitudes. Two major periods, the independence of Brazil and the Vargas era, are seen as conscious efforts involving not only the leaders but also sizable sectors of the population. Such deliberate redressing of the balance toward intellectual history richly deserves commendation and perhaps emulation by other students of Latin American history who tend to lose sight of intellectual currents in their pursuit of political or economic forces.

The book's supporting aids do not seem to reach the level achieved by the text. The first two maps, as mentioned above, are deficient; the third, regions of Brazil, shows current imbalances in population and national income in an unimaginative format. Scattered throughout the text are several well-constructed tables, and a cluster of twenty-seven photographs near the end of the book provides a visual glimpse into the variety and richness of Brazil's past. Less useful are the appendixes containing a confusing list of chiefs of state, excerpts from the five Institutional Acts of the post-1964 governments, selections from the 1967 Constitution, and a list of significant political dates. The book closes with a clearly organized list of further readings emphasizing available literature in English.

*A History of Brazil* deserves a place on the shelf of any student of Latin American history. In this useful reference work, Professor Burns has collected and organized a wealth of data, marshaled themes in chronological format, and dressed the narrative in an attractive style of writing.

JAMES R. SCOBIE  
Indiana University

GEORGE WOODCOCK. *Henry Walter Bates: Naturalist of the Amazons*. (Great Travellers.) New York: Barnes and Noble. 1969. Pp. 269. \$5.50.

- Henry Walter Bates discovered eight thousand new species, mostly insects, during eleven years in Brazil (1848-59). That should qualify him for historical mention, especially since establishment scientists of the British Museum did not believe him and had to be proven wrong. Lacking a university degree he could never land a job that would employ his entomological knowledge, and he ended his career as assistant secretary of the Royal Geographical Society.

He had long since won public approval for his superb account of the Amazon experience, and it is this unsponsored trip, which Bates began at age twenty-three in the company of Alfred Russel Wallace, that forms the core of this work. Woodcock's study is part of the Great Travellers series designed to revive popular interest in "important but neglected wanderers." That Woodcock does, but for four-fifths of the book one inevitably gets little more than rehashed Bates, albeit nicely done,

interspersed fortunately with numerous quotes in the delightful prose of Bates himself. Woodcock tries to interweave other material, but the pickings are scarce. Let the reader turn to Woodcock for introduction and conclusion, but for the trip itself there is no substitute for Bates's own account.

Woodcock sees Bates as a potential peer of Wallace and Darwin in the development of evolutionary theory. Bates let his opportunity slip by because of his compulsion to seek more data. His career never again reached the peak of his Brazilian years, and we should perhaps be grateful to Mr. Woodcock for reminding us to relive that peak through the words of Bates himself.

ARTHUR R. STEELE  
University of Toledo

JOSEPH L. LOVE. *Rio Grande do Sul and Brazilian Regionalism, 1882-1930*. Stanford: Stanford University Press. 1971. Pp. xv, 311. \$10.00.

Mr. Love presents an excellent study that is much broader in scope than the title suggests. The basic theme is the pivotal role played by the Gauchos in the decline of the Old Republic. The participation of Rio Grande do Sul in national politics is divided into four phases: dependence on military presidents, Deodoro and Floriano, 1889-94; autonomy and relative isolation under Governor Castilhos following the civil war, 1895-1903; gradual emergence as a major political force under Pinheiro Machado and Borges de Medeiros, 1904-08; and full-scale participation in national politics, 1909-30.

The era of the Old Republic (chapters two through ten) is the heart of the work. In it the author notes and provides numerous examples but does not always stress the sellout of democracy by the republicans, the failure of the republican parties to transfer power to a younger generation on the national level, the ability of São Paulo and Minas Gerais to prevail in the presidential elections if they act in concert and possibly face a revolution every four years (a revolution that the other states could only hope to win with the support of the armed forces), and the will to power of the military in conjunction with the ambitions and aims of the leaders of Rio Grande do Sul—ultimately those of the generation of 1907, as the author aptly dubs Getúlio Vargas and his contemporaries.

The work is solidly based on the personal archives of many of the leading figures. But it is curious to note that Borges de Medeiros, who provides the thread for most of the book, never emerges as a sharply defined character and is summarily dismissed in a sentence in the last chapter. This chapter, number twelve, along with the introductory chapter are the weakest parts of the work. This book, in spite of the author's occasionally guarded style, is a capital contribution to our understanding of regionalism and the Old Republic.

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RONALD M. SCHNEIDER. *The Political System of Brazil: Emergence of a "Modernizing" Authoritarian Regime, 1964-1970*. (Institute of Latin American Studies, Columbia University.) New York: Columbia University Press. 1971. Pp. xviii, 431. \$13.50.

The army leaders who took over Brazil in April 1964 hoped that the "exceptional powers" necessary for fighting "subversion, corruption, and inflation" would be few and short-lived. However, as Professor Ronald H. Schneider shows, in the years following the 1964 takeover the military leaders responded to challenges of the antimilitary opposition by abandoning all limits on dictatorship. The abandonment was done in a "legalistic" flurry of Institutional Acts, "complementary acts," and decree laws. Rules were forever being changed, often under pressure reportedly exerted by the antidemocratic "hard line" wing in the military.

As a background for this story of crises and cancellations (cancellations of the political rights of individuals), Schneider supplies "a selective summary" of his forthcoming book about Brazil, 1889-1964—a book that he has delayed publishing in favor of his present volume because, as he explains, already Thomas E. Skidmore has contributed greatly and Vladimir Reisky de Dubnic to a lesser extent to our understanding of the events leading up to 1964, whereas significant analytical literature on the post-1964 regime has been lacking.

Five chapters, consisting of 222 pages, make up the "chronological narrative," 1964-70, or "body" of Schneider's new book. This meticulous, accurate, and well-organized account of political and military matters is a tremendous

achievement—splendidly written and so complete that it is indispensable for anyone interested in learning the details of what happened. In guiding the reader from the "manipulated democracy" of the early part of Castelo Branco's regime to the "descent into dictatorship" of the last part of Costa e Silva's regime, and on through the first year of Garrastazú Médici's regime, Schneider supplies copious comments of Brazilian observers and a useful section on military education.

The book is stimulating. While political scientists can argue whether or not Schneider's final chapter is a valuable contribution to systematic political science, other scholars have the opportunity of agreeing or disagreeing with the author's pessimism. At the outset Schneider declares that "Brazil is, among the nations of the world, an adolescent giant in severe trouble." Later, pondering dicta formulated by Samuel P. Huntington, a fellow political scientist, Schneider concludes that the Médici administration fails to appreciate "the need" of developing peasant support and political institutions. Apparently writing late in December 1970, Schneider finds that "from December, 1968, on, the decay of military unity has been painfully evident," and that Médici's "prospects for serving out his full presidential term would seem to be no better than the average for the Brazilian Republic—less than a fifty-fifty proposition."

JOHN W. F. DULLES  
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Austin

J. R. FISHER. *Government and Society in Colonial Peru: The Intendant System, 1784-1814*. (University of London Historical Studies, Number 29.) [London:] University of London, Athlone Press; distrib. by Oxford University Press, New York. 1970. Pp. ix, 289. \$12.00.

A comprehensive evaluation of the functioning of the intendency in the Spanish Empire is long overdue. The neglect is difficult to explain, since the administrative innovation represents the paramount political reform of the Bourbons. The present volume by a lecturer at the University of Liverpool is a solid contribution to our knowledge of the role of this official in the viceroyalty of Peru.

Preceding its Mexican counterpart, the Peru-



vian intendancy marked another stage in the realization of the twin Bourbon goals of administrative centralization and absolutism. To the second visitor-general Jorge Escobedo was assigned the arduous task of introducing the new system, which meant in large measure overcoming the stubborn resistance of the bureaucratic establishment headed by the viceroy. There has been perhaps no more vigorous individual than Escobedo in the last fifty years of the kingdom of Peru. His career emphasizes once again the interrelationship of institutional evolution and personality. Did the adoption of the intendancy revitalize colonial government? The author's answer is a carefully qualified affirmative. The intendants managed to increase revenue for at least a decade through meticulous collection of taxes and elimination of fraud and corruption. Public administration was improved by the construction of roads, bridges, and government buildings and through the regulation of food supplies. While commercial activities were stimulated, it proved impossible to increase agricultural production. Probably the intendancy found its greatest success in the realm of municipal affairs. Moribund *cabildos* came to life by appointment of new councilors, expansion of revenues, and a program of public works. Did this mean royal oppression? The *cabildos* thought so, at any rate. That any significant alleviation of the exploitation of the Indians—dramatically disclosed earlier in the rebellion of Tupac Amaru—took place is seriously debatable.

For the most part the author's conclusions confirm the present view of the place of the intendancy in the colonial administrative system, but studies of a similar nature will be necessary to complete the picture. Generally speaking his treatment follows that of Professor Lynch, whose excellent account of the intendancy in the viceroyalty of Buenos Aires initiated scholarly investigation of this feature of government. The abundance of archival sources consulted in Spain and Peru is commendable. Undoubtedly this study will be a valuable supplement to the growing institutional literature of the Spanish Empire.

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Louisiana State University

JAMES M. MALLOY. *Bolivia: The Uncompleted Revolution*. [Pittsburgh:] University of Pittsburgh Press. 1970. Pp. x, 396. \$11.95.

JAMES M. MALLOY and RICHARD S. THORN, editors. *Beyond the Revolution: Bolivia since 1952*. [Pittsburgh:] University of Pittsburgh Press. 1971. Pp. xvii, 402. \$11.95.

Writing about unfolding social and economic revolutions poses occupational hazards because research may become outdated or need revising immediately after publication. In the Bolivian revolution, launched in 1952 and controlled or wracked by military factions since 1964, recent events cast doubt upon some of the assertions presented here. The successful revolt led by Colonel Hugo Banzer in 1971 against the radical regime of General Juan José Torres caused the most violent resistance since the *Movimiento Nacionalista Revolucionario* (MNR) seized power in 1952. But this party, which began the transformation of Bolivian society, joined forces with its fascist opponent for three decades, the *Falange Socialista Boliviana*, to back the Banzer putsch. Thus, Víctor Paz Estenssoro, major architect of the MNR who was beginning his third term in 1964, became a pathetic figure hungry for power at any price. Where is the revolution now?

The scholars who have tended to apologize for the totalitarian origins of the MNR should re-examine the historical record. Cole Blasier states that "many of these charges [of Nazi influence in 1941] were misleading, exaggerated, or false." Yet the newspaper *La Calle*, published by the MNR, conducted a vicious anti-Semitic campaign during debate on the 1942 immigration bill, in which deputies Paz and Siles both voted for the exclusion of Jews, Negroes, and Asians from Bolivia. James M. Malloy also glosses over the totalitarian aspects of the regime of Major Gualberto Villarroel (1943-46), which the MNR supported. For example, MNR journalist Roberto Hinojosa, among those whose corpses were strung up from lamp posts in 1946, had published pamphlets glorifying the "life and passion" of Adolf Hitler.

In his book Malloy writes that the depression of the early 1930s widened the chasms in Bolivian life later exposed by the disastrous Chaco defeat. He thinks the 1952 revolt oc-

curred partly because economic stagnation had dimmed the expectations of the younger and later more deeply disillusioned men of the middle sector, both civil and military, who thus became "reluctant revolutionaries." (Richard Patch earlier called Bolivia's experience a "restrained" revolution.) Malloy believes that not only was agrarian reform thrust upon these leaders, but also nationalization of the Big Three tin mines. This is inaccurate because only minor MNR figures resisted nationalization as the major goal of the MNR since Carlos Montenegro's *Nacionalismo y coloniaje* won the national literary prize in 1943.

Malloy more clearly sees the pragmatism of central MNR leaders who were forced to meet haphazardly demands of both the mine workers of the Left led by Juan Lechín (elected president of the presidium of the Popular Assembly in 1971) and the job-seeking, increasingly alienated men of the middle sector who also fought for the movement. The enfranchised Indian masses became an uncertain base of support, eventually requiring the fatal reconstruction of the Bolivian military establishment by the United States.

Political backlash exploited during the Torres regime obtained more in aid agreements from the Soviet bloc in ten months than Bolivia received from the United States in twelve years. Both Blasier and Richard S. Thorn predicted the staggering price paid in August 1971 by Bolivia for this aid. Some Bolivian writers feared that the country might become the scene of the "Spanish civil war of the Americas."

The second volume embraces a rich collection of viewpoints from various disciplines. Both books are riddled with errors in repeatedly misspelled names and improperly accented words, although Malloy uses less jargon in his contribution. Thorn gives an optimistic account of economic gains made by the revolution, the first socioeconomic one that the United States has ever supported. James W. Wilkie indicates that Bolivian control of expenditure should have been centralized and extended over the entire public sector of the economy.

Herbert S. Klein summarizes his work on Bo-

livian history to 1952, also presenting the most balanced statement of the fascist aspects of the Villarroel period. The late Carter Goodrich recalls his experiences in La Paz during the 1952 insurrection and the beginning of United Nations technical assistance, replaced later by the American efforts during the Eisenhower and Kennedy administrations.

On agrarian reform William E. Carter estimates that about one-third of Bolivia's agricultural land has been redistributed. Madeline Barbara Léons and William Léons find the impact of land reform in the Yungas area beneficial, although they emphasize that "the position of the *campesino* [Indian farmer] has [not] changed much in relation to other segments of the society." Melvin Burke compares the production of four former haciendas on the Bolivian side of Lake Titicaca with their Peruvian counterparts, finding more favorable results for the Bolivians. He thus reminds us that fragmentation of larger, possibly more economic land units is valid since the former owners did not use much of their vast acreages.

Finally, there is an excellent discussion by Murdo J. MacLeod of long-neglected Bolivian novels of social protest. He wonders if there is "something basically wrong with the Bolivian Revolution" because of the decline in quality of such literature after 1952, and his question goes to the heart of the matter. Has the transformation of Bolivian society been as profound as claimed by most scholars? The resurgence of great Mexican literature since 1910 and that of Cuba since 1959 goes without saying. Augusto Céspedes, one of three Latin American writers invited to the Soviet Union in 1971, decided to stop competing in Bolivia's literary contest in 1961 in order to encourage younger writers. But the genius of Céspedes seems lost. Who among Bolivian youth can sustain his work? They are disenchanted with the reformist trend and corruption of the MNR and its frequently repressive successors. Witness the Teoponte *foco* of 1970 and the resistance at the University of San Andrés a year later. Perhaps beyond the Bolivian Revolution lies the continued agony of Guatemala.

JERRY KNUDSON

South Dakota State University

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

You published a review of the first two volumes of *Histoire générale de la presse française* (AHR, 76 [1971]: 510-11) of which we are the editors. Somewhat surprised at its tenor, we would ask that you permit us to bring to your readers' attention the following remarks. They seem to us an indispensable corrective to the evident bias coloring the verdict returned by Professor Elizabeth L. Eisenstein on a work that has, in general, been given a better reception by other historians than by your reviewer.

Professor Eisenstein is quite entitled not to like this work but one may question the grounds on which she bases her criticism. According to her these two volumes fail "even as a coffee table showpiece," and she "cannot imagine

what group of purchasers they will please." Can your readers accept this summary dismissal without asking themselves how it was that a distinguished group of French academics could have written a work of such little interest or how a famous French publisher could have agreed to publish in several volumes so bad a piece of work? The claim that the reference apparatus of notes, index, and bibliography is very complete but that it aims to "impress the reader without much concern for actual use" will put your readers on their guard. Can they accept the opinion underlying the entire review that this history, appearing in 1969, is so out of date as to contribute nothing more than the old and learned study by Hatin written during the Second Empire? Can they believe that the authors and the publisher are so lacking in good sense that they have "chosen at random" illustrations that "rarely instruct or even entertain"?

In her more detailed comments Professor Eisenstein is still more relentless. Even without discussing all her comments, we must nonetheless reply to some so that your readers may better judge their value. Concerning the episodes in the life of the *Gazette* during the Fronde, the evidence adduced by M.-N. Grand-Mesnil is not as conclusive as Professor Eisenstein wishes to believe, and one cannot reproach M. Louis Trenard for not accepting it entirely. The numerous references the latter makes to Hatin in discussing the press of the *ancien régime* are not proof that he has merely re-copied it; rather, they should serve as a proof of intellectual exactitude and as an indication, in the many sections where it is not quoted, of the originality of the text.

An attentive reader will find no major con-

tradition between the allusions to the extent of illiteracy at the beginning (p. 159) and end of the eighteenth century (p. 402). It is difficult to understand why Louis Trenard is reproached for having adopted a very clear-cut plan that classifies, for the eighteenth century, titles of journals by category (provincial press, specialized press, and so on) and—above all—by the chronological divisions 1631–1724 and 1724–88 (which, be it noted, apply independently of the “classical” divisions of political chronology) on the pretext that “political history under the *ancien régime* is not helpful in ordering journalistic developments.”

To mention, in conclusion, only one of the criticisms made of the second volume (1814–71), it is surprising to see regrets expressed that the attitude of the Catholic Church toward the press is not discussed when, on the contrary, the book has much to say on this point both when presenting Catholic papers (pp. 70, 104–06, 128, 175, 213–15, 245, 267–69, 277, 325–26, 349) and when discussing religious problems (pp. 82–84, 295–97, 337–38).

To sum up, we consider it regrettable that readers of a periodical as highly regarded as yours must judge a work as important as this—whose next volumes (1871–1940 and 1940–70) will be appearing in the coming months—on the basis of a review that is more polemical than reasoned.

CLAUDE BELLANGER  
JACQUES GODECHOT  
PIERRE GUIRAL  
FERNAND TERROU  
*The French Press Institute,  
Paris*

#### PROFESSOR EISENSTEIN REPLIES:

I regret that it was not possible to please the editors and, at the same time, give an accurate report of their work to readers of the *American Historical Review*. I did try to balance the defects of volume 1 against the merits of volume 2 but saw no reason to conceal my disappointment that the project as a whole was modeled along the same lines as its predecessor, a hundred years ago.

There is no need to take up space with a detailed refutation of the specific objections posed by the editors. After comparing my comments

with the editors' complaints, interested readers may judge for themselves whether the latter are well founded or not. Prospective purchasers also would be well advised to ignore appeals to their credulity concerning illustrations and reference apparatus. Those interested in buying these volumes ought to look them over first.

To forestall misunderstanding I am not denying that some scholars may find it useful to own this work. I am suggesting that blurbs, prefaces, and packaging are misleading, and I think purchasers will be less dissatisfied if they have been forewarned.

ELIZABETH L. EISENSTEIN  
*American University*

#### TO THE EDITOR:

Robert Dallek's review of my *Roosevelt: The Soldier of Freedom* (*AHR*, 76 [1971]: 1503–13), strikes me as the very model of a scholarly critique: he has fairly summarized the theme and subthemes of the book, indicated the relevance of a large number of other studies, and clearly stated his own views and assumptions as well as my own. I value both his tributes to my study and his own sharply different evaluation of Roosevelt as a world leader.

Both Professor Dallek and I judge Franklin Roosevelt on the basis of our clearly stated criteria. It may be useful to note, however, that the criteria of presidential leadership in World War II that I advance are not only mine; they are also Roosevelt's. It was Roosevelt who favored and promised—and whose generals planned and pressed for—an early second front; it was Roosevelt who advocated a strong post-war role for China; it was Roosevelt who knew the weaknesses of the League of Nations and wanted a strong United Nations; it was Roosevelt who opposed colonialism; it was Roosevelt who talked about ending the old system of power politics and spheres of interest. While Roosevelt *acted* by and large in the fashion in which Professor Dallek wants presidents to act, he generally *spoke* for the kind of presidential leadership I favor, and it seems fair to evaluate Roosevelt's actions by his own precepts.

Even so, I believe that the historian has a right, if not a duty, to evaluate presidential leadership by clearly defined standards of his

own, which both Professor Dallek and I have tried to do.

JAMES MACGREGOR BURNS  
*Williams College*

TO THE EDITOR:

I learned much from the two reviews of my *Gandhi's Truth* (Joan V. Bondurant, Margaret W. Fisher, J. D. Sutherland, "Gandhi: A Psychoanalytic View," *AHR*, 76 [1971]: 1104-15). If I respond to one criticism common to both it is only because this may help to correct a widespread misunderstanding obviously caused by lack of clarity on my part.

Throughout my book I refer to the Ahmedabad strike as the Event with a capital E. Although I nowhere say so, this has been interpreted as a claim that I have found *the* key event in Gandhi's life and in the history of *satyagraha*. Actually, the capitalization of the word *event* was meant to be a literary device which would remind the reader that all the theoretical and factual strands pursued throughout the book would eventually converge on the story of the strike. This strike had become *the* event for *my book* and *for me* because industrial Ahmedabad is the Indian city I learned to know the best and some of the old men and women I met there had played significant roles in the strike and, thus, were true witnesses to it. As to the place of this event in Gandhi's life, I do claim that it was more important than has been generally recognized because it was the prophet's first nonviolent campaign in his homeland and, in fact, in his province and language area. Such a claim, after all, causes historical books to be written. But I was not so enamored with my experience and with my role that I would believe I had discovered the key event in Gandhi's long and adventurous life.

ERIK H. ERIKSON  
*Harvard University*

TO THE EDITOR:

Although I appreciate the difficulties involved in reviewing responsibly the many works that come to the attention of the *American Historical Review*, I must still protest the treatment accorded my *The Social Responsibilities of*

*Business: Company and Community, 1900-1960* (*AHR*, 76 [1971]: 1615-16).

In his pursuit of what must have been remarkably few grammatical lapses the reviewer seems to have substantially misunderstood the title and subject matter of the study, despite the fact that they were quite clearly elaborated in the preface. There it was explained, for example, that the field of labor relations was specifically excluded in order to concentrate on community relations (as the title indicated). One may question the wisdom of that decision, but I cannot accept criticism for overlooking a topic that I deliberately chose to exclude.

On the charge of having shown excessive sympathy for business leadership I am prepared to accept criticism—provided it is substantive and not merely assertive. I suspect myself that this charge may not be without foundation, although it should be noted that the book has been criticized elsewhere for being too unfriendly to business. My endeavor was to approach the material from the point of view of the executives themselves (as explained in the preface) in order to understand the factors that influenced their thought and behavior. This demands a modicum, at least, of sympathy; and it can hardly be argued that my assessment was uncritical.

I should have welcomed, finally, your reviewer's suggestions as to how I might have treated Ralph Nader in a volume whose time span (indicated in the title) ends about 1960. There is a difference between historical scholarship and contemporary polemics (referred to in the preface) that seems to have escaped him.

MORRELL HEALD  
*Case Western Reserve University*

PROFESSOR DIBACCO REPLIES:

I had no difficulty in reviewing fairly Professor Heald's book. My review was designed to convey both the merits and shortcomings of his research and writing. That the latter outweighed the former, in my opinion, is naturally disturbing to Professor Heald, but my evaluation does not constitute irresponsibility any more than reciting the Rotary motto each morning ("He profits most who serves the best") constitutes business responsibility.

Grammatical and stylistic deficiencies, unlike

wild game, need not be pursued; they have a way of making themselves known to readers. Professor Heald establishes for himself the same wide margin for error that he sets for his business spokesmen—and I am critical of such low standards.

The problem with the title is no big thing. I simply feel that a book title should not confuse the reader. *The Social Responsibilities of Business* infers that business units and their noneconomic relations with the outside world are under consideration. Professor Heald's addition of a subtitle (exclusive of dates) does not, in my opinion, convey the precision of meaning that was intended.

In my allotted four hundred-word review I did give what I believed to be substantive criticisms. Alas, Professor Heald has interpreted these as reflective of my unhistorical, contemporary viewpoint. I did not expect Professor Heald to include Ralph Nader in his study, but I do expect that a historian writing in 1970 would show more discrimination in examining the business record than Calvin Coolidge or Émile Coué.

THOMAS V. DIBACCO  
*American University*

#### TO THE EDITOR:

I very much agree with the tenor of Felix E. Hirsch's review of the memoirs of Heinrich Brüning (*AHR*, 76 [1971]: 1560–62), but I differ from his opinion in one important point, and I think a few aspects need stressing that Professor Hirsch has not mentioned.

Hirsch's review, though generally very fair to Brüning, implies an accusation when he mentions Brüning's intention to replace "his own moderate government . . . by a rightist coalition headed by Carl Goerdeler." This was not a matter of free choice for Brüning: he was aware that President Hindenburg would not indefinitely support the Brüning cabinet, which was based on toleration by the Social Democrats and which conducted a policy making such toleration possible. Under these circumstances Brüning wanted to make sure that the chancellorship would not be handed over to any rightist who might stage a coup d'état. In spite of Goerdeler's very great limitations, it would be hard to fault Brüning's judgment that among

those whom Hindenburg might conceivably be persuaded to appoint, Goerdeler was the least objectionable.

Hirsch mentions Brüning's intention to re-introduce the monarchy, but he does not draw the reader's attention to the new light thrown by the memoirs on this plan, which itself has been known for a long time. Most historians, I think, have up to now assumed that Brüning, although always a monarchist in the abstract, formed a concrete plan of restoring a Hohenzollern to the throne only as a desperate means to keep Hitler out of power (the same reason a number of Bavarian politicians in 1933 considered the restoration of the Wittelsbach monarchy), believing that the Social Democrats and other nonmonarchists would accept the restoration because, from their point of view, a Hohenzollern king was still preferable to the Nazis. Although the latter point was incontestable, even such a plan would have been unrealizable. But according to his memoirs Brüning did not think of the restoration as a crisis solution; rather, he wanted the monarch to come in after the crisis, when he himself would have found a solution for the foreign policy problems and would thereby have achieved economic recovery, abatement of civil commotion, and especially a waning of the power of the Nazis. The restoration should by no means be effected through counterrevolution but in a constitutional manner with the acclaim of the public and especially with at least the tacit consent of the working class. If a restoration of the monarchy as a crisis solution was unrealistic, this idea of a post-crisis restoration was sheer fantasy. Why should the great majority of Germans, who were not monarchists, voluntarily accept a change of regime after the most pressing problems had been solved—a change desired merely by conservative bureaucrats, big landowners, some industrialists, army officers, and a few political romanticists like Brüning himself?

The restoration plan reveals a deep contradiction in Brüning's mind. Not only his foreign policy (at least in parts) but also important aspects of his handling of domestic affairs—the way he established tolerably good relations with the moderate Left, which he had first driven into opposition in 1930—show Brüning as a leader of statesmanlike qualities; for example, his assessment of Pius XII and his rejection of

the idea of a federal concordat show a remarkable degree of realism. On the other hand, there was this absurd restoration plan and the over-estimation of the consolidating effect that would come not only from a reparations agreement but also from German equality in armament, which could satisfy only nationalistic emotions but had no bearing on Germany's practical problems. In the same category belongs Brüning's belief that his government by emergency decrees, which was acceptable to public opinion only as long as desperate conditions prevailed, amounted to a constitutional reform that would permanently give the president the position of a monarch, limited only by the purse-string powers of parliament. All this offers evidence that in addition to Brüning's world of realistic political labors, in which he acted with an admirable combination of shrewdness and honesty, he also cultivated a romantic dream world in which he had no ground under his feet.

The heavy mortgage romantic emotionalism placed on Brüning's statesmanship may provide an answer to another question. The memoirs contain nothing about Brüning's activities in the United States. He was the only one among German refugees from Hitlerism who had any chance at all to be listened to by people in the American government. Did he never try to exert some influence in opposition to the Morgenthau tendencies? The lack of evidence for

- any such attempt seems to indicate that there was none; correspondences I had with Brüning in 1941 and 1943 support the same assumption. Still there is no proof one way or the other, but what we know makes it likely that Brüning did not try to influence American foreign policy. Most probably even Brüning would have achieved nothing; yet he might have tried to reach the seats of power through Catholic circles with some influence in Washington. Surely a number of German refugees (Albert Grzesinski, Gerhart Seger, and others) would have made such an attempt if that channel had been open to them. Assuming that Brüning really kept aloof, my guess is that he realized the hopelessness of any plea for a non-Carthaginian peace for Germany, unless such a plea was combined with proposals like German disarmament and reparations for the nations victimized by Hitler, and that it was emotionally impossible for Brüning to advocate such conditions, although un-

der challenge he would surely not have denied their justification.

CARL LANDAUER  
University of California,  
Berkeley

#### PROFESSOR HIRSCH REPLIES:

I am glad that Carl Landauer and I agree fundamentally in our appraisals of Heinrich Brüning as we know him now from his *Memoiren*. Some differences of opinion that Landauer presents deserve a brief reply, however.

Professor Landauer is correct in saying that, if there was to be a rightist coalition government in June 1932, Carl Goerdeler would have been perhaps the least objectionable politician to head it. But this is a very limited compliment. As his biographer Gerhard Ritter told, Goerdeler wanted to negotiate at that point with Hitler and to offer him two or three seats in the incoming cabinet. If the Reichstag were to cause trouble, Goerdeler suggested dissolving it and having new elections two years later only. While Goerdeler was certainly a much more honorable man than Herr von Papen, I maintain he, too, would have been a very dubious chancellor for a democratic republic at such a critical juncture. Brüning, by the way, later realized Goerdeler's great shortcomings as a statesman.

I would have said more about Brüning's disastrous ideas concerning a restoration of the monarchy had the space allotted to my review permitted it. We knew from Sir John Wheeler-Bennett's *Wooden Titan* (1936), the still unsurpassed Hindenburg biography, that Brüning had seriously thought of this restoration when his regime began to falter in November 1931. But Brüning admits in his *Memoiren* (p. 146) that he had not raised fundamental objections to a restoration of the Hohenzollern rule in a discussion with General von Schleicher as early as Easter 1929, that is, long before he took office, when the Weimar Republic had not yet entered the crisis stage and when Hermann Müller's cabinet of the Grand Coalition seemed fairly stable. Later, as chancellor, it was usually Brüning, rather than Hindenburg or Schleicher, who brought the topic of restoration up for an exchange of views. Hindenburg's disloyalty to the republic is a matter of record, but Brüning, un-

til now, seemed to have been its faithful servant. Had those of us who publicly supported Brüning's campaign for Hindenburg's re-election in 1932, in spite of serious doubts about the old field marshal, known that neither of them intended to protect the Weimar Constitution, we might have hesitated to follow the chancellor's leadership. He certainly withheld his true intentions then from the German people. Most reluctantly, we may have to accept the opinion of the German historian Karl Otmar Freiherr von Aretin (*Frankfurter Hefte*, 26 [1971]: 931-39) that the real historical significance of Brüning rests in the fact that by his *Verfassungs-Untreue* he made the most important constitutional institutions of the republic ready for their destruction by Hitler.

Finally, Professor Landauer raises the question of Brüning's political role while he was in this country. He seemed rather aloof to many of his Harvard colleagues, even to a man as sympathetic to German culture as Sidney Fay. From understandable motives, the former chancellor did not choose to become an American citizen, a fact that hampered his activities in wartime. Nevertheless, he did remain deeply concerned about Germany's future. George N. Shuster, former president of Hunter College, has eloquently described Brüning's American years in his contribution to the Brüning *Festschrift* entitled *Staat, Wirtschaft und Politik in der Weimarer Republik* (Berlin, 1967). I can perhaps throw some additional light on the subject. What few people knew was that he had contacts with certain political exiles from Nazi Germany. While he disliked many refugee intellectuals, he trusted a few former Social Democratic statesmen and writers. Among them was William Sollmann, the former SPD leader in the Rhineland and minister of the interior in the Stresemann cabinet. Sollmann, a close friend of mine (see my essays on him in the *South Atlantic Quarterly*, 52 [1953]: 207-27, and in the *American-German Review*, 19 [1953]: 14-16, now supplemented by Eugene Kist's article in *Quaker History*, 60 [1971]: 88-119) had told me repeatedly of his extensive contacts with Brüning. We have a record of them in Sollmann's papers, now deposited in the Swarthmore College Peace Collection. They confirm that Brüning made serious, if unsuccessful efforts to save Socialist leaders like the former

minister Rudolf Hilferding from falling into the hands of the Nazis. Brüning refers repeatedly to wartime visits paid to him by a prominent emissary of the State Department and the OSS. He made it amply clear in his letters that he did not believe that refugees like himself could return to office after the war. In 1944, however, he suggested for an Allied occupation of Germany the formation of a small advisory committee of emigrants who had become citizens of Allied or neutral countries. Among them he named Sollmann, Friedrich Stampfer, Max Brauer, and Gerhart Seger, four moderate Social Democrats. Also a few Democrats should be included, but he reiterated that he could not belong to it himself, since he was not a U.S. citizen. He described his own frustrations during the American years to Sollmann in a letter of December 16, 1946: "In spite of my avoidance of Washington after my experience in 1939 with President Roosevelt and the intrigue in the summer of 1944 to prevent very influential people from discussing the Morgenthau proposals with me before the Quebec conference, people are alarmed even now by the possibility of my returning into politics." A few years later Brüning went back to Germany, but most of his countrymen cared but little for his advice, and he ended his days, a discouraged old man, in a little village in Vermont.

FELIX E. HIRSCH

Trenton State College

#### TO THE EDITOR:

I have only just seen the review by Robin Higham of *Documents Relating to the Naval Air Service*, volume 1 (*AHR*, 76 [1971]: 508-09), which I edited for the Navy Records Society's series. I welcome constructive criticism of my published work from any well-informed person. But I take strong exception to Mr. Higham's remarks, which are in some cases quite inaccurate and in others totally unjustified. An author or an editor of original papers is entitled to expect reasonable care and knowledge on the part of a reviewer and that he should have read the work he is discussing. Mr. Higham obviously has not fulfilled those expectations.

As to the date of the formation of the Royal Naval Air Service Mr. Higham has evidently repeated a mistake made by several writers. A



Royal Warrant is not an executive order, as he seems to suppose. It is merely an authorization. To quote the definition in the *Oxford English Dictionary*, a warrant is "a document conveying authority or security. A writing issued by the Sovereign, an officer of state or an administrative body authorizing those to whom it is addressed to perform some Act." The Admiralty, which was the body concerned with the Royal Warrant of July 1, 1914, did not act on it until thirteen months later. Indeed on July 1, 1914, the Admiralty issued a statement referring to the RNAS as "forming the Naval Wing of the Royal Flying Corps" (see doc. 47 in my book). Obviously then it was not a separate service on the date in question. On July 29, 1915, an Admiralty Weekly Order was issued stating that "the Royal Naval Air Service is to be regarded in all respects as part of the Royal Navy. . . . *These instructions will come into effect on 1st August 1915*" (italics supplied; see doc. 72 in my book). Hence the effective date of the execution of the Royal Warrant and the establishment of the RNAS is clearly established.

With regard to Rear-Admiral Murray Sueter, as I described him as "brilliantly inventive" (p. xi), which is almost verbatim what Mr. Higham says, and I also quoted Sir Walter Raleigh's tribute to him (p. 57), it is nonsense to suggest that I treated him unfairly. With regard to what Mr. Higham calls his "demise" (presumably meaning his replacement at the Admiralty in 1917), scholars will surely prefer the documents I published (docs. 150, 151) to what Sueter told Mr. Higham in 1959 when Sueter was eighty-eight years of age and within a year of his death. Nor are my remarks on Sueter's conduct undocumented, as Mr. Higham states. Documents 48 and 81, among others, are highly relevant.

For Mr. Higham to pick on my footnote about Pemberton Billing, a quite insignificant character of the period, as inadequate when I gave Billing six lines of small print is petty to the point of absurdity. Mr. Higham may like more and longer footnotes, but the NRS depends entirely on voluntary subscriptions and on its editors' giving their services free. It simply cannot afford the printing costs of long and copious footnotes. That is why I put nearly all my plentiful editorial comments and explanations in the text and indicated them by heavy

square brackets—an aspect of this book that Mr. Higham ungenerously but totally ignores.

As to books on the period, I cannot see the relevance of Mr. Higham's remark, unless it is inspired by pique at my failure to mention his work on British airships. If he had troubled to read the inscription on the flyleaf he would see that the NRS is described as existing "for the purpose of printing rare or unpublished works for naval interest"—not to quote from published books.

I will pass over Mr. Higham's concluding remark about my knowledge of the period by merely describing it as a patronizing and gratuitous insult, which should not be made by a reviewer even of a trivial work. Though I would be the last to claim complete knowledge of this or of any other period I very much doubt whether Mr. Higham can teach me much about my subject—as he evidently believes he can.

I hope that the *AHR* will find a better-informed and more fair-minded reviewer for the second volume of the work in question.

S. W. ROSKILL

*Churchill College, Cambridge*

#### PROFESSOR HIGHAM REPLIES:

Let me respond to Captain Roskill's criticisms of my review by first denying his assumptions that I am not qualified to review the work and that I did not read it.

Surely when an editor discovers important evidence, in this case that the establishment of the Royal Naval Air Service was not on the date commonly accepted by the official historians of the First World War, by naval historians, and by myself in *The British Rigid Airship* (1961), he has some duty to point this out in his introduction rather than making a bald statement and then providing the explanation in a letter to the editor of a journal in which he is faulted by a reviewer.

On the question of Rear-Admiral Sir Murray Sueter's demise at the Admiralty, I would simply respond that in cases of this sort the written record seldom tells the whole story. While it is quite true that at the time I interviewed Sir Murray he was advanced in age, his memory was quite accurate, a fact I could judge by having worked through the documentation then available, which unfortunately excluded

much of the Admiralty materials now revealed, before I saw him.

Captain Roskill should well recognize that reviewers have very limited wordage assigned. I drew attention to the footnote on Pemberton Billing simply as an example of the problem, feeling it better to be specific rather than make a sweeping charge. It is unfortunate that the Navy Records Society cannot afford better notes. One can sympathize with them on this. My basic point, however, remains that as the period recedes into the past the reader needs better identification not only of people, but also of expressions, items, and equipment. As to Captain Roskill's brackets in the text, I have rechecked, as a sample, pages 320 to 420 and find these to be simply routine linkages or explanations of the sort editors normally supply. In

this respect the notes seemed not to merit special mention in a short review, though undoubtedly they help provide continuity.

Finally, Captain Roskill obviously does not understand that bibliographies should, if they are to be of any use, guide the interested person to the relevant material on the subject. In this sense they reflect within limitations an author's or editor's knowledge of the subject. I certainly did not expect him to quote from my book, which is a monograph (devoting most of the first 229 pages to the naval airship program) and not a collection of documents. For my part, I am perfectly aware of the work of the NRS and have supported it with a standing order from our library.

ROBIN HIGHAM  
*Kansas State University*

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## Recent Deaths

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On September 19, 1971, WILLIAM FOXWELL ALBRIGHT, one of the great scholars of this century, died in Baltimore, Maryland. A giant in the field of Near Eastern and Biblical studies, his productivity was immense: in the six decades following the publication of his first article in 1911, the total number of his books, articles, and reviews exceeded one thousand. Universally acclaimed for his contributions to knowledge, he was the recipient of countless honors from academic institutions and societies, both foreign and domestic.

W. F. Albright, whose parents were Christian missionaries, was born in Coquimbo, Chile, on May 24, 1891. Educated in the United States, he received his B.A. from Upper Iowa University at Fayette in 1912 and his Ph.D. from Johns Hopkins University in 1916. The year 1919 found him in Jerusalem, a fellow of the American School of Oriental Research; by 1920 he was acting director of the school and subsequently its director (1921-29). Professor of Semitic languages at Johns Hopkins from 1929 until his retirement in 1958, his association with the American School in Jerusalem continued, and he served a second term as director (1933-36). Albright remained active in teaching, research, and lecturing after retirement until struck down by the massive stroke in July 1971 that carried him off two months later.

An archeologist, a historian, a paleographer, an epigrapher, and a profound student of ancient languages, Professor Albright was no narrow specialist; it should also be emphasized that he was the master rather than the jack of all these trades. His publication of the results of the excavations at Tell Beit Mirsim (3 vols., 1932-43) established the fundamental chronol-

ogy for Palestinian archeology. A more popular treatment of the subject, which became for many students their first introduction to the field, was his *Archaeology of Palestine*, which appeared as a Penguin paperback in 1949. Among his other well-known works one might cite *Archaeology and the Religion of Israel* (1942) or *Yahweh and the Gods of Canaan* (1968), but the true measure of the man as historian and synthesist must be his magnificent, stimulating *From the Stone Age to Christianity* (1940).

A great teacher, Professor Albright had scores of students, many of whom attained scholarly distinction of their own; certainly not the least of these was the late Nelson Glueck who began his study of archeology with Albright in Palestine in 1928. Not only those formally enrolled as his students but also many young scholars who came in contact with Professor Albright were the beneficiaries of his generous advice and encouragement. His kindness and consideration will never be forgotten.

It is good to know that the attainments of this great man were fully recognized before his death: a volume of essays in his honor signaled his retirement and another volume his eightieth birthday; the American School in Jerusalem was renamed the Albright Institute of Archeological Research. Since his death there have been numerous memorials: the most moving of these was conducted at St. George's Cathedral in Jerusalem on September 23, and the testimonials are printed in the *American Schools of Oriental Research Newsletter* (1971-72).

William Foxwell Albright was more than an Orientalist or an ornament to Biblical studies. He was a man to admire; genuine, honest, and

fine. He wished to be known as a Christian humanist, and he was truly that.

TOM B. JONES

*University of Minnesota,  
Minneapolis*

PHILIP E. MOSELY, Adlai E. Stevenson Professor of International Relations, director of the European Institute, and associate dean of the School of International Affairs at Columbia University, died after a long illness at his home in New York City on January 13, 1972.

Professor Mosely was born September 21, 1905, in Westfield, Massachusetts. He received his B.A. from Harvard University in 1926 and his Ph.D. in 1933, benefiting from the guidance of men such as Archibald Cary Coolidge and William L. Langer in diplomatic history and Michael Karpovich in Russian history. He spent from 1930 to 1932 in the Soviet Union, beginning then the acquisition of his unsurpassed knowledge of the Soviet land and peoples. The opportunity to spend 1935-36 in the Balkans and other summer travels throughout Eastern Europe helped give him the extraordinary fluency in Romanian, Russian, and Bulgarian, which, together with equal mastery of the West European languages, helped to increase his understanding and his ability to communicate.

Professor Mosely's formal academic appointments were all with Eastern institutions: Princeton, Union College, Cornell, Columbia, and the Council on Foreign Relations. From 1942 to 1946 he occupied important positions in the Department of State, serving as adviser to Secretaries of State Cordell Hull and James Byrnes. His participation in the World War II settlements, notably those at Moscow and Potsdam, increased his insight and interest in Soviet foreign policy and Soviet-American relations. It also drew him from his earlier interest in nineteenth-century diplomatic history and Balkan social history into a prominent role in helping to shape American foreign policy, because he remained an important adviser to the Department of State after 1946. Thus the exciting "open skies" proposal of President Eisenhower was his suggestion. The relationships Professor Mosely maintained with Soviet and East European scholars and diplomats were sustained throughout the crises of the past quarter century. This unusual feat reflected the respect in

which they held his knowledge, understanding, judgment, candor, and integrity. He would have served with distinction as our ambassador in Moscow or in any of the East European capitals if he had not committed himself to academic work after 1946.

As a scholar Professor Mosely was remarkably prolific. His works reflect high quality in spite of their variety and his numerous activities. His first book, *Russian Diplomacy and the Opening of the Eastern Question in 1838 and 1839* (1934), was a model of diplomatic history. His translation and editing of Victor Chernov's *The Great Russian Revolution* (1936) made available a significant interpretation of the Russian Revolution that might otherwise have been ignored. Professor Mosely excelled, however, as an author of articles, publishing more than two hundred of originality and insight on subjects that reflected his wide interests and vast learning, articles ranging from Bulgarian village life in the nineteenth century to the history of Slavic studies. In the last year of his life he planned to return to study of the *zadruga* or communal joint-family in the Balkans, an interest of his since the early 1930s, and to transform a half dozen articles into the first full study of this central but little-known social institution of Southeast Europe.

Professor Mosely was not an outstanding lecturer but he will be remembered as a great teacher. His graduate seminars were entirely dedicated to passing on the skills of the craft. His work on dissertations, those which he directed and those which were the basic responsibility of colleagues, was prompt and extraordinarily careful. Indeed, he devoted the spring of 1971, his first sabbatical and one taken to help him recover from several painful operations, to his usual careful analysis of theses. Mosely also made counseling his own students and others an art. He always found time and energy to give advice and help, when asked, to students and colleagues who needed technical assistance, advice concerning fellowships or positions, or simple solace. His concern for all students, his almost intuitive understanding of them, and his devotion to the academic community all help explain the fruitful role he was able to play when Columbia was torn by dissension and troubles in 1968.

Professor Mosely was also one of the first to

recognize that teaching is not limited to the classroom. He was very active with businessmen's groups interested in international politics. He felt an especial obligation to meet foreign scholars of all political persuasions in international conferences, and he was eager to consult with scholars from other countries, especially in Eastern and Western Europe and in Asia, concerning means of improving higher education and international understanding.

Professor Mosely's greatest contribution, however, was in expanding and improving research and instruction concerning Russia and Eastern Europe and in promoting multidisciplinary study. He was one of the original members of the famed Joint Committee on Slavic Studies, established by the Social Science Research Council and the American Council of Learned Societies in 1937, which has served as the principal planning and coordinating body for American study of Russia and Eastern Europe. This group not only brought major scholars together in a cooperative program, but it created a spirit of joint endeavor that helped the Russian field to spread easily into new centers of study and to avoid the shattering conflicts that divided Chinese studies so bitterly in the 1950s.

Professor Mosely was one of the founders of Columbia's Russian Institute, which since 1946 has been the model for training scholar-teachers in all non-Western areas. He served as director of the institute from 1951 to 1955, when he left Columbia for eight years to become director of studies at the Council on Foreign Relations, where his work at a more advanced level helped to stimulate research and the publication of a series of volumes on China and Western Europe.

His services extended far beyond the usual confines of even a great university. Impressed by our need for more information and by the plight of refugees from the Soviet Union and Eastern Europe, he helped to organize and administer the Research Program on the USSR and the East European Fund, which from 1951 through 1961 and 1952 through 1961, respectively, enabled hundreds of refugee scholars to continue their studies and to make the transfer to American academic life. He helped establish the Chekhov Publishing Company, which published the works of refugee scholars as well as Russian classics then unavailable in Soviet editions. Professor Mosely somehow found time to

establish a Russian archive at Columbia University, which has already become an important repository for manuscripts and memoirs, both written and recorded on tape. He served as a consultant for both the Ford and the Rockefeller Foundations and on the boards of trustees of organizations such as the Foreign Policy Corporation and the RAND Corporation.

In short, Professor Mosely enjoyed a full, successful, and satisfying life of service. He was acknowledged for his labors by honorary degrees from the University of Notre Dame, Union College, and Middlebury College. Above all, he won the respect and affection of hundreds and even thousands of American and foreign scholars for the immense learning and understanding he carried so lightly, for his keen, analytical intelligence, for his warm interest and compassion, and for his absolute integrity and decency.

ROBERT F. BYRNES  
*Indiana University*

WILLIAM L. NEUMANN, professor of history and chairman of the American Studies program at Goucher College, died on September 30, 1971, at the age of fifty-six.

Born in Buffalo, New York, he earned a bachelor's degree from the State Teachers College in Buffalo and then a master's degree and a doctorate from the University of Michigan. A pacifist and conscientious objector during World War II, he served in lieu of military service as a forest ranger in California and Massachusetts, as a lumberman in northern Michigan and Oregon, and then for a year as a human guinea pig at the University of Pennsylvania Medical School. At the end of the war he joined the faculty of the University of Hawaii, leaving there in 1949. That year he became executive secretary of the Foundation for Foreign Affairs and editor of *American Perspective*, a quarterly journal of foreign affairs. He first came to Goucher in 1952 while serving as staff consultant on foreign affairs for the U.S. Senate Republican Policy Committee. By 1958 he was a full professor at Goucher. He also taught at Howard University, the University of Virginia, the University of Maryland, the University of Wisconsin, Temple University, and Morgan State College.

His distaste for war carried over into his scholarly work. A revisionist historian in the

tradition of Charles Beard, he was a constant critic of U.S. foreign policy. His role as critic took him into several fields of endeavor: speaking before various organizations, radio and television appearances, and membership in organizations such as the Society for a Sane Nuclear Policy, the Conference on Peace Research, the Society for Historians of American Foreign Policy, the American Civil Liberties Union, the American Historical Association, and the American Association of University Professors.

Author of many scholarly articles he also wrote two books: *America Encounters Japan: From Perry to MacArthur* (1963) and *After Victory: Churchill, Roosevelt, Stalin and the Making of the Peace* (1967). A popular and dynamic teacher, his sudden death saddened a host of friends.

GEORGE A. FOOTE  
Goucher College

ALLAN NEVINS, former president of the American Historical Association, died on March 5, 1971, after a varied and distinguished career of some sixty years in journalism, teaching, writing, scholarship, and academic and public service. Born May 20, 1890, on a farm outside Camp Point, Illinois, of Scottish and German forebears, Allan Nevins learned early the cardinal virtues of duty, industry, thrift, perseverance, and probity—qualities he displayed in exemplary form the rest of his life. In 1908 he entered the University of Illinois where he studied English literature and edited the college paper. He stayed on for a year after graduation to study under the brilliant Stuart Sherman and to write the first of his many books, a biography of Robert Rogers. Professor Sherman recommended the young literary critic to friends in New York City; in 1914 Nevins moved to that city, which remained thereafter his spiritual and intellectual home, and took a job as editorial writer on the *Evening Post*—whose history he promptly wrote. He was now fairly launched on that editorial career which was to command his lifelong allegiance, serving successively as editor on the *Post*, literary editor on the *New York Sun*, and editorial writer under Walter Lippmann on the *Morning World*. Nevins's abiding interest in journalism was attested by his history of the *Post*, a collection of *American Press Opinion from Washington*

to Coolidge (1928), several volumes of collected editorials by Walter Lippmann, and contributions to the *Dictionary of American Biography* on newspapermen and journalists, which together constitute a compendious volume. Somehow amid the exacting demands of editorial work Nevins found time to produce, in 1924, a history of the *American States during and after the American Revolution*, which was awarded the first of many prizes he was to receive in his long literary life and which remained, for almost half a century, a standard and almost a classic work in its field. Next year came the first of several versions of a biography of John C. Frémont, and in 1927 he wrote a volume in the new History of Social Life series, *The Emergence of Modern America*, which foreshadowed a lifelong interest in the Reconstruction era of American history.

Now fully—though not exclusively—committed to a life of scholarship, editor Nevins became, in 1927, Professor Nevins of Cornell University. Within one year, however, he had returned to New York City to join the faculty of Columbia University, an institution to which he was married and faithful for the rest of his long life.

Professor Nevins was now fairly launched on what proved to be the most productive scholarly and teaching career of any American historian, for none other of this century produced so many major books and so many major students and disciples as this unassuming scholar who was himself innocent of any academic study of history and who so conspicuously lacked the imprimatur of the Ph.D. Now that he could devote full time to scholarship, Nevins turned his cascading energies to the rewriting of much of American history; what was perhaps most remarkable is that he managed to write equally well for a scholarly and a popular audience; his scholarly works were written in vigorous graceful prose and his popular articles, essays, and reviews with a scrupulous regard to scholarly standards. He did not confine himself to any one specialty or area of history but was equally at home in—and productive in—biography and political, diplomatic, military, economic, social, and cultural history. His study of Frémont was followed by major biographies of Henry White (1930), Grover Cleveland (1932), Abram Hewitt (1933), Hamilton Fish (1936),

and Herbert Lehman (1963). Three biographical studies proclaimed Mr. Nevins's abiding interest in the history of business—a two-volume biography of John D. Rockefeller (1940), which was in effect a history of the Standard Oil Company and the Rockefeller Foundation; an essay (written with Jeanette Mirsky) on *The World of Eli Whitney* (1952); and three volumes (written with Frank E. Hill) on *Henry Ford: the Time, the Man, the Company* (1954–63), a full-scale history of the Ford Motor Company and even of the automobile industry in America. Two volumes in the new Yale Chronicles of America series—in addition to innumerable articles—attested a lifelong concern with foreign policy. *The Gateway to History* (1938), designed to invite amateurs as well as to instruct professionals, remains perhaps the most luminous introduction to the study of historiography; and a small study, *State Universities and Democracy* (1962), managed to say something original about American higher education.

In 1945 Professor Nevins decided to concentrate his major energies—for he could never resist the temptation of forays into other interesting fields—on a rewriting of James Ford Rhodes's history of the United States in the Civil War era. To this work, which he planned to span a period of roughly thirty years and which he expected would require twelve volumes, he gave the felicitous name *The Ordeal of the Union* (1947–71). He lived to complete eight of the volumes, carrying the story from the close of the Mexican War to Appomattox. *The Ordeal of the Union* provided a bridge from the old to the new history. Narrative in form, based on exhaustive research in newspapers, manuscript, and archival materials, rich in original interpretations, and presented in a style always vigorous and lucid and often eloquent, *The Ordeal of the Union* was closer to the great narrative histories of the nineteenth century—those by Parkman, Henry Adams, and Rhodes—or to those English models Mr. Nevins so admired—those by Macaulay, Lecky, Churchill, and the two Trevelyan—than it was to the new technical history that (with his encouragement) many of his own students were already writing. Allan Nevins was in many ways a stout traditionalist, but he was also—witness his sponsorship of oral history—an innovator, and one of his last articles celebrated the new techniques

- which were even then transforming much of

the conventional history that he had written.

Not content with this prodigious output of original work, Professor Nevins undertook editorial activities sufficient to occupy the full time of most scholars. While still editor on the *World* he wrote a minor classic of social history—*American Social History as Recorded by British Travelers* (1923). In 1927 he began to make available some of the more famous diaries of American history: that of the New York City social leader, Philip Hone, in two volumes; those of Presidents Polk and John Quincy Adams in single volumes; two volumes of the journals of Toledo's Brand Whitlock; and, most valuable of all, the massive four-volume diary of George Templeton Strong. Alongside these stand a dozen or so other collections of letters and public papers—the previously mentioned volumes of Walter Lippmann's editorials, the letters of Grover Cleveland and Abram Hewitt, and a selection from the public papers of John F. Kennedy. And during the whole of Nevins's academic life he exercised close editorial supervision over several major series: the American Political Leaders series, the new Chronicles of America series, the Nations of the Modern World series, Heath's College and University History series, and, in the last decade of his life, the fifteen-volume Civil War Centennial series.

This most industrious of scholars was also the most dedicated of teachers. For thirty years he lectured to large classes of graduate students and, in his overcrowded seminars, guided literally scores of others through dissertations and into scholarly careers—dissertations which he supervised and edited with meticulous care, careers which he encouraged with ceaseless benevolence, for his relationship with his graduate students was always *in loco parentis*. His teaching was not confined to Columbia University. He lectured widely, taught for one year in the universities of Australia, and twice held the Harmsworth Chair of American History at Oxford University, where he managed to penetrate the formidable barriers of traditionalism and introduce some long-needed reforms.

Professor Nevins had a third career, which might be characterized—in no pejorative sense—as entrepreneurial. Ceaselessly concerned for the reputation and well-being of Clio, he turned much of his energy to celebrating her virtues, protecting her from those he thought her enemies, and advancing her cause. There was

a Napoleonic quality about Nevins's combination of grand strategy and tactics in these undertakings. We can recall him looking out from his eyrie on the sixth floor of Fayerweather Hall—that vast book-lined and paper-strewn room where the very air vibrated with his energetic presence—looking out not merely over the campus of Columbia University but over the whole broad realm of history and planning forays and excursions to assure its prosperity and its triumph. Thus he launched a campaign to build up the history collections of the University library and brought it to a fortunate conclusion by obtaining for the University the munificent Frederic Bancroft Fund. Thus he campaigned to introduce more American history into the high schools, and he succeeded in influencing legislation everywhere in the nation. Thus he championed the potentialities and the dignity of business history; in his own voluminous writing and those of his associates and students and in the creation of business archives, he did much to give respectability to that heretofore neglected enterprise. Thus he worked long to establish a popular journal that would present American history as *Harper's*, the *Century*, and *Scribner's* of the late Victorian era had done; though he had little help here from his professional colleagues, he succeeded in founding the Society of American Historians and launching the now widely read *American Heritage*, whose counselor he remained to the end of his life.

Doubtless most important of all was Nevins's role in creating—or reviving—modernizing, systematizing, and institutionalizing oral history. Oral history itself was as old as Homer and the Icelandic sagas; what Professor Nevins envisioned was a systematic and professional exploitation of personal recollections of men and women who had played public roles. What would we not give, he used to ask, for Washington's account of his services in the Revolution, for Lincoln's detailed observations on his presidency. It was an argument that persuaded President Truman, and scores of others, to provide thousands of pages of oral history to eager tape recorders. The Bancroft Fund enabled Mr. Nevins to inaugurate this project at Columbia University; soon he had worked out an effective technique and trained a corps of younger scholars in that technique; soon the oral history project developed into a formidable and in-

valuable archive. The idea spread from academy to academy, to private associations like the medical associations and the Red Cross, to government bureaus, and to corporation offices until within two decades oral history became a new dimension of historical research at home and abroad.

In 1958 Mr. Nevins retired, formally, from Columbia University and took the post of senior research associate at the Huntington Library. There he devoted his still abounding energies to building up the library and manuscript collections of that institution, helping to make it pre-eminent in many fields of history and literature, and to his own writing. He was, by long training, a bookman and a book collector; his private library, much of which he disposed of in his own lifetime, numbered well over twenty-five thousand volumes.

Allan Nevins had a distinguished public as well as scholarly and academic career. During the Great War he served as cultural attaché to the United States Embassy in London, and later was a kind of cultural ambassador to Australia. He was an adviser to presidents and statesmen—General Eisenhower, Cordell Hull, Herbert Lehman, John G. Winant, Adlai Stevenson, and John F. Kennedy among them. He rescued the Civil War Centennial Commission from the hands of antiquarians and party hacks. He served, at various times, as president of the Society of American Historians, president of the American Academy of Arts and Letters, fellow of the New-York Historical Society, and historical adviser to the Sixth Fleet. Nor should we fail to recall that shortly after his retirement from Columbia University he gave that institution, out of his literary earnings, half a million dollars to endow a chair of American economic history—a chair that now bears his name.

In all this Allan Nevins sounds like an institution and, indeed, had he not been so ebullient and so dynamic, observers might have mistaken him for that. No mere formal account of his career does justice to his affluent personality—to that enthusiasm for learning so contagious that few could resist its importunities; to that single-minded practicality which had no time for academic dalliance but concentrated on getting results; to that tireless tenacity which overcame obstacles of nature and of human nature and brought his own enterprises and those of his students to fulfillment;



to that generosity, moral and intellectual even more than material, which left no room for envy or malice (in almost forty years of intimacy I never heard Allan make a malicious remark about a fellow scholar); to that homespun simplicity and unpretentiousness which permitted him to take on whatever tasks came to hand that he thought worth doing, to accept old and young with equal fellowship, to extend help to amateurs as readily as to fellow scholars; to a capacity for friendship and affection which brought him the devotion of friends in almost every segment of society—academic, journalistic, political, business, military, and merely neighborly. Somehow Nevins found time, too, for an immense correspondence, not only with fellow scholars and graduate students, but with the exalted world of statesmen, with the fluctuating world of business, with editors, novelists, poets, social workers, and old friends. A collection of some fifty thousand of these letters now awaits the student of American cultural history in the archives of his beloved university.

The pattern of Allan Nevins's private, social, professional, and public life was ever harmonious. That same harmony will be found in the historical monument he left behind him—a monument that will, for many years, cast its long shadow across the historical landscape which he surveyed, explored, cultivated, and embellished with such devotion and passion.

HENRY STEELE COMMAGER  
Amherst College

WALTER M. SIMON died unexpectedly at his home in Ashley, Staffordshire, early in June of last year. Professor of history and head of the department at Keele University, he was also prominent in the affairs of that university, serving on many committees as well as being one of three regular advisers of the vice-chancellor.

Although born in Germany, on May 29, 1922, Walter Simon spent his formative years in England. He attended Repton School where, along with a sound education, he acquired his enthusiasm for cricket. His undergraduate training was at Wesleyan University, where he received the B.A. in 1943. From then until 1946 he served in the United States Army. Following the war he studied at Yale under the late Professor

Hajo Holborn; he received his M.A. in 1948 and his Ph.D. in 1949. His first teaching assignment was at Stanford University, where he remained until he moved to Cornell in 1953. In 1965 he accepted the position at Keele.

Although Simon's original field of specialization was modern Germany, his interest early began to shift to intellectual history, particularly to Auguste Comte and the Positivists. His dissertation was published by the Cornell University Press in 1955 under the title *The Failure of the Prussian Reform Movement*, and was followed by articles that eventually were incorporated in his *European Positivism in the Nineteenth Century* (1963). By the time he moved to Keele he seemed to have hit his stride, with volumes appearing every other year: *Germany: A Brief History* (1966), *Germany in the Age of Bismarck* (1968), and *French Liberalism 1789-1848*, which was in the press at the time of his death and is being published by Wiley. In addition, he was at work on a major book on the Enlightenment, had made several contributions to the forthcoming *Dictionary of the History of Ideas*, and was hoping to return to his first love, the French Revolution.

No one who knew him could doubt Walter Simon's dedication to scholarship and teaching, although some, including not a few students, found his standards idiosyncratic as well as exceptionally high. His concept of discipline, intellectual and personal, was exacting, but students willing to work with him on his own terms clearly profited from his influence. As a colleague, in the United States, he was correct, courteous, and reserved—qualities that earned more respect than close friendship, though there were striking exceptions, notably his association with the late Theodor Mommsen.

To many who had known him in the United States he seemed more in his proper element after his return to England where his work flourished and his qualities were at once recognized and appreciated.

E. W. FOX  
Cornell University

Other members of the association who have died recently include John Askling of Malaga, New Jersey, and Herbert C. Cohen of C. W. Post College, Greenvale, New York.

## Other Books Received

Books listed were received by the AHR between December 1, 1971, and February 1, 1972. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

APTER, DAVID E., and ANDRAIN, CHARLES F. (eds.). *Contemporary Analytical Theory*. Englewood Cliffs, N. J.: Prentice-Hall. 1972. Pp. x, 688. \$12.50.

BAIROCH, PAUL. *Le Tiers-Monde dans l'impasse: Le démarrage économique du XVIII<sup>e</sup> au XX<sup>e</sup> siècle*. Collection Idées. [Paris:] Gallimard. 1971. Pp. 372.

BAKKE, E. WIGHT, and BAKKE, MARY S. *Campus Challenge: Student Activism in Perspective*. [Hamden, Conn.:] Archon Books. 1971. Pp. x, 573. \$14.50.

BARON, SALO WITTMAYER. *Steeled by Adversity: Essays and Addresses on American Jewish Life*. Ed. by JEANNETTE MEISEL BARON. Philadelphia: Jewish Publication Society of America. 1971. Pp. xi, 729. \$9.00.

BECKFORD, GEORGE L. *Persistent Poverty: Underdevelopment in Plantation Economies of the Third World*. New York: Oxford University Press. 1972. Pp. xxvii, 303. Cloth \$7.50, paper \$2.95.

*Bibliographie internationale de l'Humanisme et de la Renaissance*. Vol. 5, *Travaux parus en 1969*. Fédération internationale des Sociétés et Instituts pour l'étude de la Renaissance. Ouvrage publié sur la recommandation du Conseil International de la Philosophie et des Sciences Humaines, avec le concours du C.N.R.S. et de l'U.N.E.S.C.O. Geneva: Librairie Droz. 1971. Pp. x, 614.

CHERNIAVSKY, MICHAEL, et al. *Social Textures of Western Civilization: The Lower Depths*. In 2 vols. Waltham, Mass.: Xerox College Publishing. 1972. Pp. xvii, 411; xv, 302.

COLLIER, BOYD. *Measurement and Environmental Deterioration*. Research Monograph No. 34. Austin: Bureau of Business Research, University of Texas. 1971. Pp. xi, 100. \$3.00.

CORVISIER, ANDRÉ. *Précis d'histoire moderne*. Paris: Presses Universitaires de France. 1971. Pp. 477. 59 fr.

DECONDE, ALEXANDER (ed.). *Student Activism: Town and Gown in Historical Perspective*. New York:

Charles Scribner's Sons. 1971. Pp. viii, 342. Cloth \$10.00, paper \$3.65.

DIESING, PAUL. *Patterns of Discovery in the Social Sciences*. Observations. Chicago: Aldine-Atherton. 1971. Pp. x, 350. \$11.75.

DOLGOFF, SAM (ed., tr. and with an introd.). *Bakunin on Anarchy: Selected Works by the Activist-Founder of World Anarchism*. Preface by PAUL AVRICH. New York: Alfred A. Knopf. 1972. Pp. xxvii, 405, vii. \$10.00.

GATZKE, HANS W. (ed. with an introd.). *European Diplomacy between Two Wars, 1919-1939*. Modern Scholarship on European History. Chicago: Quadrangle Books. 1972. Pp. 277. \$3.45.

GILBERT, ARTHUR N. (ed.). *In Search of a Meaningful Past*. New Perspectives in History. Boston: Houghton Mifflin. 1972. Pp. x, 233. \$3.95.

GLAZIER, KENNETH M., and HOBSON, JAMES R. *International and English-Language Collections: A Survey of Holdings at the Hoover Institution on War, Revolution and Peace*. Hoover Institution Survey of Holdings: 3. Stanford: Hoover Institution, Stanford University. 1971. Pp. iv, 20. \$2.00.

GOLDMAN, EMMA. *Red Emma Speaks: Selected Writings and Speeches*. Comp. and ed. by ALIX KATES SHULMAN. New York: Random House. 1972. Pp. x, 413. Cloth \$10.00, paper \$2.45.

GRANT, CHARLES. *The War Game*. New York: St. Martin's Press. 1971. Pp. 191. \$8.95.

GREER, THOMAS H. *A Brief History of Western Man*. 2d ed.; New York: Harcourt Brace Jovanovich. 1972. Pp. xii, 546.

GUNST, PÉTER (ed.). *Bibliographia Historiae Rerum Rusticarum Internationalis, 1967-1968*. Budapest: Museum Rerum Rusticarum Hungariae. 1971. Pp. 355.

MARQUIS, ROGER. *English-French, French-English Vocabulary of Prehistoric Archaeology: Vocabulaire français-anglais, anglais-français d'archéologie préhistorique*. Foreword by ALEXIS KLIMOV. Montreal: Presses de l'Université du Québec. 1972. Pp. xii, 42, xii, 43. \$5.00.

MICHELS, ROBERT. *Les partis politiques: Essai sur les tendances oligarchiques des démocraties*. Tr. by S.

JANKELEVITCH. Preface by RENÉ RÉMOND. Science. [Paris:] Flammarion. 1971. Pp. 309.

MICHOFF, NICOLAS V. *Contribution à l'histoire du commerce de la Turquie et de la Bulgarie*. Vol. 6, *Auteurs français, allemands et anglais*. Bulgarska Akademiia na Naukite, Tsentralna Biblioteka. Sofia: Izdatelstvo na Bulgarskata Akademiia na Naukite. 1970. Pp. 573. 5.42 Lv.

MOUSNIER, ROLAND. *Le gerarchie sociali dal 1450 ai nostri giorni*. Ed. by ETTORE ROTELLI. Cultura e storia, No. 7. [Milan:] Editrice Vita e Pensiero. 1971. Pp. 1, 165. L. 3,200.

OECD *Economic Outlook*. No. 10. [Washington:] Organisation for Economic Co-operation and Development. 1971. Pp. 99. \$3.80.

OLSON, RICHARD (ed.). *Science as Metaphor: The Historical Role of Scientific Theories in Forming Western Culture*. Belmont, Calif.: Wadsworth Publishing Co. 1971. Pp. ix, 321.

PETERSON, AGNES F. *Western Europe: A Survey of Holdings at the Hoover Institution on War, Revolution and Peace*. Hoover Institution Survey of Holdings: 1. Stanford: Hoover Institution, Stanford University. 1970. Pp. iv, 60. \$3.00.

REDLICH, FRITZ. *Steeped in Two Cultures: A Selection of Essays*. Torchbook Library Ed. New York: Harper and Row. 1971. Pp. xviii, 430. \$9.00.

ROTELLI, ETTORE, and SCHIERA, PIERANGELO (eds.). *Lo Stato moderno*. Vol. 1, *Dal Medioevo all'età moderna*. Bologna: Società Editrice il Mulino. [1971.] Pp. 294. L. 3,000.

SALPER, ROBERTA (ed., with introds.). *Female Liberation: History and Current Politics*. New York: Alfred A. Knopf. 1972. Pp. xi, 246.

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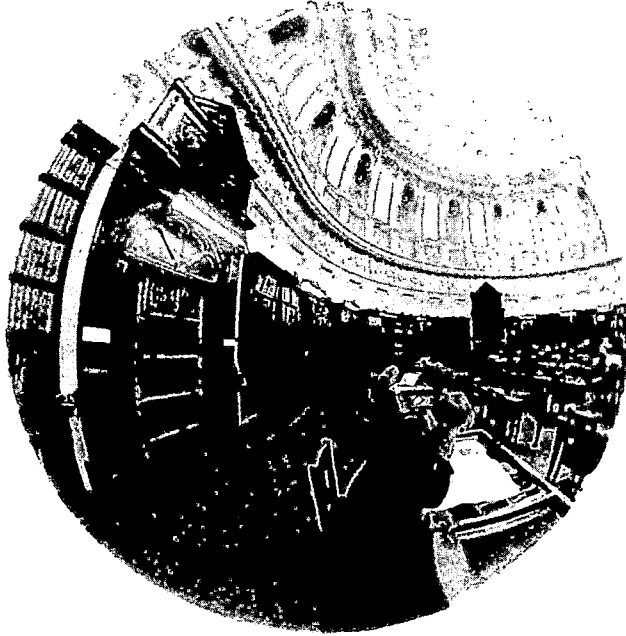
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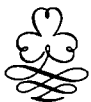
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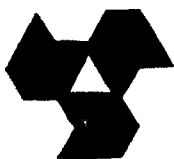
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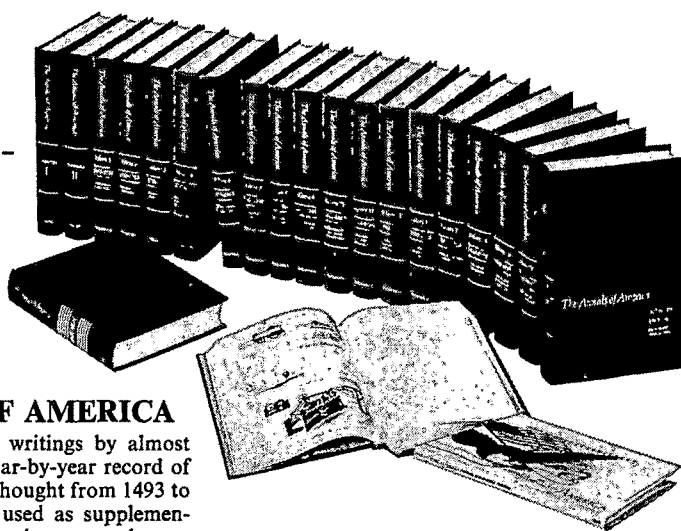
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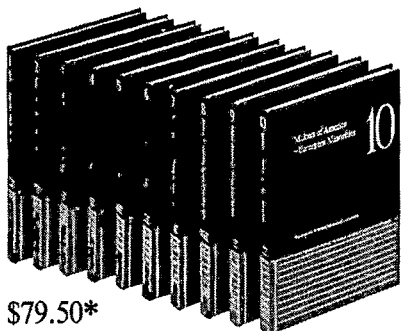
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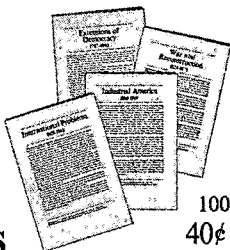
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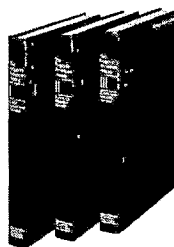
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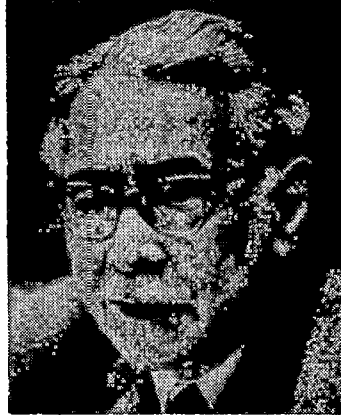


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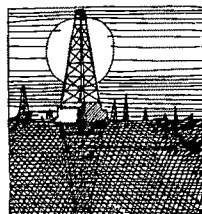
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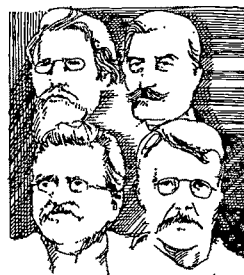
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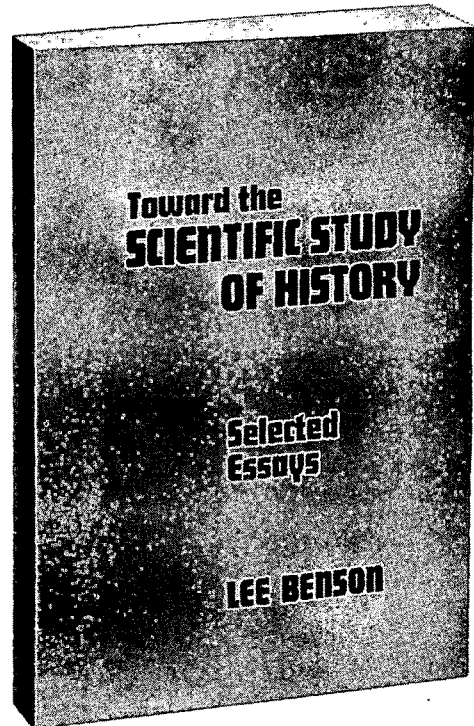
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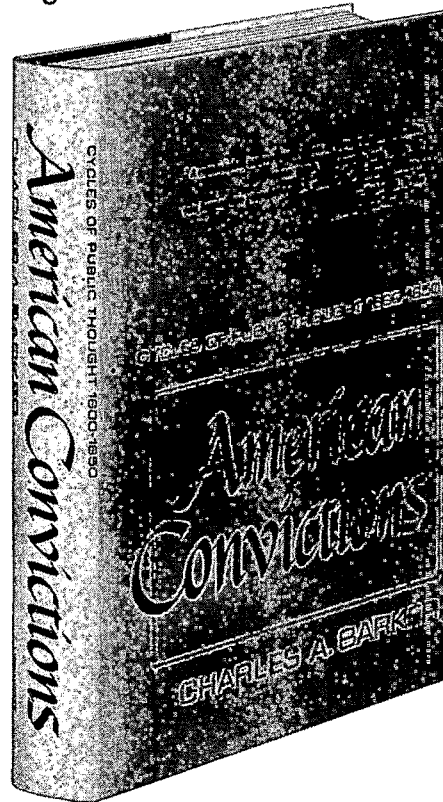
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## The *AHR* and the AHA: An Editorial

---

The financial crisis through which the American Historical Association is passing results in part from the inflationary pinch in which every organization finds itself these days. To that general problem, however, are added two pressures more specific to the AHA: one, the marked increase in activities and services over recent years; the other, the academic recession, which has brought a leveling off in number of members and in anticipated dues income. The cash balance with which the business office must operate is far from adequate, and at times in the past fiscal year meeting payrolls and printing bills has been, as Wellington said of Waterloo, "a damned nice thing—the nearest run thing you ever saw in your life." At least qualitatively, the AHA has shared a situation all too familiar in parts of the university world.

The search for economy goes on constantly at the Washington headquarters these days, but prices continue to rise, and it is clear that cheeseparing alone—even rather brutal cheeseparing—is not going to remedy the situation. Merely gaining reasonable security for ordinary day-to-day operations, let alone a sufficiency to allow for coping with emergencies or expanding into new services, must depend on increasing income. And that means primarily increasing the number of members and subscribers.

The Council is considering means of bettering performance on both these counts. It may be useful in the interval, however, to say something about the role of the *AHR* in the financial affairs of the AHA. The *AHR* is the largest and most expensive single operation within the AHA: the biggest bills the AHA pays are printing bills for the *AHR*, and a little under 40 per cent of the salary allocation in the headquarters staff goes to the *AHR*. But there are less immediately obvious ways in which the *AHR* figures in the financial picture. It is to those interrelations and their implications that this editorial is directed.

### BASIC FINANCE

Although the *AHR* means large outlays for printing and salaries, the *AHR* also produces large amounts of revenue, revenue that would simply not

exist if the *AHR* did not exist. Two sources of this revenue are major items in the AHA balance sheet:

(1) Subscription revenue—that is, money paid by libraries and other institutions for the *AHR*—runs well over \$140,000 per year.

(2) Advertising revenue yields somewhat less, but if a modest anticipated rise in advertising rates does not adversely affect the volume of advertising, anticipated income from this source may begin to approach the income from subscriptions.

In the calculations that follow, only these two clearly definable and measurable items are used as *AHR*-generated income. But additional sources of revenue that arise solely from the *AHR* include:

(1) A relatively small income from sales of back copies.

(2) Income from permissions to reprint articles, and from royalties on the reprinting and microreproduction of the entire journal.

(3) Income from promotional activities. Over the past three years copies of the *AHR* sent to libraries have carried two coupons. At a total cost for printing and insertion of under \$3,000, this campaign has secured more than 1,650 new members, just under two-fifths of them regular members and the remainder student members—a net yield of more than \$16,000.

(4) Finally, an indeterminate amount must be reckoned for those memberships held by persons who join the AHA only because they want to receive the *AHR*. Present constitutional provisions do not allow individuals to subscribe separately.

Two conclusions follow: Subscription and advertising revenue *alone* cover about 70 per cent of the total cost of producing the *AHR* (including distributed overhead), a proportion that would rise somewhat if the other sources of *AHR*-generated income could be taken into account. It follows, using the membership circulation for the issue for June 1972 and including salaries and office overhead in total cost, that, in effect, no more than \$6.50 (and probably somewhat less) of a member's dues pays for the five issues of the *AHR* he receives each year. This sum invites favorable comparison, considering size, coverage, and quality, with any other learned journal to which a member may subscribe.

Some consideration must be given to a large increase in resources made available in 1969. In that year the AHA severed its connection with the Macmillan Company. From 1895 to 1969 the *AHR* was published by Macmillan, under a contract that was singularly generous when the journal was founded and its future uncertain. Macmillan never provided any editorial services under that contract: they were the responsibility of the managing editor and his then very small staff. To this end the Macmillan Company contributed \$2,400 per year, the salary of an assistant editor in 1895. (It might be pointed out, at the risk of a mutiny in the present staff, that that sum was the equivalent in purchasing power of perhaps \$12,000 in 1970.) The services the publisher provided were maintaining the list of subscribers,

but not of AHA members; handling financial and contractual (but not editorial) matters with the printer; and securing the advertising. The Macmillan Company also agreed to absorb any losses the *AHR* might incur. In return the board of editors (and, after the sale of the journal in 1917, the AHA) agreed to pay the publisher a 10 per cent commission on advertising secured and one-third of the net income (if any) of the journal.

These terms, including the payment of \$2,400 for editorial services, were still in effect in 1968. But by that time Macmillan was receiving some \$7,000 in agency fees and some \$30,000 as its share of the net income. When the AHA became its own publisher in 1969 that money became immediately available to the AHA and the *AHR*. The money made possible expansion of the staff and services of the *AHR* and improvement of the salary scale and accompanying fringe benefits for the entire AHA staff. The *AHR* had long been working with an entirely inadequate staff, in terms of numbers, and under routine pressure so great that there was no possibility of expanding and improving the scholarly usefulness of the journal or of finding time for long-range planning: the editing was done, the deadlines were met, and that was all. The salaries were scandalously low—and this in a city where decent academic scales (all that we have aspired to) are inevitably at a disadvantage compared with what government and private agencies pay. Without the expansion of staff that this change permitted, none of the improvements in the *AHR* in the last few years would have been possible; and members of the AHA must feel some relief in knowing that their association no longer presumes upon the self-sacrifice of its staff. The work load is still heavy and deadlines still press, but we are no longer using sweated labor.

One final point must be made about basic finance. Over the three fiscal years from 1969 to 1972, the advertising and subscription revenue from the *AHR* has gone up by roughly 50 per cent. Over the same period the percentage of total AHA revenue ascribable to *these two AHR items alone* has risen from about 27 per cent to 32 per cent. Meanwhile, the total expenditures on the *AHR* (including distributed overhead) have remained virtually constant as a proportion of total AHA expenditure, falling from an even 46 per cent to 45.3 per cent. Taking actual amounts of money, from the fiscal year 1969–70 to the fiscal year 1970–71, total expenditures for the *AHR* rose by roughly \$42,000, while its income from advertising and subscriptions rose by roughly \$45,000—more than covering the increased expenditure, even without taking into account the money made available by the termination of the Macmillan contract and without drawing more heavily than before on dues income. The increase in expenditures on the *AHR* from 1970–71 to 1971–72 was roughly \$25,000, while income from advertising and subscriptions rose by almost half again as much, by some \$36,000. The additional revenues the *AHR* has generated have in good part gone to support expanded AHA operations.

## OPERATING COSTS

Since 1968 the editors of the *AHR* have made a number of changes in editorial policy and practice.

(1) The review articles are probably the most immediately evident innovation, one apparently much welcomed. Review articles have, of course, increased the amount of space devoted to articles.

(2) The bibliographical services have been markedly altered. A bibliographer has been added to the staff of the *AHR* (though a part of his time goes to important AHA activities); he has introduced a number of technical changes while securing far more systematic coverage in the lists of periodical articles. We anticipate that other changes in the near future will make possible yet more efficient and sophisticated services. A future editorial will directly address the question of the obligations and accomplishments of the AHA and the *AHR* in the field of bibliographical control.

(3) Promotional campaigns have been launched to increase memberships and subscriptions, an effort that will grow as we gain time, resources, and experience to allow the careful planning that must underlie such work.

(4) A good many technical reforms have improved the effectiveness of the operations within the *AHR* staff. They include ways of keeping down the printer's charges for authors' alterations; more systematic handling of the extremely complicated matter of copyright and permissions, along with more generous financial provision for the authors of articles involved; rationalization and simplification of copyediting procedures; and improved filing and record keeping.

As the proportional figures above suggest, these changes have been made with a steady eye to cost-effectiveness, as well as to a careful assessment of their scholarly usefulness.

By all odds the most striking change that has been made in the *AHR* is the redesign that took effect in February 1971. While the welcome accorded this new format, within the AHA and outside it, has been overwhelmingly favorable, some members have continued to be plagued by doubts about the wisdom of such a departure at precisely the time the AHA was entering a period of financial stringency. This concern has been anticipated and answered on earlier occasions, both in the *AHR* and in the report of the managing editor for 1971. But, as the question of whether we can afford such luxury persists, a fuller, more prominent explanation is called for.

The problem of controlling printing costs is a very difficult one, inasmuch as, in recent years, operating costs in the printing industry have risen some 5 to 7 per cent annually. At the same time significant technological changes have been taking place. These have offered opportunities to control costs but have in some instances required changing specifications. The production cost of any publication is determined by the cumulative effect of three factors—physical specifications, design esthetics, and quantity of material. We can examine each of these factors as they apply to the *AHR*.

PHYSICAL SPECIFICATIONS: In close consultation with our printer, we have undertaken a number of technological changes.

(1) Beginning with the issue for December 1968 we changed from the traditional book-sewn binding to adhesive binding, at a considerable saving.

(2) We are now using computerized composition to set and store indexes, thus making possible the publication of cumulative indexes, at five-year instead of ten- or twenty-year intervals, without the necessity for resetting.

(3) The lists of periodical articles are being set by computer, allowing some slight saving in cost, greater ease of proofreading, and far greater capacity for control and retrieval and for future flexibility of service.

(4) We have converted from sheetfed letterpress to web offset printing. A number of consequences—and opportunities—have flowed from this basic transformation.

(a) Paper. The paper presently used for the *AHR* is cheaper, though no less permanent, than the basic text stock used in the old format. The web offset process allows us to print illustrations on the same kind of paper used for text matter. Because advertisers have always required the possibility of illustration, the old format and technique required the use of a different, much more expensive coated stock for the advertising section. This use of two kinds of paper in the old *AHR* was not only costly, it was by general agreement ugly and caused more than a few crises in laying out advertising and front matter.

(b) Illustrations. The use of pictures in the text, prohibitively expensive with the old methods, is relatively inexpensive with the new. It costs much less to prepare a page of illustrative material for a web offset press than it does to set a page of type. The printer's charges for preparing illustrations have averaged around fifty to seventy-five dollars for each issue.

(c) Cost. The most expensive single issue ever produced, in terms of printing cost per copy, was that for October 1968, using the old process and format: \$1.25 per copy for 432 pages of editorial text and 92 of advertising. In the report of the managing editor for 1971 it was pointed out that the average per-copy cost of the last three issues printed by letterpress in the old design was just under \$.98, while the average per-copy cost of the first three issues printed by offset in the new design was just over \$.93, although the general level of printing costs rose that year; the amount of material in each set of three issues was, so nearly as it can be calculated, roughly the same. Another comparison follows. Constructed on the basis of groups of five issues running from October through June, this table represents the organization of volumes prior to volume 76 rather than the present organization, which corresponds to the calendar year. But we gain in comparability over a four-year period and perhaps to some extent from the coinciding of this definition of the volume year with the AHA fiscal year.

|                      | <i>Total pages of text<br/>per volume</i> | <i>Average printing<br/>cost per copy</i> |
|----------------------|---|---|
| 1968-69              | 1828                                      | 1.06                                      |
| 1969-70 <sup>a</sup> | 1565                                      | .97                                       |
| 1970-71 <sup>b</sup> | 1634                                      | .95                                       |
| 1971-72              | 1757                                      | 1.04                                      |

<sup>a</sup> Shift to web offset printing with the issue for June 1970.

<sup>b</sup> Shift to new format with the issue for February 1971.

In summary, while printing costs have been rising, we are currently benefiting from an average printing cost per copy that is certainly no more than it was four years ago and is probably less. In terms of dollars, it is equivalent to at least a 20 per cent reduction over the four-year period. And we believe we are producing a better journal. The wisdom of our approach has been reinforced by the fact that we will have no increase in printing price in the coming year. We continue to look aggressively at new technologies and methods in a continual effort to control costs without sacrificing quality.

DESIGN ESTHETICS: There is no question but that the *AHR* can be produced more cheaply. Smaller type sizes could be used, but the journal would be proportionately less readable. We could use still cheaper paper, but it would not look or wear as well, and its shelf life would in all probability be limited. It is in this connection that questions of technology and economics come together with questions of design.

The change in technology dictated redesign at the earliest possible moment, to gain still further economies as well as to take fullest esthetic advantage of the new methods. By way of illustration, we may raise two questions that have been asked about the design.

(1) Are we not wasting paper on bigger pages and wide margins? When the *AHR* was printed in its old size on the web offset press—the issues for June through December 1970—the copies had to be trimmed down; paper was thus wasted as scrap. We now take the fullest advantage of the size of the press. This determines the margins in the length of the page. Some saving in paper could be attained by narrowing the width margins. But to do so would produce a page with a rather queer shape. The present length of the line used in printing articles is the longest the linotype machine will set, and given the type size we are using, it is also probably the longest line that the eye can tolerate readily, in purely psychological terms. If the wider margins in the articles sections are a luxury, it is a very small one and pleasing as well; practically, they make the text easier to read in a book of this size. Material other than articles is now printed in double columns and in smaller type than in the old format; this means a very considerable gain in the amount of print on the page over what we used to manage. Together with the redesign of the headings, particularly in the review section, these changes in format mean a markedly more efficient use of paper.

(2) How can we afford such a luxurious and elegant cover? The relative cheapness of using pictures has already been explained, but the glossy paper seems to stick in some readers' minds as the quintessence of extravagance. They are unaware that our paper cost for the journal itself is significantly lower than was the case in the old format, with savings that are scarcely eroded by the cost of the coated stock used for the covers. A cover for a copy of the *AHR* costs about a penny more than we paid for the old fraying and faded blue cover. The gain in durability is obvious, that in attractiveness should be equally obvious; and together with all our other economies they surely warrant a marginal increase in expenditure on this one item.

To sum up, one must make choices between esthetics and economics. As an editorial staff we have worked hard to understand the economic alternatives, to take the best advice available to us, and to make judgments that are sound both as to cost and as to appearance. Everyone knows that the cheapest product available is not invariably the best buy, and a cheaply produced journal is not going to attract readers, advertisers, or contributors. On this score, the new design seems to have made as well as saved money. Subscriptions have increased; the response to the coupon solicitation rose noticeably with the new design; perhaps most important of all, the level of advertising sold has held remarkably steady at a time when the publishing business has been cutting back on advertising budgets. The new printing methods, moreover, make the preparation of advertising copy easier, more flexible, and more certain; and a comparison of advertising in the new and old formats will quickly reveal how much more attractively designed the advertisements are—a direct consequence of the altered methods and design.

Even more important, the new design has proved more attractive to the *AHR's* audience—and not merely in an esthetic sense. The use of illustration has made it possible to attract a kind of article that would not have come to the *AHR* before, as it has made possible the enrichment of more traditional articles through a valuable but hitherto prohibitively expensive category of evidence. Of the truth of this statement the present issue is a remarkable confirmation. What is more, every evidence that has come into the Washington office has indicated a much higher degree of interest on the part of readers. Since the primary purpose of the *AHR* is scholarly communication, this last should be the only justification needed for the changes we have made, but it is pleasant to know that the better fulfillment of our central purpose could be accomplished with the added advantage of economic gain.

**QUANTITY OF MATERIAL:** It is apparent from the table above that the *AHR*, after dropping back in size in 1969, has crept steadily up again. To some extent this has been deliberate. The present editors determined to try a greater flexibility than was possible before with respect to length of articles



accepted and length of book reviews. There is no doubt, at least in the managing editor's mind, that this decision is completely justified from a scholarly point of view. It has been possible to give fuller attention and argument to important books, while gaining in the use of space by increasing resort to joint reviews. The greater amplitude allowed to authors of articles has been well used. Since the size of a given issue of the *AHR* is committed about a year in advance of its publication, the unusually large issue (by recent measures) for October 1971 was in press before the serious situation presented by the fiscal year 1970-71 was known. Readers will note a slimming down in some current and future issues; strict limitations have been put on the length of communications; fewer review articles will be published than we had hoped; on occasion the number of articles in an issue will drop from four to three or even two, at least partly in an effort to cope with a backlog of reviews that is building up. Some economy will result, but it will not be very great: a few pages here and there do not bring about a financial revolution.

A number of more drastic possibilities have been raised by members of the AHA and have been steadily in the minds of the editors. It is important to consider what such changes, if implemented, might mean.

(1) Dropping back from five issues per year to four. To cut a yearly volume to four issues would deny the AHA some \$20,000-\$25,000 in advertising revenue, almost exactly the cost of printing the issue. To do that would necessitate reducing the subscription rate as well, for subscribers pay only for the *AHR* and cannot be asked, by means of an indirect and ill-concealed rate increase, to subsidize the activities of the AHA. But even if there were no loss of income from subscriptions, the only sure gain from dropping one issue per year would be the saving of perhaps three thousand dollars for wrapping and postage, and much of that saving would undoubtedly be eaten up by increased postage on the remaining four issues, as they would each be considerably larger. Cutting out one issue would mean no saving in staff cost. The present members of the staff—some of whom also carry responsibilities for other publications and activities of the AHA—are stretched to (or beyond) the limit in putting out five issues; putting out four somewhat larger ones would not decrease the need for labor, but would merely redistribute it slightly in the calendar. Indeed it could be argued that the best economic course for the *AHR* would be to keep approximately the same amount of material in a volume and move to six issues per year. But that is not feasible with the present size of staff, and it is uncertain that the additional revenue generated by a sixth issue would cover the needed additions to staff.

(2) Cut the size of the journal drastically, whatever the number of issues. To do that would effect major economies in both staff and printing costs. But it would also completely transform the nature of the journal.

(a) The number of significant broad-scale articles we publish is already

at about the lowest level consistent with maintaining the journal's obligation to cover all fields of history. To impose draconian limits on the size of articles might very well mean losing articles we would otherwise get; hence, little reduction can be counted on in that area.

(b) A likely candidate for the axe would be the bibliographical service. It should be kept in mind, however, that the present lists of articles, approximately 260 pages a year, cost the member about \$.65 of his dues money. Abolishing the operation would save us 10 per cent of our overall costs. But if the bibliographical function were to vanish in an economy drive, nothing would take its place in providing notification of current periodical publication. To be sure, some fields are served, with varying success, by other journals and bibliographies. But what the *AHR* has been doing, for all fields of history, is unique.

(c) On the reviewing side no very great saving could be accomplished by reimposing the strict four-hundred-word limit that used to obtain. Real economy in this area would arise only by moving from a comprehensive to a highly selective reviewing policy—the form of reviewing that is practiced by every other historical journal in this country. Again much would be lost: perhaps half to three-quarters of books of the kind currently reviewed would be passed over and left to specialized journals, who might in their turn pass them over or touch on them only lightly; a good many foreign books that now come only to the *AHR* would simply not be reviewed in this country at all. Without claiming that the reviewing in the *AHR* is incapable of improvement, either in coverage or quality, we insist that in its present form it provides a service that, like the bibliographical operation, is vital to scholarship.

(d) Finally, it must be remembered that there is no other journal like the *AHR*. If, to paraphrase, the *AHR* were to lose all its distinctiveness by losing half its grossness, it might lose a great deal more besides. It would be less attractive to a number of readers who prize it for its comprehensiveness, to librarians who value its service as a record, and—crucially—to advertisers. It is impossible to predict what loss of membership and subscriptions might result, but that there would be loss seems certain. That loss might very well wipe out whatever gains were made by the economies.

#### WHAT IS TO BE DONE?

The *AHR* cannot promise any windfalls to match those stemming from the change in publishers in 1969 or from the new technology and the accompanying redesign in 1970–71. The staff can promise to continue to seek economies in operation and to find ways of increasing the income that the *AHR* itself generates. Certain economies beyond those already mentioned are being effected for the duration of the current crisis:

(1) One of the two meetings of the board of editors has been cancelled, as it was last year, although omitting this meeting necessarily increases staff work and inhibits effective planning.

(2) Funds for staff travel have been sharply reduced, even though attendance at scholarly meetings is an important way of recruiting articles and advertising, an aid to promotion, and the best means of extending the personal acquaintance so important to the effective working of the *AHR*, as a broadly based journal and as the principal publication of the major scholarly association in our discipline.

(3) Some improvements in our internal operations (for example, improved organization of a sadly outdated reviewer file) are sufficiently expensive that they must be stretched out over a longer period than we anticipated two years ago.

(4) Effective this past February, the board of editors decided, with great reluctance, to do away with the modest honorarium the *AHR* had offered (without advertising the fact) to authors of articles. It was only five dollars per printed page, and it helped to cover some typing and postage costs for authors. But it seemed better to save two or three thousand dollars a year there than to accept the alternative of doing away with the minimum quota of reprints we provide to authors of articles.

These small sums may mount up to something useful, but they are not going to solve the AHA's problems.

The present editors and staff of the *AHR* have, we believe, turned in a remarkable record of improved services, greater attractiveness, and genuine economy. With a stable and highly professional staff, a clearly defined purpose, and a commanding position in the profession and in the market place, the *AHR* will continue, so long as its present management is concerned with it, to be a journal of the highest quality and broadest interest. It will be maintained as well at the highest possible level of efficiency, both in organization and in cost. It is of course open to the AHA to decide to alter the essential nature of the journal in favor of other activities. But that would be a decision of great complexity, with consequences that would far outrun any obvious short-run gains in money. It is hoped that this editorial will have made clear, not only the rationale and accomplishments of changes in policy implemented over the past four years, but the possible consequences of a radically different definition of the journal and of its place within the American Historical Association and the profession of history in America.

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# The Florentine Palace as Domestic Architecture

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RICHARD A. GOLDTHWAITE

RENAISSANCE FLORENCE experienced a building boom probably more spectacular than that undergone by any other city in Europe throughout the Middle Ages. From the second half of the fourteenth century to the beginning of the sixteenth century construction was a general phenomenon throughout the city in all spheres of life—ecclesiastical, public, and private. Besides the completion of the cathedral, one of the largest churches in Christendom, with its great dome by Brunelleschi, the churches and monastic buildings of most of the dozen or so major orders in the city were largely, if not completely, remade. Almost a dozen new charitable institutions (*ospedali*) and many more, smaller convents were founded; and almost all the city's churches underwent some kind of architectural modification. At the same time a number of guilds erected new halls; and then, above all, there were the *palazzi*, the large private town houses of rich patricians, and their villas outside the city gates. All this building would be remarkable in any circumstance, but it is especially so in the case of Florence, inasmuch as the city, far from expanding, had experienced in the course of the fourteenth century such a drop in its population that by the beginning of the fifteenth century it was less than half the size it had been at the time of Dante—and it showed no signs of very dynamic growth throughout the period of the Renaissance. What is most remarkable is that this veritable building boom occurred during a period of stylistic innovation that marks one of the most glorious moments in the history of architecture.<sup>1</sup>

An earlier version of this article was delivered at a conference on the Renaissance at Wellesley College in February 1970. Additional research in Italy was made possible by a grant from the National Endowment for the Humanities.

<sup>1</sup> The subject of the building of Renaissance Florence has hardly been touched by scholarship. A good general view is to be found in Gene Brucker, *Renaissance Florence* (New York, 1969), ch. 1. There is nothing comparable to the treatments of the medieval Tuscan city in W. Braunfels, *Mittelalterliche Stadtbaukunst in der Toscana* (2d ed.; Berlin, 1959) and in Enrico Guidoni, *Arte e urbanistica in Toscana, 1000-1315* (Rome, 1971); but for some provocative ideas, see P. Francastel, "Imagination et réalité dans l'architecture civile de '400," *Homage à Lucien Febvre—Éventail de l'histoire vivante*, 2 (Paris, 1953): 195-206. For the later sixteenth century, see Giorgio Spini, "Architettura e politica nel principato mediceo del Cinquecento," *Rivista storica italiana*, 33 (1971): 792-845. An impressive list of buildings attributed to the Michelozzo

Private construction was by far the major sector of this building boom; perhaps as many as a hundred palaces were built in the course of the fifteenth century. In the history of art they represent an important stage in the elevation of domestic architecture to the realm of the fine arts and the introduction of a palace style that was picked up by the rest of Italy as well as all of Europe and imitated for the next three centuries. In the history of the city it is obvious that the total effect of the building of so many such vast works of art was the transformation of the physical appearance of the medieval city into the Florence we know today. Fourteenth-century descriptions of the city hardly mention private buildings; but for Benedetto Dei, writing about 1470, they were as important as public buildings in contributing to the glory of his "Fiorenza bella";<sup>2</sup> and a half century later Varchi almost tripled Dei's list of thirty notable palaces, adding that if one were to name merely those built after Dei wrote one "would have too much to do."<sup>3</sup> For Florentines at the time all this palace building clearly had a dramatic effect on the appearance of their city.

As much of an impression as the building of palaces made on contemporaries and as prominent as the palaces are even today on the Florentine scene, they have not been big enough to impress scholars. Even as art objects, to take the most salient aspect of the palaces, they constitute what one of the best architectural historians of Florence has called a no-man's land.<sup>4</sup> Almost all such structures are anonymous as works of art; they have not even been adequately cataloged. In these circumstances it goes without saying that we know hardly anything about the evolution of palace style in the Renaissance.<sup>5</sup> From an economic point

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circle alone is to be found in Howard Saalman, "The Palazzo Comunale in Montepulciano: An Unknown Work by Michelozzo," *Zeitschrift für Kunstgeschichte*, 28 (1965): 44-46. The standard bibliographical guide to printed materials for churches and other ecclesiastical buildings is Walter and Elisabeth Paatz, *Die Kirchen von Florenz* (Frankfurt, 1952-55); but the catalog of buildings in general by Walther Limburger, *Die Gebäude von Florenz* (Leipzig, 1910), is inadequate and long outdated. It is to be hoped that the recent international effort to inventory surviving urban buildings of the Middle Ages and the early Renaissance will arouse some interest in the urban development of Florence. See Robert S. Lopez, "Enquête sur l'architecture domestique et civile," in *Les constructions civiles d'intérêt public dans les villes d'Europe au Moyen Âge et sous l'Ancien Régime et leur financement* (Pro Civitate, série Histoire, no. 27 [1969]), annexe, 7-12.

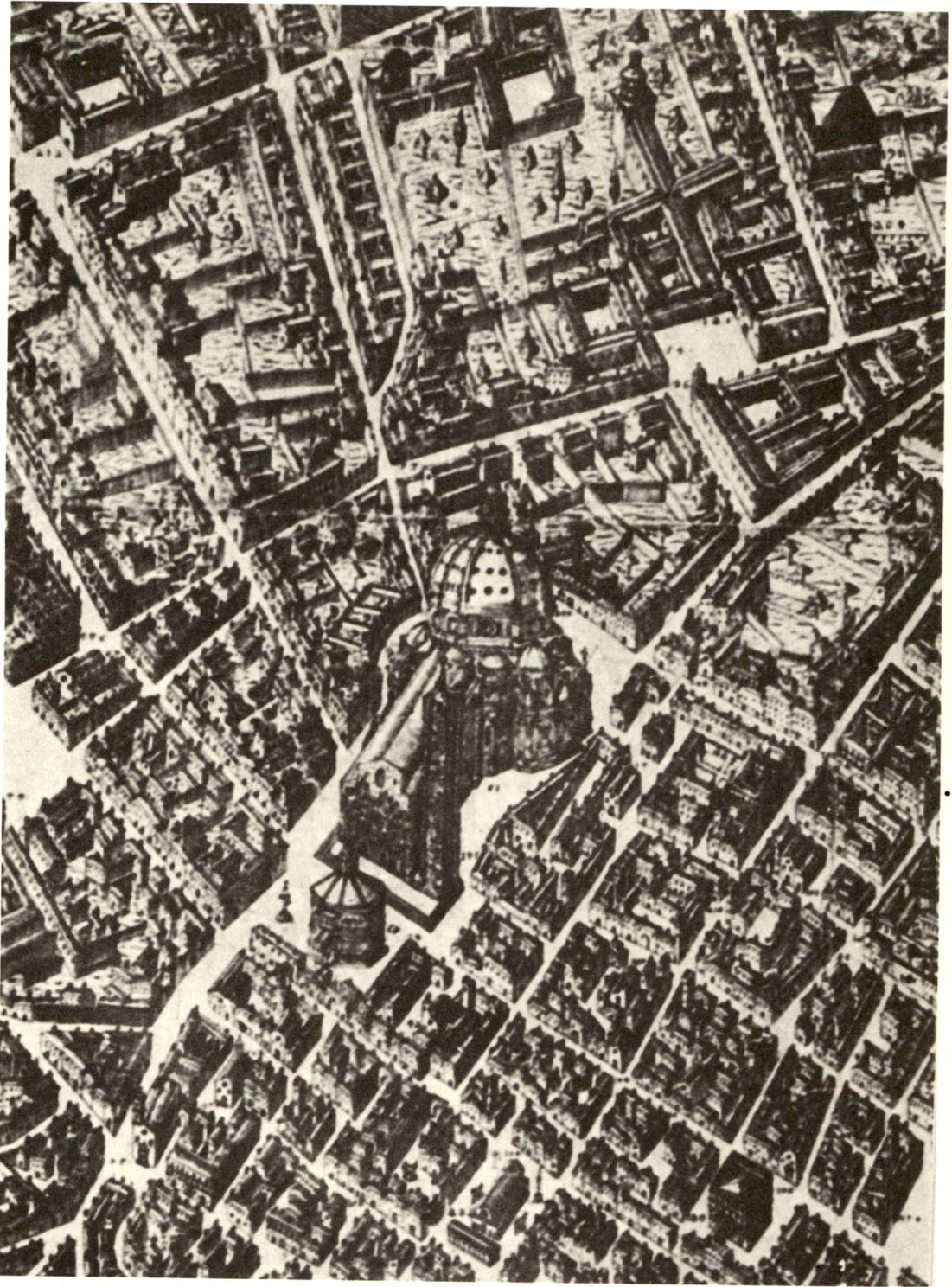
<sup>2</sup> Benedetto Dei, "Cronache," *Archivio di Stato di Firenze* (hereafter ASF), MSS, no. 119, fol. 34v.

<sup>3</sup> Benedetto Varchi, *Storia fiorentina* (Florence, 1888), bk. 9, sec. 38. Other Florentines who mention specific palaces are Francesco Baldovinetti (C. von Fabriczy, "Aus dem Gedenkbuch Francesco Baldovinetti," *Repertorium für Kunstwissenschaft*, 28 [1905]: 539-44); Antonio Billi (*Il libro di Antonio Billi*, ed. Carl Frey [Berlin, 1892]; "Il libro di Antonio Billi," ed. C. von Fabriczy, *Archivio storico italiano*, 7 [1891]: 299-368); and Agostino Lapini (*Diario fiorentino*, ed. G. Odoardo Corazzini [Florence, 1900]).

<sup>4</sup> Howard Saalman, "The Authorship of the Pazzi Palace," *Art Bulletin*, 46 (1964): 388.

<sup>5</sup> The catalog of Limburger with all of its inadequacies remains the most complete bibliography of printed materials; but Carocci's unpublished "Elenco degli edifizii monumentali" (1896) in the library of the Museo di Firenze com'era (shelf mark 25.D.27) is also useful. There is, however, a recent catalog of those palaces with decorated façades in Gunther and Christel Thiem, *Toskanische Fassaden-Dekoration in Sgraffito und Fresko, 14. bis 17. Jahrhundert* (Munich, 1964). A new series of fascicles, *Tutta Firenze ieri e oggi*, which began publication in





*Fig. 1.* This detail of Bonsignori's aerial view of Florence in the late sixteenth century shows quite clearly how little built up some areas were, even those right in the center of the city. Photograph: Alinari.

of view these palaces are an even bigger unknown. The construction of so many of them represented a significant shift in investment habits of the rich and at the same time provided a considerable stimulus to the internal economy of the city; yet none of this has ever been taken up by economic historians in their continuing and for the most part fruitless debates over the state of the Florentine economy.<sup>6</sup> Finally, the palace remains as big an unknown as a social phenomenon with respect to its function as a home—and what was a palace after all if not a home? It is the social aspect of the palace that is the subject of this article, and I offer it as the prolegomenon to the study of the palace not as an art object that marked a new era in taste but as a home that performed a new function in society.

IN THE COMMUNAL ERA the distinguishing feature of a great family's presence in the city was the concentration of the households of its various members in the same vicinity so that the family as a whole had a geographic identity in the city.<sup>7</sup> The great monuments of private architecture were in fact those structures that represented the families' collective public status and expressed their outward involvement in communal affairs. These structures were principally the great towers, where families defended themselves in their violent feuds with one another, and the open loggias on the streets, where residents assembled for public ceremonies in more tranquil times. The tower and the loggia served important public functions for the communal *consorteria* and symbolized its broad sociability and its internal cohesion in an era when public authority was still very feeble; and they were the focus of the family's location in the city. Even after the heyday of tower building, however, "new" families still tended to cluster together as they proliferated and grew in importance in business and politics. The Strozzi, for instance, at the opening of the fourteenth century bought up properties concentrated in the area of the present piazza Strozzi, including the piazza itself, which was bound by agreements among all the Strozzi to be preserved forever as their property;<sup>8</sup>

1971, is providing the popular market with a very full photographic coverage of palaces; another series has begun with Mario Bucci and Raffaello Bencini, *Palazzi di Firenze, introduzione all'architettura. Quartiere S. Croce* (Florence, 1971). This is not the place to present a full bibliography of the literature on the purely stylistic problems of Florentine palaces; but for two different general interpretations see Walter Paatz, "Ein antiker Stadthauptypus im mittelalterlichen Italien," *Römisches Jahrbuch für Kunstgeschichte*, 3 (1939): 129-40, and Bernhard Patzak, *Palast und Villa in Toscana: Versuch einer Entwicklungsgeschichte* (Leipzig, 1912-13).

<sup>6</sup> In Florence it was in the construction industry and woodworking (which would include furniture) that the artisan class showed the greatest growth between 1352 and 1551. Pietro Battara, *La popolazione di Firenze alla metà del '500* (Florence, 1935), 85.

<sup>7</sup> An attempt to locate these family areas in the thirteenth century has been made by F. J. Carmody, "Florence: Project for a Map, 1250-1296," *Speculum*, 19 (1944): 39-49.

<sup>8</sup> There are a few surviving fragmentary records of the purchase of parts of the piazza by several Strozzi in 1326, along with a seventeenth-century copy of their agreement to maintain

and the area became permanently identified with their presence. In the earlier period these family residential areas may have been something like great compounds, even architecturally enclosing the entire family and offering the possibility of being sealed off in times of public unrest. The sixteenth-century aerial view of the city by Bonsignori in which formal arches serve as entrance-ways to some side streets suggests that areas that may have corresponded to the concentration of residences of particular families achieved identity through architecture. With the possible exception of the so-called loggia of the Cerchi, no such archways survive today in Florence (although in Venice such structures can still be seen).<sup>9</sup>

Towers, loggias, family residential concentrations—these were the features of private architecture in the era of the commune. Individual buildings or palaces, although sometimes quite large, were not in themselves prominent monuments. Contemporaries do not even bother to mention them in their descriptions of the city.<sup>10</sup> Such buildings had little of what can properly be termed style; hence they lacked esthetic identity. Façades were often made of rusticated stone, but there was little variety and a minimum of decorative elements. At the street level there was usually a row of arched openings for entrances and shops (*archi da bottega*, as they are referred to in the documents) that ran continuously from building to building, so that the identity of each was somewhat lost in the continuity of the motif. Furthermore, to judge from the literary evidence of their destruction by fire and flood, riot and political vendetta, these earlier buildings could not have been very substantial structures. The worst fire was the holocaust of 1304, which, according to Villani, destroyed more than 1,700 buildings; and we have more than his testimony for the disaster wrought by the flood of 1333, which swept away almost everything along the river. Especially serious was the threat of fire, which was so ever-present that Paolo da Certaldo advised keeping large sacks and rope about the house so that when the alarm came one could collect his personal possessions and make a quick escape. The threat of political violence was just as serious: how often it is that the political annals tell us about the destruction by mob action of the houses of men whose political fortunes had gone awry. Considering all these

it always as a piazza, with a 1,000 florin penalty to whomever might build on it. ASF, Carte strozziane, ser. 3, no. 123, fol. 66; no. 191, fols. 4–12. There are records of the acquisition of properties in this area by the Strozzi going back to the beginning of the fourteenth century; earlier than this they apparently were not concentrated there. See the plan of Carmody, "Florence: Project for a Map," *passim*.

<sup>9</sup> An architectural description of Florence about 1300 (with bibliography) has recently been made by Ugo Procacci in the *Enciclopedia dantesca*, 2 (Rome, 1970): s.v. "Firenze: l'aspetto urbano di Firenze dai tempi di Cacciaguida a quelli di Dante."

<sup>10</sup> The only private palace mentioned by these early chroniclers is that of the Tossinghi, which is described impressively by Malispini: "alto novanta braccia, fatto a colonnelli di marmo, e una torre con esso alta braccia centotrenta"; but it was destroyed in 1239, and considering the problems of dating Malispini's chronicle we can hardly be sure about the accuracy of the description. See Patzak, *Palast und Villa in Toscana*, vol. 1, bk. 1: 58, where there is a fanciful reconstruction of the palace.



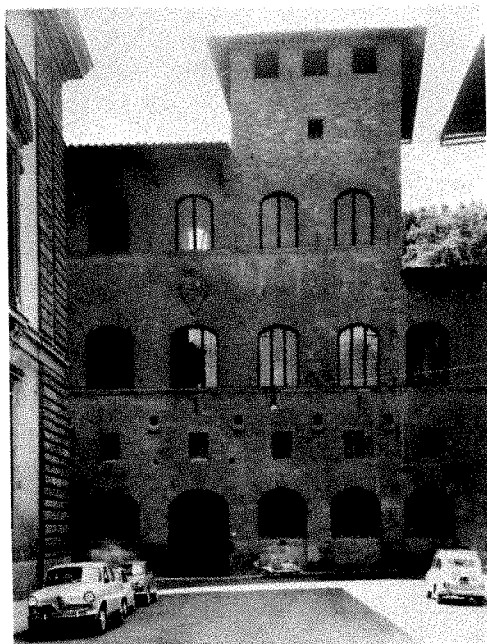


Fig. 2, left. The palaces of the Mozzi family, which probably have undergone much re-finishing on the exterior, are among the few that survive from an earlier period. With their series of arches at street level, their unadorned façades, and the base of a tower, they represent something of the anonymity of the architecture of earlier palaces. Photograph: Alinari.

Fig. 3, below. Domenico Veneziano's *A Miracle of St. Zenobius*, a mid-fifteenth-century street scene. Notice the extensive use of wood buttressing (*sporti*) for protruding upper floors and roofs, a particularly hazardous feature that contributed to the prevalence of fires in the earlier period. Reproduced by permission of the Fitzwilliam Museum, Cambridge.



aspects of their vulnerability it is not difficult to conclude that many of these medieval buildings could not have been the structures their successors were. The fifteenth-century biographer of Brunelleschi, looking back to the historical antecedents of the new palace style of his own era, was probably right in commenting that "during that period the method of building was very crude as may be observed in contemporary and earlier buildings."<sup>11</sup>

These earlier private buildings not only lacked structural monumentality and esthetic identity, but for the most part they had no precise functional identity. They must have been very much like the modern Italian *palazzo*, with shops below and apartments within. With the commercial activity of the shops at street level and stairways that opened directly onto the streets there was a constant penetration of street life into the palace, so that the private and public worlds were not so clearly demarcated. Within the palace ownership was frequently divided; and shops, apartments, and even single rooms could be held by a number of different parties not necessarily of the same family.<sup>12</sup> Buildings, therefore, did not identify single private residences; and conversely, the private household—even that of a patrician—did not necessarily have an architectural identity. When the very wealthy Messer Pagolo di Baccuccio Vettori wrote out in his diary a description of his house, he found that it was structurally all jumbled up with his neighbor's, with common walls, shared loggias, and division by floors—and he apparently was not very sure of being able to get an agreement on what the division actually was.<sup>13</sup> A century later Bartolomeo di Tommaso Sassetti (brother of Francesco, the rich business associate of the Medici) inherited the family house, or palace, as he calls it; but it was hardly a very private and distinct building. When the brothers divided it a room that should have remained part of the house somehow went instead to Francesco's house next door. Moreover, the window of an adjacent house was situated in Barto-

<sup>11</sup> Antonio Manetti, *The Life of Brunelleschi*, ed. Howard Saalman (College Park, Pa., 1971), 40 (commenting on Brunelleschi's first palace, for Apollonio Lapi).

<sup>12</sup> This vagueness about the identity of a building is frequently encountered in the tax declarations of the *catasto*. There it was customary to describe property in the city by identifying all contiguous properties; and when a declarant listed, for example, a shop that he may have owned in a larger building, he may not have described it as being in the building but may merely have listed the owners of the adjacent properties, whether or not they were other parts of the building. In such a case the fact that the shop was in a larger building would only emerge (if at all) from information on other declarations. This is a major obstacle to using *catasto* records to reconstruct the city as it appeared in the fifteenth century.

<sup>13</sup> "Ricordanza che addì xv di febraio cccxviii io Pagolo Vettori feci una scritta di mia mano a Capponcino che la pancha e'l tetto della loggia che passa il mio pilastro del muro è in su quello di Capponcino; e il muro della loggia della corte come tiene la loggia e la sala insino al bordone della sala è tutto del detto Capponcino, e dal bordone in su è tutto mio tanto quanto tiene la mia casa, e dinanzi dov'è la loggia è tutto mio il muro infino a' merli; e a ogni sua petizione debbo levare ciò che io tengo del suo e'l simile de' fare a nme di sgonbrare dove lui achupassi [sic] del mio"; but then he added out at the side, "non ebbe la scritta da nme." ASF, Carte Riccardi, no. 521 (*ricordi* of Messer Pagolo di Baccuccio Vettori, 1331-77), fol. 26r. Besides this residence, which was in the parish of San Jacopo sopr'Arno, Vettori possessed five other houses in the city and seventy-eight rural properties. *Ibid.*, fols. 18r-20r.

lomeo's courtyard, and it was possible to enter his house from the window.<sup>14</sup> A product of the fifteenth century, when standards of privacy were more highly developed than they had been in the previous century, Sassetti eventually bought up these other properties precisely so he could close himself off in his own residence. But in the fourteenth century it must not have been unusual for even the rich and powerful to live in such an architectural hodgepodge. There may in fact be some basis in Trecento architectural reality for that marvelous sense of space in the *Decameron* where the continual flow of characters in and out of bedrooms and houses almost defies a definition of privacy.

It is not until the second half of the fourteenth century that the palace begins to come into its own as a distinctive and more conspicuous monument with a clearer stylistic and functional definition. In fact, although archeological research is still lacking, the few palaces traditionally considered medieval are very likely to have actually been built (or much rebuilt) after the Black Death. In any case, the pace of building increased toward the end of the century; and in contrast to the anonymous buildings of an earlier era the homes men now built slowly assumed something of the individuality of minor works of art. Unfortunately that process has never been subjected to a careful morphological analysis. It seems to have begun with the esthetic isolation of façades from their neighbors by emphasis of the principal entrance and eventually by elimination of all other openings at the street level. Slowly over the next century more possibilities were developed for treatment of the entire façade, usually with a complex system of rustication or contrasting use of *intonaco* and stone, but sometimes with an elaborate surface decoration in fresco or *sgraffito*; and there was more conscious use of decorative elements such as string lines, window moldings, cornices, and even pilasters. Inside, too, there were innovations, notably interior vaulted staircases, the opening up of a large rectangular courtyard surrounded by arcaded porticos and loggias, and the beginning of systematic planning of internal space. With such ideas as these—all expressed with an increasingly classical vocabulary—architects were finally able to endow the private Florentine home with what can properly be called style.

Another dramatic feature of these new homes is the scale on which they were built. There were of course very large medieval palaces (if one is to accept the traditional but dubious dating of some surviving buildings), but even the average Renaissance palace is an extraordinarily massive building for a home, a social fact not usually commented on by art historians. Henry James, for one, was struck by the sheer height of the structures; four centuries after they were built he could still write that they were "the tallest habitations in Europe that are frankly and

<sup>14</sup> ASF, Carte strozziane, ser. 5, no. 1750 (accounts and *ricordanze* of Bartolomeo di Tommaso Sassetti, 1455-71), fols. 181 left, 184 left.

amply habitations—not mere shafts for machinery of the American grain-elevator pattern.” They are so beautifully proportioned that we can easily overlook the fact that their usual elevation of three stories is two, perhaps even three, times the height of three-story buildings in our less spacious (and less well proportioned) age. Their horizontal extent is no less impressive. As we know from tax records (the *catasto*) in almost all cases each palace replaced three or more pre-existing structures, usually including the former family residence. Some, in fact, were not altogether new constructions but primarily new façades behind which several older buildings were simply thrown into a single residence. Virtually all had a large open interior space in the form of an arcaded courtyard; and many originally had private gardens attached to them, even those in the center of the city—so many that both Dei and Varchi considered the fact worth mentioning as a distinguishing characteristic of the Florence they were describing.<sup>15</sup>

The Renaissance palace, in short, represented the esthetic identification of the private home and at the same time a dramatic enlargement of residential space. These buildings were more significant structurally as well. Looking at this new generation of palaces today it is hard to imagine that flood or fire or even the will of an infuriated mob could do much damage to them, and indeed by the fifteenth century we do not hear anything more about this kind of destruction in the annals of the city. It can be no accident that they increasingly come to be called palaces, not simply houses—“a house, or rather a palace,” as they are described time and time again by their owners in tax declarations and private accounts. According to Varchi they were veritable palaces, which had “all the ornaments and all the comforts that houses can have, such as terraces, loggias, stables, courtyards, hallways and rooms, and above all”—he concludes on a more practical note—“if not two, at least one well with healthy and fresh water.”<sup>16</sup> And the very word *palazzo* was generally reserved by the architectural theorists only for the homes of princes.

One could speculate on the urban shift, as we would call it today, that must have occurred with the building of palaces throughout the city. To the extent that they replaced former commercial space, the building of so many of them must have had a considerable total effect on the dislocation of local commerce, so that in those areas where palaces were concentrated—contemporary observers singled out the via Maggio, the via dei Servi, the via Tornabuoni, and borgo Pinti—the bustle of street life must have been appreciably reduced. A good example of this transformation is the piazza Strozzi. Although since the early Trecento this was the nucleus of the great Strozzi clan, it was also a busy urban area, full of many modest houses and shops of all kinds. In the mid-fifteenth

<sup>15</sup> Dei, “Cronache,” fols. 29r–35v; Varchi, *Storia fiorentina*, bk. 9.

<sup>16</sup> *Storia fiorentina*, bk. 9, sec. 38.

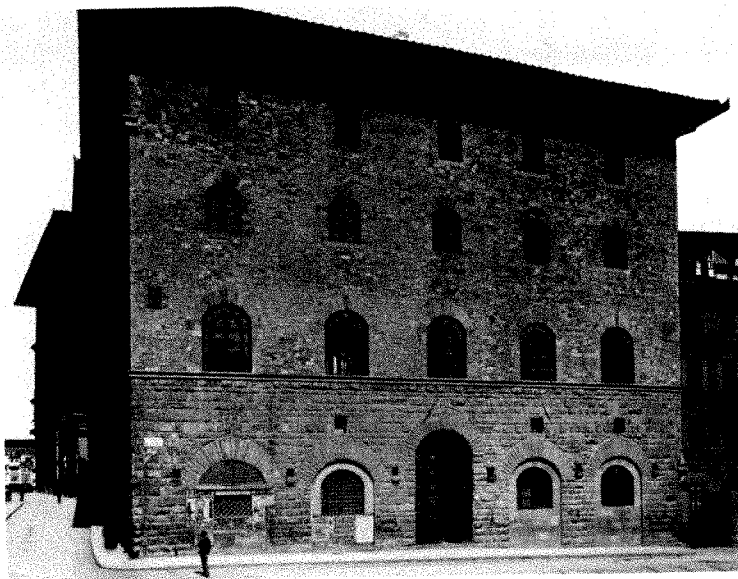
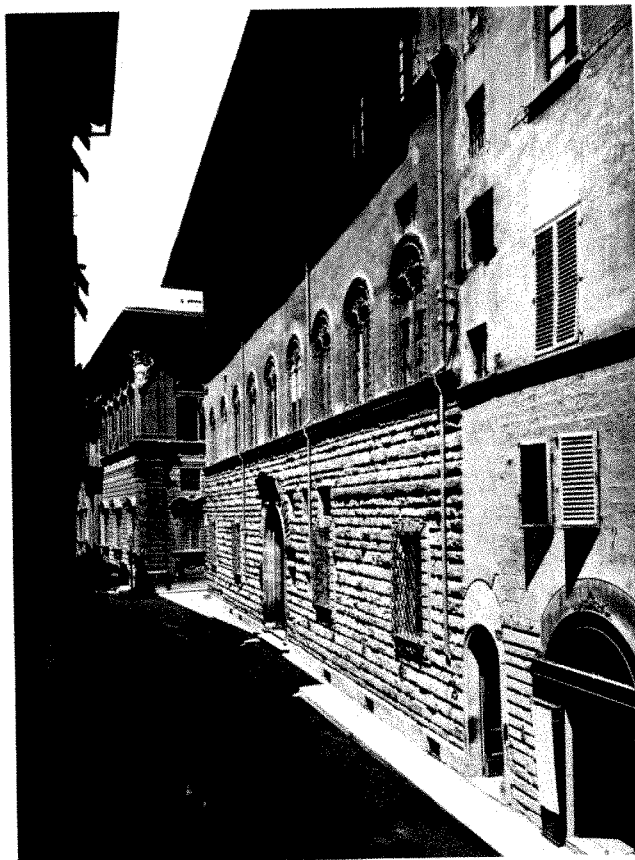


Fig. 4, above. The Castellani palace, built in the second half of the fourteenth century (and later enlarged), represents a stage in the development of increased esthetic identification of the palace. Besides the somewhat irregular arched openings, the principal entrance becomes the central focus of the building; and the rustication of the lower level is an additional embellishment. Photograph: Alinari.

Fig. 5, below. The Pazzi palace, with its contrasting use of rustication and *intonaco* and its richly decorated window moldings. Photograph: Alinari.



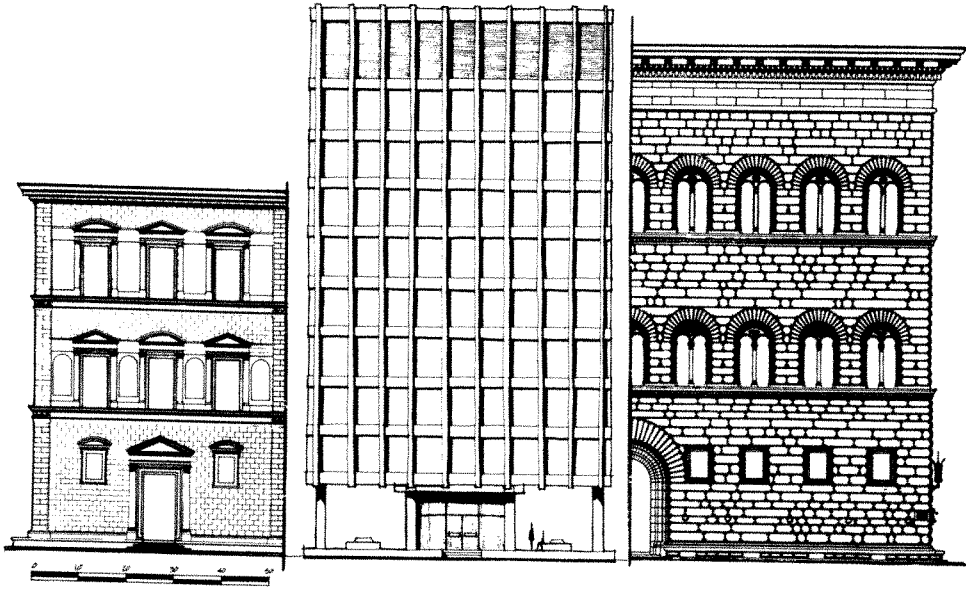
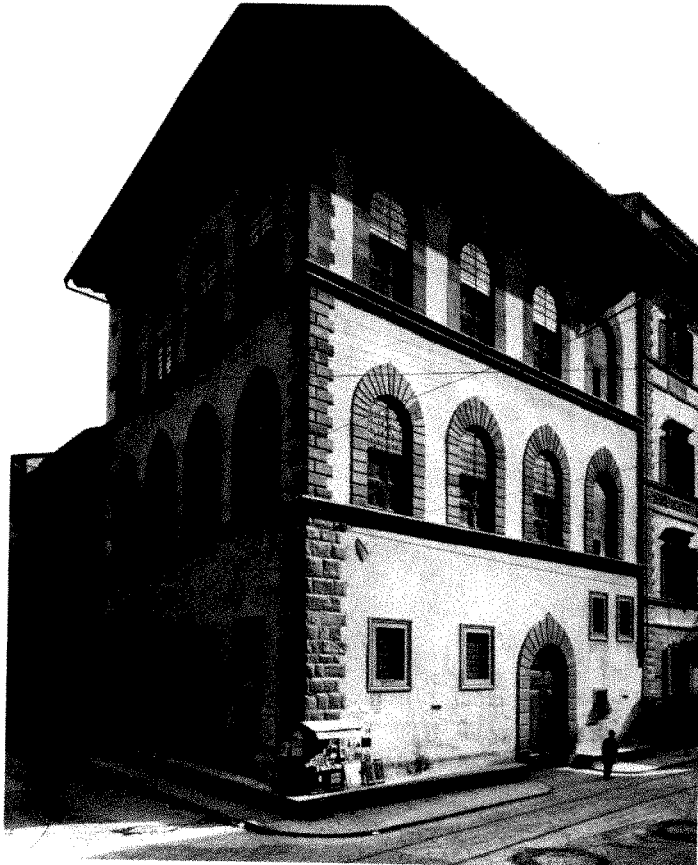


Fig. 6, above. Some idea of the scale of Florentine palaces can be had from this comparison of a ten-story modern building and two palaces, the Bartolini (left) (from Grandjean di Montigny and Famin), about the average size of Florentine palaces, and half the short side of the Strozzi (right) (after Stegmann and Geymüller), the largest of fifteenth-century palaces (cf. fig. 8). Rendering by Implementation, Baltimore.

Fig. 7, below. The Corsi palace, whose size is typical of Florentine palaces. Its façade, a combination of *intonaco* and stone, represents one of the characteristic stylistic types. The palace has no more than a dozen rooms, but notice its size with respect to neighboring buildings. Photograph: Brogi-Alinari.



century Palla di Palla Strozzi cleared out over a half dozen of these small houses (*casette, casolari*) to erect the palace known as the Strozzi; a generation later Filippo di Matteo Strozzi transformed several more into a small palace, and by the end of the century over a dozen different kinds of shops (along with three buildings identified as patrician residences, one complete with tower) were replaced by the great Strozzi palace.<sup>17</sup> Altogether, the building of these three palaces, not to mention a few others in the same square, removed considerable street activity from the vicinity and transformed it into a patrician residential center. A more notable instance is the via Maggio, which in the fourteenth century was one of the two principal centers of the flourishing wool business, the single most important industrial activity in the Florentine economy. In the fifteenth and early sixteenth centuries it became lined with the great palaces that even today make it one of the most authentic Renaissance streets that survive in all Florence, and with their construction the bustling trade was removed from the area. One observer tells us that these palaces in the via Maggio displaced sixty wool shops, and by the mid-sixteenth century only five remained in the entire Oltrarno quarter.<sup>18</sup> The new palace style, therefore, not only detached the private residence from street activity but in some cases at least led to a removal of much of that activity from the vicinity, which thereby became more strictly residential. In a sense the Renaissance palace was a kind of coagulant that reduced the fluidity of medieval urban life with its continual interpenetration of the public and private spheres.

With this newly gained aloof presence on the Florentine scene the patrician residence moved into its own as the characteristic monument of private architecture; and if this was the consequence of the withdrawal of the family into a new realm of privacy, so the same social process of involution led to the obsolescence of those earlier patrician monuments that had formerly symbolized the family's active outward involvement in the public affairs of the commune—the tower and the loggia. The towers had for the most part disappeared by the fourteenth century primarily as a result of the emergence of a new concept of public authority and the government's deliberate policy of tearing them down in order to reduce the political and military might of the clan. On the other hand, the loggia, the public assembly point for the family, simply became outmoded as the family itself dissolved.<sup>19</sup> One sixteenth-century chronicler

<sup>17</sup> The property descriptions in the *catasto* declarations of Palla di Palla Strozzi (not Messer Palla di Nofri, as they are identified by Fabriczy) are published by Cornelius von Fabriczy, in "Michelozzo di Bartolomeo," *Jahrbuch der königlich preuss. Kunstsammlungen*, 25 (1904), Beiheft, 102-04. Properties replaced by the palace of Filippo Strozzi are listed in Guido Pampaloni, *Palazzo Strozzi* (Rome, 1963), 44n.

<sup>18</sup> "Di poi insino a questo anno 1520 in via maggio semurato molte chiese bellissime elevato via piu di sessanta botteghe darte dilana ghrosse vi si faceva." Baldovinetti, "Memoriale," 543. For the distribution of wool shops in the mid-sixteenth century see Pietro Battara, "Botteghe e pigioni nella Firenze del '500: un censimento industriale e commerciale all'epoca del granducato mediceo," *Archivio storico italiano*, 95 (1937), pt. 2: 9.

<sup>19</sup> For general remarks on loggias, see Guido Carocci *et al.*, *Il centro di Firenze: studi storici*

implies that by his time the loggia was no longer being used;<sup>20</sup> and as far as we know only two of the new projects for palaces in the Renaissance included loggias. The corner of the Medici palace originally opened as a loggia—although the Medici, of course, are never the appropriate example of patrician practice in general; one could argue that their public prominence makes them the exception that proves the rule. Still, in the early sixteenth century even they enclosed their loggia. The other example is the loggia of the Rucellai across the street from the palace designed by Alberti. Built by the most prominent private builder in Renaissance Florence after Cosimo de' Medici, the Rucellai loggia was erected on the occasion of a marriage between the two families.<sup>21</sup> It is, however, perhaps the last loggia to have been constructed. When in the early sixteenth century Varchi drew up a list of surviving loggias there were only twenty-six names on it.<sup>22</sup> It can be ascertained that clearly a third of those had nothing to do with palace projects in the fifteenth century, and there is no evidence that it was otherwise with the rest. Loggias simply had no more function in the life of the patrician family, many of whom walled up these areas to use them for other purposes. In the later sixteenth century they were considered signs of ancient family nobility by some men who looked with nostalgia on these outmoded and disappearing family monuments of a past age. Hence, when a Frescobaldi heiress closed up her family's loggia in order to make space for four shops, she was accused of avarice and of putting utility before honor and respect for ancient tradition.<sup>23</sup> Sentiment, however, could not revive practicality. By the end of the century Borghini, commenting on the nobility of these family monuments, was able to discern only fourteen, and of these at least six had already been walled up or were in ruins.<sup>24</sup> Public assembly in the streets was obviously a thing of the past for the patrician family, just as it no longer had need for mass defensive action in its towers. The new symbol of its status was now the residence itself.

HOW DO WE EXPLAIN why men felt the urge to build on such a grand scale? Perhaps it is obvious that, in part at least, the phenomenon can be explained as the desire for public display of private status; and if it

*e ricordi artistici* (Florence, 1900), 49-53; Marco Tabarrini, "Le consorterie nella storia fiorentina del medio evo," *La vita italiana nel Trecento* (Milan, 1904), 115-16; Attilio Schiaparelli, *La casa fiorentina e i suoi arredi nei secoli XIV e XV* (Florence, 1908), 67-72; and Patzak, *Palast und Villa in Toscana*, vol. 1, bk. 1:39.

<sup>20</sup> "Nelle loggie, che in questi tempi qualche poco ancor si frequentavano." Filippo Nerli, *Commentari dei fatti civili occorsi dentro la città di Firenze dall'anno 1215 al 1537* (Florence, 1859), 1: 62.

<sup>21</sup> Thiem, *Toskanische Fassaden-Dekoration*, 69-70.

<sup>22</sup> *Storia fiorentina*, bk. 9, sec. 40.

<sup>23</sup> "Con molto biasimo d'avarizia d'una gentildonna di quella famiglia, antepoendo l'utile all'honore e al mantenimento della memoria di tanta antichità." Baccio Cecchi, "Memorie," ed. I. Del Badia, *Miscellanea fiorentina di erudizione e storia*, 1 (1902): 37.

<sup>24</sup> "Delle famiglie nobili fiorentini," ASF, MSS., no. 190, doc. 3.



is not surprising from the point of view of our consumer-oriented society that rich men engaged in such conspicuous consumption, it is something strikingly new in the history of moral thought that now intellectuals broke out of the context of the medieval world and actually sought to justify such spending on moral grounds. In an earlier era extravagant spending was something writers warned against, either because it was improper in a Christian society inspired by Franciscan ideals and on its guard against excessive materialism or (perhaps more realistically in the competitive business community of Florence) because it simply was too dangerous to expose one to the jealousy and envy of other men. The anonymous fourteenth-century Florentine merchant who said that "spending a lot and making a big impression are in themselves too dangerous" offered the kind of advice that is typical of his age.<sup>25</sup> At the beginning of the fifteenth century, however, the humanists introduced a new civic ethic; they rejected traditional ideals of Franciscan poverty and worked out instead an explicit moral justification for private wealth.<sup>26</sup> Drawing heavily on Aristotle's ideas about magnificence and the good man, they praise wealth precisely because it gives man the wherewithal to express his status publicly. Leon Battista Alberti and Matteo Palmieri in their moral tracts could still counsel against extravagant expenditures and call for personal economy, although their arguments were slightly different from those of their medieval predecessors; but when it comes to building, an altogether new attitude is introduced. "The magnificence of the buildings," says Alberti, talking about houses, "should be adapted to the dignity of the owner."<sup>27</sup> And Palmieri warns that "he who would want . . . to build a house resembling the magnificent ones of noble citizens would deserve blame if first he has not reached or excelled their virtue."<sup>28</sup> "But since all agree," concludes Alberti, "that we should endeavor to leave a reputation behind us, not only for our wisdom but our power too, for this reason . . . we erect great structures, that our posterity may suppose us to have been great persons."<sup>29</sup>

Alberti was right; in Florence building did become a measure of a man's greatness. Giovanni Rucellai, one of the wealthiest merchants and most vigorous builders in mid-fifteenth-century Florence, was explicit on the point: "I think I have given myself more honor, and my soul more satisfaction, by having spent money than by having earned it, above all

<sup>25</sup> "Troppe male circhunstanzie à in sè il tenere grande stato e fare grande spese." Gino Corti, ed., "Consigli sulla mercatura di un anonimo trecentista," *Archivio storico italiano*, 111 (1952): 118. See also Robert S. Lopez and Irving W. Raymond, eds., *Medieval Trade in the Mediterranean World* (New York, 1955), 423.

<sup>26</sup> Hans Baron, "Franciscan Poverty and Civic Wealth as Factors in the Rise of Humanistic Thought," *Speculum*, 13 (1938): 1-37.

<sup>27</sup> *Ten Books on Architecture*, trans. James Leoni (ed. Joseph Rykwert; London, 1955), bk. 9, ch. 1.

<sup>28</sup> "Chi sequitasse e valesse assimigliare le magnifiche case de' nobili cittadini, merita biasimo se prima non ha aggiunte o superate le sue virtù." *La vita civile* (Bologna, 1944), 164.

<sup>29</sup> *Ten Books on Architecture*, bk. 9, ch. 1.

with regard to the building I have done."<sup>30</sup> Furthermore, what is it that Florentines talk about when they discuss the art patronage of their fellow citizens? Building, almost exclusively. For the adulators of Cosimo de' Medici his building projects were the best proof of the magnificence of the man; and commentators from Vespasiano to Machiavelli hardly mention anything else with respect to Medici patronage in general.<sup>31</sup> It is impossible to find any comment in the chronicles and histories of the period on men's patronage either of sculpture or of painting, the puny private arts; but when men built they made an impression on their fellows and on history, not only with the buildings themselves but in written commentary about them. Furthermore, despite popular myths it was the patron not the artist who made the impression. In his highly laudatory biography of his father, Lorenzo di Filippo Strozzi puts particular emphasis on Filippo's building projects but never once bothers to mention an architect's name—not even that of Cronaca, the creator of the Strozzi palace, whom Lorenzo, a man of great learning and interest in the arts, knew well.<sup>32</sup> In fact, before Vasari there is hardly a contemporary attribution for any of these Florentine palaces, including the Medici and even the Pitti, which was immediately recognized as one of the great landmarks of the city. Palaces were seen not as creations of artists but as monuments to their builders' magnificence.

The personal splendor these palaces conferred on their builders was amplified by the hope that the structures were to be the monumental foundation of the dynasty that was to extend from the builders themselves. Filippo Strozzi was obsessed with the concern that his great monument be forever exclusively the residence of his family, and in his exceptionally lengthy testament he rambles on in tortuous detail to anticipate all contingencies in the line of his descent to make sure that it would always remain Strozzi property and would never be alienated.<sup>33</sup> Giovanni Rucellai was less verbose but just as insistent that his palace was always to belong to someone with the name Rucellai ("genie seu stirpe vel consorteria de oricellaris") and was never to be alienated or even rented. He was particularly adamant in his insistence that whatever happened the palace was under no circumstances to belong to any other Florentine family; in the case of the extinction of the Rucellai the building was to pass to the commune to be used (appropriately) as the residence of an ambassador of a foreign prince—provided that he not be

<sup>30</sup> *Giovanni Rucellai ed il suo zibaldone*, vol. 1: *Il zibaldone quaresimale*, ed. Alessandro Perosa (London, 1960), 118.

<sup>31</sup> See A. D. Fraser Jenkins, "Cosimo de' Medici's Patronage of Architecture and the Theory of Magnificence," *Journal of the Warburg and Courtauld Institutes*, 33 (1970): 162-70, and also Rab Hatfield, "Some Unknown Descriptions of the Medici Palace in 1459," *Art Bulletin*, 52 (1970): 240-41.

<sup>32</sup> *Vita di Filippo Strozzi il Vecchio scritta da Lorenzo suo figlio*, ed. Giuseppe Bini and Pietro Bigazzi (Florence, 1851).

<sup>33</sup> ASF, Carte strozziane, ser. 5, no. 1221.

Florentine or of Florentine origin!<sup>34</sup> By the sixteenth century a man could best satisfy dynastic ambitions by building a palace; as it was expressed by Michelangelo, who wanted perhaps nothing more from his spectacular career than to re-establish the fallen patrician status of his family in Florence, "a noble house in the city brings considerable honor, being more visible than all one's possessions, and because we are citizens descended from the noblest of lines."<sup>35</sup> Most of the great palaces built in the later sixteenth century were in fact built by those men whose status had been newly established by the grand dukes.

As Michelangelo observed, palaces gave visibility to the status of a family, and to leave no doubt of their identity they were endowed with sure signs of their ownership. Façades proudly bore the coats of arms of their owners; and in some the armorial bearings of the family and even the personal devices of their builders were worked into the very fabric of the structure as decorative motifs. The Pazzi dolphins, for example, constitute the principal element of the capitals and corbels of that family's palace; on the stairway of his, Giuliano Gondi put a modified form of his private insignia on the faces of each of the step ends;<sup>36</sup> and running all the way across the façade of the Rucellai palace are the sails of fortune that had such a personal symbolic importance for Giovanni, its builder.<sup>37</sup> Undoubtedly the most exaggerated case of palatial flouting of ownership is the Strozzi palace, where the Strozzi moons appear everywhere—in the spandrels of the windows, incised on virtually every piece of iron on the façade, worked into the interior corbels (along with falcons and sheep, the builder's own personal devices). Furthermore, a horoscope was cast for the Strozzi palace to determine the propitious moment to begin its construction; an elaborate ceremony celebrated the occasion and a medal was struck to commemorate it. And it may be that such rituals accompanied the construction of other palaces, endowing them with a kind of mystic personality that loomed behind the public symbols of their identity.

In a society of entrepreneurs, where personal magnificence and dynastic ambitions could find such conspicuous public expression, where the competitive instinct must have taken its own course once men started to build, how was the proud patrician to resist the challenge? An ambassador from Venice was impressed with the amounts of money being spent by the Florentines, who, he says, were building on the heels

<sup>34</sup> ASF, Notarile antecosimiano, L 130, no. 31. My thanks to Rab Hatfield for this reference. Cf. *Il zibaldone quaresimale*, 144.

<sup>35</sup> "Perchè una casa onorevole nella città fa onore assai, perchè si vede più che non fanno le possessioni, e perchè noi sian pure cittadini discesi di nobilissima stirpe," cited in Ugo Procacci, ed., *La casa Buonarroti a Firenze* (Florence, 1965), 6.

<sup>36</sup> See John Pope-Hennessy, *Catalogue of Italian Sculpture in the Victoria and Albert Museum*, 1 (London, 1964): 183–87.

<sup>37</sup> Aby Warburg, "Le ultime volontà di Francesco Sassetti," in *La rinascita del paganesimo antico* (Florence, 1966), 236.

of one another at such a pace that some who had only 60 or 70 florins income in cash might spend as much as 6,000 or 7,000 florins or more for a palace in the countryside.<sup>38</sup> Although apparently he was talking about villas and his figures (and ratios) are exaggerated, he must have sensed something of the building craze Florence was experiencing at the time. The palace of the average patrician—to judge from the evidence of accounts and *ricordi*—had a book value of between 1,500 and 2,500 florins, which could have represented as much as fifty per cent of his total estate. The really big ones, of course, cost much more—Filippo Strozzi (and his heirs) paid almost 40,000 florins for what was probably the most expensive in the city (equal to over a thousand times the annual salary of a highly skilled workman),<sup>39</sup> and this represented over a third of his total wealth. The builders of some of the most famous of these palaces, however, were not so fortunate as to be rich enough, or secure enough, to afford the luxury of indulging in the new fad of trying to make an impression. For all his smug satisfaction, Giovanni Rucellai did not finish his palace, which after all was primarily no more than a façade. Giuliano Gondi made all Florence sit up and take notice of the new palace he started building in 1489, but he was not able to get it half built (and it stood unfinished until the nineteenth century). Bono Boni presumably finished his in the mid-1460s, but within a decade he was bankrupt and had to sell it.<sup>40</sup> And so with other builders, like Lorenzo de' Larione, who had to sell his on the heels of bankruptcy,<sup>41</sup> and Bartolomeo Barbadori, whose burden of debts forced him to halt construction.<sup>42</sup> It is surprising how many of these palaces changed hands within a generation of their construction; one suspects that in these cases the family fortune was simply not up to the drain of capital a palace required. Nevertheless, it is in this context of competitive, extravagant, and sometimes wreckless egotism that we can partly understand the scale on which these men built as well as their awakening consciousness of style.

The building of palaces appears all the more extravagant when one considers that the inflation of space they represented had a minimum economic value almost entirely limited to its importance for the household economy of the family. To be sure, the rooms on the ground floor might have included an office for the commercial and banking business of the owner, and to this extent he saved something in rent. Datini's

<sup>38</sup> *Relazioni degli ambasciatori veneti al senato*, ed. Arnaldo Segarizzi (Bari, 1912-16), 3, pt. 1: 19.

<sup>39</sup> Obviously even approximate equivalents of the dollar and the florin cannot be made; but the cost of the Strozzi palace in relation to today's skilled worker's income of ten thousand dollars would be ten million dollars.

<sup>40</sup> Today Palazzo Antinori. Maddalena Trionfi Honorati, "Il palazzo degli Antinori," *Antichità viva*, 7 (1968), no. 2: 65-80 (based on unpublished research of Guido Pampaloni).

<sup>41</sup> Today Palazzo Canigiani, via de' Bardi, 28/30. Saalman, "The Authorship of the Pazzi Palace," 391n.

<sup>42</sup> Manetti, *The Life of Brunelleschi*, 99.

office (but not his *fondaco*), for instance, was on the ground floor of his palace, as was the *scrittoio* of many other merchant bankers, to judge from surviving inventories. But if in addition to these financial interests a patrician had warehouses or shops that were used for industrial production, for the manufacture of silk or wool cloth—and most Florentine patricians probably had investments in such establishments—these were not located on residential premises; they were located elsewhere and sometimes the owner even paid rent for the shop rather than turn over a part of his home for its operation. Furthermore, it was not likely that any space in these palaces was rented out. The earliest of them still had shops that opened onto the street for commercial use, but as palace style evolved these were excluded. To judge from a comment of Lorenzo Strozzi's such an arrangement was considered a blight on the beauty of the building as well as an inconvenience for the owner.<sup>43</sup> Moreover, no interior space was rented as living quarters, and we have no evidence that these palaces were used then as most are used today, as apartment houses. As a matter of fact, in those cases where the entire palace was later rented out, usually as a result of difficulties among heirs or the absence from Florence of the owner, the income was so small that it would have been an insignificant return on the total capital outlay represented by the building. In the 1490s the Antinori (then Martelli) palace was rented for between eighty and a hundred ducats a year;<sup>44</sup> in the middle of the following century half the Strozzi palace (it was built as two residences) was offered for rent at forty-five florins;<sup>45</sup> and a few years later the Corbinelli rented one of their palaces in the via Maggio to Bianca Cappello for fifty florins.<sup>46</sup> At a time when eight per cent was a normal return on an investment these rents would have represented a capital investment of 500 to 1,000 florins, hardly anything like the building costs of such structures.

This extraordinarily low rental potential meant that the market value of a palace was a very nebulous matter. At the end of the sixteenth century, for example, when the Pazzi palace was sold to Lorenzo di Carlo Strozzi, the price was no easy thing to determine. Several consultants were called in. One based his calculation on the capitalization of its net rental value of 180 *scudi* (after taxes and maintenance expenses) at three per cent. Three per cent was a very low return on an investment for a Florentine, even in the late sixteenth century, and it was obviously used

<sup>43</sup> "Disegnando di fare sotto la casa molte botteghe per entrate dei suoi figliuoli: il che arditamente gli era contradetto, mostrando di quanta bruttezza, servità e incommodo saria alli abitatori." *Vita di Filippo Strozzi*, 25.

<sup>44</sup> Owned then by Carlo d'Ugolino Martelli. ASF, Carte strozziane, ser. 5, no. 1468 (accounts and *ricordi* of Luigi d'Ugolino Martelli, 1489-92), fol. 108r; no. 1469 (accounts of Luigi d'Ugolino Martelli, 1492-93), fol. 11.

<sup>45</sup> ASF, Carte strozziane, ser. 5, no. 136 (accounts and *ricordanze* of Palla di Lorenzo di Filippo Strozzi, 1552-55), fols. 204r-208v.

<sup>46</sup> *Illustratore fiorentina, calendario storico*, 1906, ed. G. Carocci, 27-29.

to push the value as high as possible; yet the resulting figure of 6,000 *scudi* was so ridiculous that the assessors, "considering the nobility of the site and its beauty as well as other things," arbitrarily increased it by one-third to 8,000 *scudi*. On the other hand, a detailed builder's estimate of replacement costs drawn up for the same purpose put a value of 11,900 *scudi* on the structure, a remarkable sum considering the fact that inflated labor costs in the second half of the sixteenth century probably meant that the building could not have been built for even that figure.<sup>47</sup> In 1659 the Medici palace was sold to the Riccardi for 40,000 ducats, about the same price it had cost to build two centuries earlier, but as a result of inflation the sale price represented only half the value of the original building cost. Moreover, the Riccardi spent close to three times again that amount (116,623 ducats) to enlarge it to its present size.<sup>48</sup> Not only was the rental value of a palace very low, but its resale value was hardly equal to its cost. The enormous structures, in short, were utterly nonproductive as investments.

Although these palaces are to be seen as one of the most flagrant examples of conspicuous consumption by the bourgeoisie that Europe had known up to that time, the intellectuals, the so-called civic humanists, were nevertheless able to find a higher social justification for all this private energy. Above selfish interests there was the common good, and the purely selfish objective could be justified by regarding the total effect such generous and magnificent expenditures had for the embellishment of the city as a whole. It was Palmieri's view that "although made by individuals it is nonetheless better to treat [such buildings] as communal utilities rather than as private comforts, because they are very important for the universal ornament of the city and they compose the beauty of the city."<sup>49</sup> One could say that this is nothing but the medieval virtue of largesse now given a purely esthetic outlet. Most palaces were self-contained projects, but at least a few were planned more dramatically, in a setting that reached out beyond the buildings themselves to reorganize the immediate vicinity. For instance, we know now that there

<sup>47</sup> ASF, Carte strozziane, ser. 3, no. 177 ("Scritture spettanti alla compera del Palazzo de' Pazzi, fatta dal sig. Lorenzo Strozzi"), fols. 78, 102r-103r, 112r-13v, 117r-18r, 152: The statement that because of "la nobiltà del sito et bellezza che è et ancora altre cose lo stimi più scudi 2000, che tutti fanno scudi 8000," is found alone at the top of fol. 119r; and considering its location, the hand, and the way these documents are bound together, I have linked it with fols. 102r-103r, where the assessment made on the basis of the rental value of the property occurs.

<sup>48</sup> Frank Büttner, "Der Umbau des Palazzo Medici-Riccardi zu Florenz," *Mitteilungen des Kunsthistorischen Institutes in Florenz*, 14 (1970): 394-95.

<sup>49</sup> "Questo sarebbe posta in narrare del commodo e ornamento dello splendido vivere, dove si comprende le case magnifiche, gli edifici in publico fatti, le masserizie abbondanti, i famigli, cavalli, e altre cose che più tosto per bellezza di vita che per nostro bisogno s'appetiscono, cercano e tengono. Tali cose benchè da particolari sieno fatte, nientedimeno perchè sono atissime all'universale ornamento della città e fanno la bellezza civile, della quale seguita grandezza, stima et utile civile, più tosto si convengono trattare infra l'utilità commune, che infra i privati commodi, faremo dunque fine a' privati commodi, e diremo di quegli essere a bastanza detto, e per l'avenire diremo dell'utilità comuni, cioè di quelle che all'universale corpo della città e tutta la repubblica s'appartengono." *Della vita civile*, 154.

were plans for a large square next to the Medici palace on which Brunelleschi's church of San Lorenzo also would have opened.<sup>50</sup> Similarly Filippo Strozzi's plans to build his huge palace included widening the street, opening a piazza, and possibly incorporating the façade of a nearby church in which he had already shown considerable interest. At any rate (if we are to believe his son) Strozzi was able to plan for such an impressive structure in the first place only by playing up to Lorenzo de' Medici's expressed desire to have the city adorned with such beautiful and magnificent buildings.<sup>51</sup> The great Pitti palace, now expanded almost beyond recognition of its original appearance, was deliberately perched on high ground with a piazza to set it off; and the success of this plan is evident in all the early views of the city, where it indeed appears "more resplendent even than Monte Morello," to quote a contemporary bit of poetry on the subject.<sup>52</sup>

Our knowledge of these few projects, however, is vague; and they were at any rate the exceptions. Most palaces were inserted into the medieval city freed from the earlier communal controls for orderly town planning and without the opening up of vast public spaces characteristic of later urbanistic ideals. The beauty of the urban scene in Renaissance Florence was not (and still is not) found in large squares and public spaces, and certainly not in unitary schemes imposed on the city by either communal or princely authority, but in the collective impression of numerous self-contained private efforts.<sup>53</sup> That impression, however, is not without its own internal coherence. The new palace style did not represent, as has been claimed, "an esthetic of maximum individuality" (which could perhaps be more appropriately said of a modern American city).<sup>54</sup> Indeed, architectural historians have always been impressed with what they call a strong conservatism in Florentine palace style, a point that is especially telling in considering the utter failure of the most original of them all, Alberti's palace for Giovanni Rucellai, to find any imitators; or the failure of Brunelleschi's for Cosimo de' Medici to get built at all; or the discrepancy between the fantasy of architectural settings in painting and the reality of the city's streets. This conservative taste—albeit with considerable variety within the bounds of its canons—may in fact have been a subconsciously imposed upper limit to the competitive egotism that these palaces represent, so that despite the lack of urbanistic schemes a peculiar stylistic unity emerged, however fortuitously, from all this private and unregulated activity.

<sup>50</sup> Carol Herselle Krinsky, "A View of the Palazzo Medici and the Church of San Lorenzo," *Journal of the Society of Architectural Historians*, 28 (1969): 133-35. For an unrealized early sixteenth-century project for a square, see Alessandro Parronchi, "Due note," *Rinascimento*, 8 (1968): 358-59.

<sup>51</sup> Lorenzo Strozzi, *Vita di Filippo Strozzi*, 22-28.

<sup>52</sup> "Di Luca Pitti o visto la muraglia che sfavillava più che mongibello"; see Emilio Pasquini, "Il codice di Filippo Scarlatti," *Studi di filologia italiana*, 22 (1964): 429.

<sup>53</sup> In this respect the comments of Francastel ("Imagination et réalité") are very suggestive.

<sup>54</sup> J. S. Ackerman, *The Architecture of Michelangelo* (New York, 1961), 76.

At any rate, Florence (after Venice) may well have been the first city in Europe where the individual private residence comes into its own as the distinctive feature of the urban scene, although palaces elsewhere in Italy have been so much less studied than those of Florence that any generalization on the subject is difficult to make. Here a distinction must be made between private bourgeois residences and those of princes and courtiers, of popes and prelates. Venice, of course, had an even older tradition of spectacular palace building, but the palace there definitely served both a commercial function and a rather more extensive social one as the residence of a larger family group (the *fraterna*). On the other hand, fifteen-century Genova, another city with a wealthy mercantile aristocracy, was still very medieval in its appearance; and the elevation of domestic architecture there to a notable art form does not get underway until well into the sixteenth century.<sup>55</sup> At any rate, the public display of private residences is certainly not to be found in the great cities of Northern Europe at the time—not in aristocratic Paris, where throughout the entire Renaissance into the seventeenth century the town house fails to make the public appearance it does in bourgeois Florence in the fifteenth century,<sup>56</sup> and certainly not in contemporary London, where street frontages of great houses were rented out as tenements.<sup>57</sup>

JUST AS THE FAÇADE of the palace as a public monument embodies the public and civic implications of a new individualistic morality, so the living arrangements behind the façade reflect the changed social conditions underlying that new morality—the withdrawal of the family into a world of privacy. It has been observed that inside a Florentine Renaissance palace the windows are so high that simply to look out from within one has to climb up steps in the window bays to peer over the high sills; such was the exclusion of the outside world. What kind of family was it, then, that enjoyed this privacy? The rationale for building the palaces in the first place was partly at least a desire to express the nobility of the individual patrician, to give vent to his proud ego in a public monument; but when he withdrew within, it was to enjoy its privacy with his family. That is to say, as a residence the palace housed primarily the owner's immediate family—his wife, his children, perhaps a stray unattached relative here and there, a widowed mother, for example

<sup>55</sup> Ennio Poleggi, *Strada nuova: una lottizzazione del Cinquecento a Genova* (Genova, 1968), ch. 1; Jacques Heers, *Gênes au XV<sup>e</sup> siècle* (Paris, 1961), 197.

<sup>56</sup> "Nous sommes bien loin de la masse imposante du palais italien en quadrilatère, îlot seigneurial qui s'impose du public par la majesté de son architecture externe." Jean-Pierre Babelon, *Demeures parisiennes sous Henri IV et Louis XIII* (Paris, 1965), 120. On domestic architecture in late medieval Paris see Simone Roux, "L'Habitat urbain au Moyen Âge: le quartier de l'Université à Paris," *Annales: E.S.C.*, 24 (1969): 1198-1219.

<sup>57</sup> C. L. Kingsford, "On Some London Houses of the Early Tudor Period," *Archaeologia*, 71 (1921): 17-54; Kingsford, "A London Merchant's House and Its Owners, 1360-1614," *ibid.*, 74 (1925): 137-58.



—and, originally at least, it was certainly not the residence of the larger extended family or clan. Even in those cases where the palace was built by a father for his sons, or by brothers, the building was distinctly divided into the appropriate number of parts, usually with separate entrances at the street level.<sup>58</sup> The privacy of a man's home meant not only withdrawal from public life but also detachment from most relatives who were not members of his immediate family.

That these palaces were built primarily for smaller family groups emerges from the history of their occupancy and ownership through the generations immediately following that of their builders. Here much can be learned from the history of private wealth, especially from the settlement of estates and the disposition of palaces at each juncture as they passed to successive generations. Since at least as early as the mid-fourteenth century it was usually the case in Florence after the death of a father that estates were eventually divided among his sons. It did not always happen immediately—indeed, legally it could not be done until the youngest son came of age—but one senses that the division could not be put off for long. Anyone who has read through the numerous testimony of private books of accounts and *ricordanze* is familiar with this process of disintegration of patrimonial wealth with each successive generation, a process that resulted in what one might call the individualization of wealth in contrast to what seems to have been the corporate nature of patrician fortunes of an earlier era. The phenomenon is obviously related to the dissolution of the *consorteria* and the emergence of the conjugal family with its own private wealth distinct from that of even the closest of relatives. It goes without saying that such division of estates could create financial problems even for the heirs of rich men. Gino di Neri Capponi, for instance, advised his sons to put off the division at least until their financial condition would permit;<sup>59</sup> but his very concern reveals a basic fact of patrician society: the old economic bonds were broken and it could not be taken for granted that patrimonies could be shared by larger family groups.<sup>60</sup>

Periodic division so characterized the history of family fortunes that one feels that by the fifteenth century individual private wealth had become an essential condition of a man's happiness and that to preserve peace in the family it was probably even necessary eventually to divide

<sup>58</sup> The Strozzi and Busini palaces were designed as two separate residences. Palaces with two separate entrances include the Corsi (via Maggio, 50/52), the Velluti-Martellini (via Maggio, 9), the Rustici (via de' Rustici) and the Machiavelli (via S. Spirito, 5/7); the Strozzi has three (and within there were three separate households. See Uffizi drawing 1561A, reproduced in Pampaloni, *Palazzo Strozzi*, 57).

<sup>59</sup> "Da soffrire è lo stare insieme un pezzo, tanto che abbiate il modo a dividervi con unità, a che abbiate megliorato condizione." *Rerum italicarum scriptores*, ed. Lodovico Antonio Muratori (Milan, 1723-51), 28, col. 1150. The same concern is found in the testament of Francesco Sassetti, Warburg, "Le ultime volontà," 226-30.

<sup>60</sup> For elaboration of this thesis, see Richard A. Goldthwaite, *Private Wealth in Renaissance Florence: A Study of Four Families* (Princeton, 1968).

a patrimony among brothers so each could have his own estate. We have at least one explicit expression of such a sentiment in the *ricordanze* of Ugolino di Niccolò Martelli, an important partner in the Medici business enterprises.<sup>61</sup> He and his eight brothers made no division of their patrimony after their father's death in 1425, partly because several of them were still minors, partly because some of them were living abroad; but with time the pressures mounted. In 1440, probably because the youngest, Alessandro (b. 1417), had recently come of age, two of the brothers, Francesco and Messer Domenico, took their shares of the patrimony and went their own way; and five years later another, Giovanni, did the same. In 1451 the remaining six divided into two groups—Ugolino, Bartolomeo, and Martello on one hand; Antonio, Roberto, and Alessandro on the other—and made a property settlement, "which division," wrote Ugolino, "was prompted by Antonio and us in order not to be disunited and . . . to conserve us in peace."<sup>62</sup> They divided virtually everything (except certain military gear, which was to be available to any brother who might take public office) down to the sheets and kitchen utensils, and the inventories were written up in the presence of their mother. "We remain content with this," continues Ugolino after having made a complete copy of the inventories; "I first sought every opportune remedy in order not to make this division so that no harm should come to our family universally in all things, but for peace and unity among us we made it."<sup>63</sup> For their part, Ugolino and Bartolomeo kept their property in common—in 1451 Martello was away in Brittany, and in 1455 he made a final settlement with them—and in 1457 the two were living together with their large families.<sup>64</sup> Yet by the time Ugolino died in 1484 he had a completely private estate, no part of which was shared with any of his brothers; and hardly a year passed before his estate in turn was completely divided among his surviving sons.<sup>65</sup> As Ugolino clearly recognized, the family could stay together only as long as they did not have to share together, as long as each member had his private wealth and was free to go his own way.

This development in the history of the family obviously has profound implications for the history of the home. With the inevitable division of estates as they passed from one generation to the next and the forma-

<sup>61</sup> ASF, Carte strozziane, ser. 5, no. 1461 (*ricordanze* of Ugolino di Niccolò Martelli, 1432–82). References to the property divisions discussed below are fols. 26v–30r, 83r, 65r–70r, 71r.

<sup>62</sup> "La quale divisa fu mossa d'Antonio e noi per non istare disuniti e a presso perchè eravamo multiplicati in famiglie e per chonservarci con buona pace." *Ibid.*, fol. 65r.

<sup>63</sup> "E noi rimanemo chontenti. Ciercho prima ogni oportuno rimedio di non dividere perchè ne seghuitava danno della chasa universalmente in ciaschuna chosa, ma per la pace e unità di noi lo faciamo." *Ibid.*, fol. 69r.

<sup>64</sup> ASF, Catasto, no. 823 (Leon d'oro, 1457), doc. 181 (declaration of Ugolino and Bartolomeo).

<sup>65</sup> Ugolino left six sons. One of these, Carlo, had already received his share of the patrimony before his father's death; references to the subsequent division among the other five are found in their books of accounts and *ricordi*: ASF, Carte strozziane, ser. 5, no. 1467 (Messer Ludovico), fol. 89v; no. 1466 (Carlo), fols. 123r, 127v, 128r; no. 1463 (Luigi), fol. 114r.

tion of completely private wealth for the individual patrician, he tended to establish his own living arrangements by withdrawing with his immediate family into a private household. It was not altogether unusual for married sons to live with their father, even when they had their own families; but as soon as the father died, they more often than not sought their own homes. The history of the occupancy and ownership of palaces reveals what seems to be their natural tendency to remain single household dwellings.

In the division of estates, therefore, a family palace could present very special problems. At the same time that there was a tendency for heirs to divide all wealth and to establish independent living arrangements there were pressures to maintain the family residence as a prestigious property that rightfully should belong to them all. It was in fact not infrequent that a testator declared his palace indivisible ("per non diviso") and inalienable. In cases where such strictures were observed the palace could remain the shared property of heirs even beyond one generation, and ownership could thus be fractured into a number of shares. In this way after a couple of generations a palace could become hopelessly divided and would have to be sold. When in 1311 the Strozzi bought two  $\frac{1}{18}$ , two  $\frac{1}{36}$ , and one  $\frac{1}{6}$  shares of the same house, each segment was acquired from a different member of the Mazzinghi family; and in 1326 the Strozzi bought another  $\frac{1}{4}$  share.<sup>66</sup> In the mid-fourteenth century the Medici bought first nine and then eleven of the twenty parts of a house on the via Larga;<sup>67</sup> when the Busini palace was sold in 1473 (to another branch of the family) it involved four transactions for purchase of two  $\frac{1}{8}$ , one  $\frac{1}{4}$ , and one  $\frac{1}{2}$  shares;<sup>68</sup> and when the Medici bought the Pitti palace in 1550 the sellers consisted of seven parties representing thirteen individuals.<sup>69</sup>

It is certainly not at all clear that in these cases of multiple ownership the palace actually became the residence of all the owners; but in the fifteenth century when ownership of a palace was divided it could very well mean that if the owners intended to make their residence there, the palace would be physically divided into separate homes. Entries in the *ricordanze* to this effect are not infrequent: there are descriptions of the division and sometimes lists of the resulting remodeling expenses, so there is no doubt about actual physical division.<sup>70</sup> In 1476 Jacopo and

<sup>66</sup> ASF, Carte strozziane, ser. 3, no. 191, fols. 4r-5v (seventeenth-century copy of records of purchase); cf. p. 980 above.

<sup>67</sup> ASF, Medici avanti il principato, no. 152 (*ricordi* of Foligno di Conte d'Averardo de' Medici), fols. 40v-41r.

<sup>68</sup> ASF, Catasto, no. 1005 (Leon nero, 1480), fols. 67-68; this today is the Bardi palace in via de' Benci.

<sup>69</sup> Francesca Morandini, "Palazzo Pitti, la sua costruzione e i successivi ingrandimenti," *Commentari*, 16 (1965), no. 1-2: p. 44 n. 2 (where purchase document is published).

<sup>70</sup> For example, in 1459 Bernardo Rinieri and his cousin Luca made a notarized division of their house in via del Cocomero (today via de' Ricasoli), with the former getting three-fourths of it and the obligation to build a dividing wall; ASF, Conv. supp., 95, no. 212 (accounts and

Priore di Messer Giannozzo Pandolfini, who had been left without residences after the division of the estate of their very wealthy father (who had died in 1456), bought a large house; but in the presence of a notary they made an agreement by which they very carefully divided it into two separate parts, splitting the entrances, stairways, stables, and two *necessari*, building a dividing wall and adding a second well, so that each half would be a completely separate residence.<sup>71</sup> Likewise in 1522 Cristofano di Bernardo Rinieri made an entry in his book of *ricordanze* of how he and his nephew divided their palace at a cost of 410 florins, and he included all the detailed instructions given to the builders about how new walls were to be built to allow each residence complete separation from the other, even without a means of direct communication between the two.<sup>72</sup> Yet to judge from other entries in the same source Rinieri and his nephew were on the best of terms; they simply wanted private households. In 1516 two grandsons of Pierfrancesco de' Medici, Giovanni di Giovanni and Pierfrancesco di Lorenzo, divided a palace by lot after engaging two builders to make the division and estimate remodeling costs. The entrance way went entirely to one part and so did the garden; remodeling expenses included walling up doorways, dividing a large room on the piano nobile, and enclosing one side of the courtyard so that each part was completely sealed off from the other.<sup>73</sup> When this kind of division occurred each part of the palace was henceforth considered a separate property. Hence the Del Pugliese palace in the sixteenth century was actually owned by two different families;<sup>74</sup> and likewise in the seventeenth century the great medieval palace of the Spini, which had long been broken into apartments, was divided between

*ricordi* of Bernardo di Stoldo di Luca di Piero Rinieri, 1457-1503), fol. 155r ("Giudichomi detti entrata della via del Chochomero e una cortte con 1° pozo e 1° necessario era alla mia chamera terrena andassi in quel modo sino al tetto, e io avessi a fare il muro della trameza di 1/2 braccio"). Buonaccorso Chelli (or Serchelli) likewise described the work done in his house in via Maggio when he and his brother Piero divided it in 1479 ("ognuno di noi ispendemo in chasa per istare ognuno di per sè. Facciamo certi achoncini in chasa chome si vede"). ASF, Montalve di Ripoli, S. Piero a Monticelli, no. 185 (accounts and *ricordi* of Buonaccorso di Leonardo di Piero Serchelli, 1476-1507), fol. 12r.

<sup>71</sup> Ospedale degli Innocenti (Florence), 149, no. 648 (accounts and *ricordi* of Jacopo di Messer Giannozzo d'Agnolo Pandolfini, 1467-87), fols. 166r-68r. This house, purchased from Francesco Sassetti for 2,500 florins, was on the via del Proconsolo, across the street from the Pazzi palace.

<sup>72</sup> "Richordo chome questo dì viiii di maggio 1522 noi Andrea di Francesco Rinieri mio nipote e io Christofano di Bernardo Rinieri abbiamo fatto insieme chompromesso per dividere la chasa di Firenze che al presente abitiamo a chomune; e detto Andrea à chiamato Bernardo Pistochi muratore e io ò chiamato Antonio Pilacchi muratore, i quali anno a fare 2 parti di detta chasa e anno tempo tutto dì 24 del presente mese, le quale due parti anno a paregiare, e quella valessi meno anno a rifare chon danaro; e quando aranno fatto dette parti se sarenò d'achordo a pigliare quello arà a rifare denari; ne à avere tempo mesi xviii prossimi futuri allora paghando ongni mesi 6 la terza parte insino al'intero paghameto"; the detailed instructions follow. The house was in via de' Ricasoli. ASF, Conv. sopp., 95, no. 220 (*ricordanze* of Christofano di Bernardo Rinieri, 1496-1553), fols. 31v-33v. Cf. n. 70 above.

<sup>73</sup> ASF, Medici avanti il Principato, no. 86, doc. 138 (a copy of the builders' report). An inventory of the contents of this palace also survives. *Ibid.*, no. 104, doc. 19 (in the archival inventory the two parties, Giovanni and Pierfrancesco, are incorrectly identified as brothers).

<sup>74</sup> *Illustratore fiorentino; calendario storico*, 1907, pp. 9-12.

two families (and one-half of it was further divided into three separate residences).<sup>75</sup>

The fact is, however, that most palaces were probably not actually subdivided either legally or physically with the passing of the generations. In Siena a great palace like that of the Tolomei remained the shared property of all members of the vast clan right up through the eighteenth century; by the thirteenth century there already were as many as 120 shares.<sup>76</sup> In Florence ownership was less diffuse. A man's grown and married sons might live with him in his palace, which according to his testament was to remain the common property of them all, but following his death, and sometime after the legal settlement of the estate, the usual arrangement was a private agreement that turned the palace over to only one of the sons and required the others to move out and find homes of their own. When Ilarione and Andrea di Lippaccio de' Bardi divided their patrimony in 1430, they should have excepted the sizable palace if they were to honor their father's instruction. They thought about dividing it into two households, but they found it could not be done conveniently without ruining the entire structure, despite the fact that during the immediately preceding period when they were living in it together they had spent over 800 florins for improvements; and so it passed to Ilarione alone.<sup>77</sup> When Ugolino di Niccolò Martelli died in 1484 his patrimony was divided immediately, and this included partition of two palaces among six sons. The family palace in the via Martelli was to be shared by five of them, but within a few months Cosimo and Luigi had bought out the shares of the other three, and by 1487 Luigi bought Cosimo's half and thus came into possession of the entire palace.<sup>78</sup> Meanwhile the other palace (today the Antinori palace), which Ugolino had acquired for his sons, was likewise bought up by Carlo alone.<sup>79</sup> Perhaps the best example of the businesslike way in

<sup>75</sup> Biblioteca Nazionale di Firenze, MSS Tordi, no. 523 ("Quaderno della muraglia che io Geri Spini farò nelle mie tre case del palazzo delli Spini per ridurle a una per mio uso," 1606-14; Spini possessed one-half of the palace). There are some miscellaneous papers attached to this document that trace the occupancy and ownership of the palace; see also Luigi Passerini, "Il palazzo Spini," in his *Curiosità storico-artistiche fiorentine* (Florence, 1866-75), 2: 63-89.

<sup>76</sup> G. Prunai, G. Pampaloni, and Nello Bemporad, *Il palazzo Tolomei a Siena* (Florence, 1971), 81-84.

<sup>77</sup> "Anchora veduto e ritrovato che il detto Andrea e Ilarione tenevano e possedevano chomunemente e per non diviso uno palagio grande chon due orti nel quale amendue abitavano . . . e perchè il detto palagio non si potrà comodamente dividere che non si ghustasse però ongni parte del detto palagio a vero overo à nel detto palagio è tutto e intero, il detto palagio al detto Ilarione agiudichiano e a esso Ilarione dicierniamo apartenersi per piena ragione di dominio e proprietà, avere tenere e possedere e ciò che al detto Ilarione e alle sue rede da quinci innanzi piacerà perpetualmente di fare." Today Palazzo Canigiani, via de' Bardi, 28/30. The palace had been bought in 1414 for 2,200 florins; when the division of the patrimony was made it had a value of Fl. 3,039 3s. 3d. ASF, Conv. supp., 79, no. 119 (accounts and *ricordi* of Ilarione di Lippaccio de' Bardi, 1420-31), fols. 255v-57v, 172. Cf. above, p. 993.

<sup>78</sup> ASF, Carte strozziane, ser. 5, no. 1463 (accounts and *ricordi* of Luigi d'Ugolino Martelli, 1484-88), fols. 12, 114r, 119v-120r, 122r, 125v-126r. The palace, which had a value of 2,000 florins *di suggello*, was in via Martelli; cf. above, p. 999.

<sup>79</sup> ASF, Carte strozziane, ser. 5, no. 1466 (accounts and *ricordi* of Carlo d'Ugolino Martelli,

which brothers could settle the problem of commonly owned palaces is the case of the five sons of Antonio di Leonardo Gondi. They lived together for twenty years after his death in 1486 until the youngest reached his majority in 1506 and the patrimony was finally divided. When it came to the palace the brothers drew up a written agreement by which they recognized that the house would remain in the possession of two of them while the other three were to move out; and since obviously the three could not move out immediately further arrangements were made and again there was a written agreement, according to which the three were to share expenses at specified rates and pay rent until such time as they could find new quarters. Within only a few months, however, all three had left, while Antonio, one of the two remaining brothers, took up what was to become a permanent residence in France. The family palace therefore remained in effect the single family dwelling of Alessandro. Finally there is the case of the five Guicciardini brothers, who shared ownership if not residence of their father's palace for about eleven years after his death; but when Girolamo married in 1524 it was, significantly, one of the stipulations of his bride that he have the palace for himself alone. Thus came the inevitable settlement, and the other brothers had to find homes of their own.<sup>80</sup>

The Florentine palace, then, was more often than not a residence of a single conjugal family. Despite the desires of testators to save the family residence from the inevitable division of their estates among sons, it usually ended up as the property—and the residence—of only one man; and by the sixteenth century, in some cases at least (the great Pandolfini palace, for example),<sup>81</sup> primogeniture became an explicitly recognized principle of inheritance of palaces. Hence the enlargement of the private family dwelling that these palaces mark in the history of domestic architecture was not a consequence of family growth. On the contrary, as we have already seen, the enlargement of the private family dwelling occurred precisely at a time when the communal clan had lost much of its cohesive force and the family was reduced to its minimum size. Perhaps we can say that the enlarged private dwelling was an ironic consequence of the dissolution of the family clan, inasmuch as that dissolution of the larger social group released the forces of individualism that led to such spectacular palace building. Likewise, the abandonment of the loggia, long the public symbol of the sociability of the clan, is an aspect of the phenomenon of the social fragmentation of the family. In short, the peculiar development of palace architecture in Renaissance

1490-94), fol. 127v; also *ibid.*, no. 1468 (accounts and *ricordi* of Luigi d'Ugolino Martelli, 1489-92), fols. 102v, 108r. Cf. above, p. 999. The palace had a value of between 3,500 and 4,000 florins.

<sup>80</sup> Goldthwaite, *Private Wealth*, 132, 157, 167.

<sup>81</sup> ASF, Carte strozziane, ser. 2, no. 116, doc. 2 (copy of act of donation of the palace by Giannozzo to his nephew Pandolfo d'Agnolo, 1520).

Florence reflects a fundamental transformation at the very base of Florentine society. Alberti, with all his nostalgia for a former style of family life (which he probably in fact never really experienced), sensed that something very profound and ultimately inexplicable was happening in Florentine society: "To make two families out of one requires double expense, and many things happen that it is easier to judge of by experience than by talk, easier to feel than to explain. Indeed, I am not pleased with this dividing of families, this going in and out of separate entrances."<sup>82</sup>

THE PALACE REPRESENTED a new world of privacy, and it was the privacy of a relatively small group. Further evidence for this proposition—and at the same time the best indication we have of the kind of private world that the Florentine patrician created for himself—comes from a closer inspection of the general disposition of space within the palace. Perhaps the most striking characteristic of the use of that space is the small part of the total cubic area incorporated in these vast structures that was available for actual residential use. If from the outside these palaces appear to spread over a vast area it must be remembered that inside an appreciable part of that area—perhaps a fourth, sometimes a third—was actually left open. The center of the palace and of life within it was the enlarged square or rectangular courtyard with open arcades on the ground level and usually an open loggia on the third, or top, level. As to their elevation, as high, as has been said, as a six- or seven-story modern office building, we again find the exterior appearance deceptive. For the most part there were actually only three floors, and of these the lower had little living space while a good part of the upper opened as a loggia facing onto the courtyard. Consequently the inhabited apartment consisted of not much more than a dozen or so rooms—if indeed even that many—mostly on the middle floor. This basic plan remains essentially the same regardless of the size of the overall structure, so that the largest palaces simply had larger rooms, not more of them. In other words, despite the massive block these buildings appear to be from the outside, there was not a correspondingly large residential space within at the disposition of the family. These palaces are indeed impressive for their sheer size if for nothing else, but they loom even larger when one realizes how deceptive their appearance is in disguising the relatively small internal area that was actually inhabitable. They were obviously designed to give a small family an extraordinarily spacious, private world of its own that extended well beyond their actual living chambers. The most innovative feature of the palace, in short, is perhaps best described

<sup>82</sup> *The Family in Renaissance Florence*, a translation of *Della famiglia* by Renée Watkins (Columbia, S.C., 1969), 86.

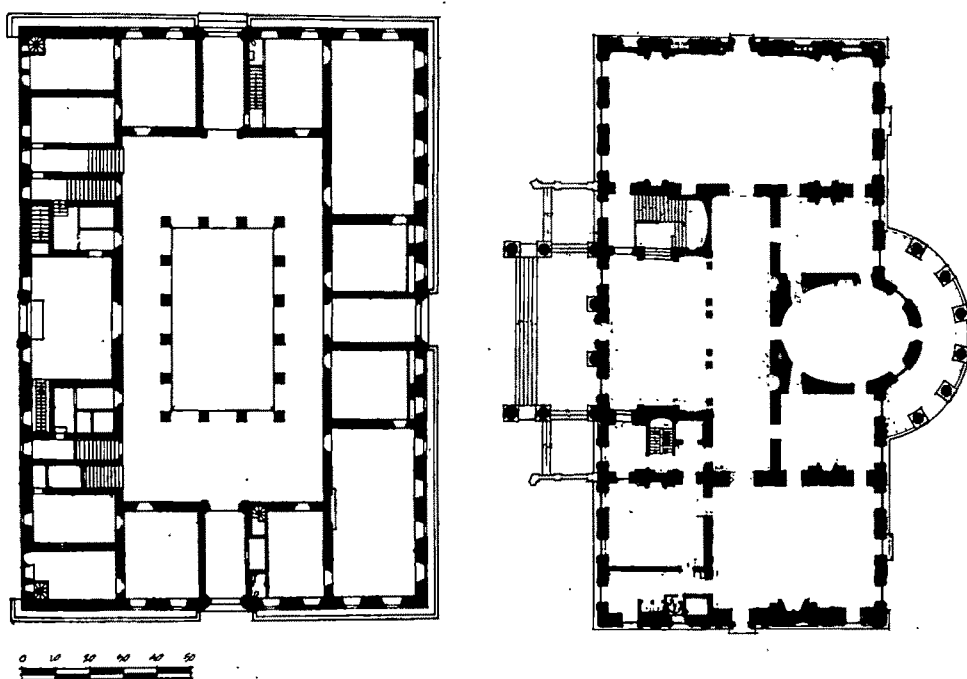


Fig. 8. The ground area of the Strozzi palace (left) (from Stegmann and Geymüller) is almost fifty per cent larger than that of the White House (right). The Strozzi palace is also almost twice as high (see fig. 6), but it has only three floors to the White House's four. The upper floors of the White House, with the president's private quarters, contain several dozen rooms laid out to the even smaller scale of twentieth-century living. Although the Strozzi palace is exceptional in being the largest palace to have been built in the fifteenth century and in having been designed for the two families of brothers, it illustrates clearly that a dramatic increase in scale did not change the basic plan for a palace. The number of rooms did not increase, but their size did—perhaps the clearest evidence we have that the function of these buildings was not to accommodate a highly complex patrician style of life. Rendering by Implementation, Baltimore.

as the luxurious inflation of private space around the nucleus of a relatively modest-sized apartment.

It is difficult to penetrate much further into the privacy of these palaces to determine the deeper social implications of living arrangements within. We simply lack the kinds of documentation that would lead us directly into the subject. Florence of course is exceptional for the survival of a remarkable quantity of personal diaries and letters, chronicles and histories, and imaginative and thoughtful literature written by her citizens from the fourteenth century onward; but personal household arrangements and the intimate relations of members of a family were not the subjects that Florentines very often wrote about. In all the vast literature we find only random remarks here and there that at best bear indirectly on the subject of the disposition of space within a palace. There are, for example, numerous contemporary descriptions of the Medici palace in the fifteenth century, but most of them are literary ventures occasioned by great quasi-public ceremonies and none men-



tions actual domestic arrangements in the Medici home.<sup>83</sup> Likewise Alberti's famous treatise, where there is much about domestic economy and familial affections, has little to say about practical household management that is relevant to understanding the internal organization of a palace.

Even the authors of the theoretical architectural literature on houses seem to have little sense of the physical arrangements and internal functions of a palace. In the great treatises by Alberti and Filarete, both Florentines of the fifteenth century, despite considerable interest in the higher social functions of buildings and the city as a whole, there is nothing about how the architect is to lay out the private palace for actual living other than very general advice on kitchens, stables, plumbing, storage, and so forth.<sup>84</sup> When it comes to arrangement of living quarters there is virtually nothing, and it is almost impossible to uncover the underlying assumptions of these theorists about the kind of life the family was to have within. In this respect, incidentally, their relatively slight interest in the private patrician palace may be related to the fact that although they were both born in Florence, they lived most of their lives away from the city, so that they really did not have Florence in mind when they wrote. When Alberti writes, for example, that the townhouse should have a chapel and that more attention should be given to the merchant's shops than to the beauty of the interior of his house, one can rightfully wonder whether Alberti was thinking about Florence.<sup>85</sup> He was, of course, more interested in discussing humanist ideals than in describing reality; but the irrelevance of much of what he has to say may also be evidence that the private bourgeois palace (as distinct from the princely palace with which he as well as Filarete concern themselves) was indeed a distinctively Florentine development during the early Renaissance and not yet a reflection of architectural practice elsewhere.

It is generally held that architectural theorists of the later sixteenth century broke the spell of humanist idealism, so that no longer was there the Albertian preoccupation with the city in the platonic sense, as the perfect abode of the perfect society. Yet for all their greater practicality with respect to urban planning, the treatises (or rather the notes for treatises) of the Florentines Bartolomeo Ammanati<sup>86</sup> and Giorgio Vasari il Giovane<sup>87</sup> show as little concern for practical living arrangements. Both

<sup>83</sup> Hatfield, "Some Unknown Descriptions of the Medici Palace in 1459," 240. A recent attempt to reconstruct the interior of the Medici palace has been made by Wolfer A. Bulst, "Die ursprüngliche innere Aufteilung des Palazzo Medici in Florenz," *Mitteilungen des Kunsthistorischen Institutes in Florenz*, 14 (1970): 369-92.

<sup>84</sup> Alberti, *Ten Books on Architecture*, especially bk. 1, ch. 9; bk. 5, chs. 14, 18; bk. 9. Filarete, *Treatise on Architecture*, ed. and trans. John R. Spencer (New Haven, 1965), bks. 11, 12.

<sup>85</sup> I know of only two Renaissance palaces with chapels: the Medici-Riccardi and that of Pierfrancesco de' Medici (see above p. 1001)—and the Medici, of course, are always a very special case. Chapels were added to many palaces later, however.

<sup>86</sup> *La città: appunti per un trattato*, ed. Mazzino Fossi (Rome, 1970).

<sup>87</sup> *La città ideale: piante di chiese (palazzi e ville) di Toscana e d'Italia*, ed. Virginia Stefanelli (Rome, 1970).

men were interested in houses for all strata of urban society, and both left actual designs; but apart from service arrangements there is no indication of the function of rooms. On Vasari's designs the terminology consists of *salone*, *sala*, and *camera*, but these are related to size not function; and the terms are no more precise than they are either in the fifteenth-century inventories or in the seventeenth-century dictionary prepared by Baldinucci. In short, the Florentine architectural treatises from Alberti to the younger Vasari give us no help in understanding the domestic uses of interior space; and the readers of what these men have to say about palaces might well remark, along with a critic of another era, that "tis very fine; but where d'ye sleep and where d'ye dine?"

This is not the kind of question that has even been raised by architectural historians; their interest in the practical problems of the organization of interior domestic space is as slight as Renaissance theory is irrelevant. Historians at best follow the theorists themselves in exalting the organization of space within these palaces for being based on certain rational principles that somehow allow the creation of a more purely human environment.<sup>88</sup> When historians get around to considering actual living arrangements, however, they are likely to dismiss them as completely inadequate and to explain that the motivation for building palaces in the first place was a desire to impress rather than to accommodate.<sup>89</sup> While it is not difficult to be somewhat put off by the inconveniences posed by these palaces for twentieth-century living one must remember that to ignore the question of practical domestic accommodations is to overlook the fact that however unplanned and inadequate these palaces may appear to us today they must nevertheless have conformed to the patrician style of life. It may in fact be the case that in this respect these palaces hold the key to an understanding of that life style. If architectural historians would concentrate more on the social realities of a building's function than on architectural theory, style, and techniques, some of the most innovative features of domestic architecture in the Renaissance might be uncovered, and at the same time we might learn something very significant about private aspects of patrician culture.

<sup>88</sup> "Quest'edificio [the Palazzo Medici] che può veramente dirsi la prima *Wohnhaus* moderna in Italia, non tanto per i criteri di comodità domestica cui risponde, ma proprio perchè tali criteri sono superati in un'invenzione, governata da una ragione puramente umane: e in questo Michelozzo è pianamente rinascimentale." Ottavio Morisani, *Michelozzo architetto* (Milan, 1951), 52. "The building [also referring to the Medici palace] is intended to absorb evenly the life of the patron in all its complexity, and there is nothing to prevent us from seeing this coordination of purposes as a result of the desire for uniform development of personality." Paul Frankl, *Principles of Architectural History: The Four Phases of Architectural Style, 1420-1900*, trans. and ed. James F. O'Gorman (Cambridge, Mass., 1968), 165. Frankl's call for the study of "purposive intention" has not been heeded by architectural historians of the Renaissance (although in various places Ackerman has hinted at its importance); and the sociologists have been as negligent in the study of domestic space (see, for example, Amos Rapoport, *House Form and Culture* [Englewood Cliffs, N.J., 1969]).

<sup>89</sup> "Naturally, these structures were built to be looked at more than to be lived in . . . and it is hard to imagine where one slept, washed, or found privacy." Ackerman, *The Architecture of Michelangelo*, 78. Cf. Giuseppe Samonà, "Palazzo," in *Enciclopedia italiana*, 25: 962.

The best prospect for penetrating the interior of these palaces and getting some sense at least of the physical organization of domestic space is the study of household inventories and the interior decorative arts. Schiaparelli's remarkable book was a first attempt at presenting a description of palace interiors based on inventories, but it is seriously flawed by its failure to consider various problems of his sources.<sup>90</sup> Nevertheless, it has yet to be followed by a more analytical study. Indeed most of the decorative arts—furniture, maiolica, tapestries—have remained largely untouched by scholarship, and virtually nothing has been done on them from the wider perspective of social history. It is quite clear, however—and not at all surprising, considering the implications of the enlargement of domestic space represented by the palace—that paralleling the evolution of domestic architecture there is a very notable development of the decorative arts. Palaces, after all, had to be furnished—and they were, in ways not at all possible heretofore. Earlier all kinds of domestic activities went on in the same rooms, and furniture tended to be simple in its forms and adaptable to different functions. By the fifteenth century, however, the inventories reveal quite clearly that rooms were beginning to have more specialized functions not only with respect to domestic activities but also with respect to their occupancy by specific members of the family. Furniture therefore became more appropriate to these functions. For example, storage furniture, such as chests, developed in a number of forms as it was increasingly necessary to have more space for the storage of the more numerous possessions of a society that consumed more luxury objects, and the chair evolved with respect to both comfort and form. The new style of private life found expression in these interior decorative arts, and it is in this period that such mundane objects as chairs, tables, chests, pottery, and even picture frames were raised at least to the level of the minor arts. In fact it was precisely this extraordinary demand for such possessions that gave birth to a vigorous Florentine artisan tradition whose importance for the total Florentine economy has never been properly assessed either for the Renaissance or for the continuing tradition of arts and crafts that is the economic basis for Florence's survival today. The social, economic, and artistic repercussions of palace building go much beyond their mere construction.

Despite the lack of documents and scholarship on the organization of interior space and on the decorative arts, one thing is clear: the palace represented a new world of privacy, and it was the privacy of a relatively small group. Although Alberti may have longed for the old sociability his sense of private family life was distinctly of his own time. The household he depicts in his dialogue on the family is that of a single

<sup>90</sup> *La casa fiorentina e i suoi arredi nei secoli XIV e XV* (Florence, 1908). Mario Praz's *La filosofia dell'arredamento* (Rome, 1945) is not without merit, but it lacks systematic analysis of style and function.

conjugal family, a self-contained social organism living in isolation and privacy and sealed off from the broader social and civic world outside its palace door. For Alberti privacy is the hallmark of family life. Not even servants intrude; and he has nothing to say about old and faithful family retainers who might have endeared themselves to their master and become virtually members of the family. When the one servant, Buto, appears it is not in the discussion of domestic life but at the beginning of book 4 as comic relief; and he certainly is not an utter dependent of the family. To judge from private books of accounts and *ricordi* and from household inventories, servants seem not to have been very numerous. There were perhaps not more than two or three for wealthy families, even those with large palaces. Moreover, the rate of turnover among servants could be quite high, employment sometimes lasting only a few months.<sup>91</sup> There is nothing in all this of the social expansiveness of the aristocratic family of the baroque age surrounded with its retainers and servants and playing out a sumptuous quasi-public social life on a truly palatial scale.

Within the isolation of the palace with its spacious privacy and increasing elegance, where relations were more secluded and less extended, where the family withdrew into itself, is it surprising that men found a keener appreciation of the values of domestic life? Alberti's book itself is the most attractive expression of this new sense of the home as an intimate relation among husband, wife, and children, but is not much of the culture of the Florentine Renaissance rooted in the new style of life being played out within the palace? The focus of that culture, in fact, is put on women and children, with the renewed interest in the education of children—merchant pedagogy, as it has been recently termed<sup>92</sup>—and with the remarkable rise in the status of women—for Alberti (at least in the dialogue on the family) the woman was a veritable *capofamiglia*, keeper of the keys, mistress of the household, and privy to almost all her husband's secrets. And the other woman in the lives of these patricians is invariably their children's wet nurse, on whom they lavished gifts and whose importance is to be measured by her prominence among the vital family statistics that constitute the private diaries of the period.

How else is one to explain the fascination, almost the obsession, with children and the mother-child relation that is perhaps the single most important motif in Florentine art during the first century of the Renaissance, with its *putti*, its children and adolescents, its secularized madonnas, its portraits of women. Works of art with these themes dominated

<sup>91</sup> In the mid-sixteenth-century census the population included a large number of servants (Battara, *La popolazione*), but few households had very many; in via Maggio only eight of twenty-seven households had more than two, and only three had more than five; and the two households in the great Strozzi palace had only five each. ASF, MSS, no. 179 ("Strade di Firenze"), fols. 210-11, 253r.

<sup>92</sup> Christian Bec, *Les marchands écrivains, affaires et humanisme à Florence* (Paris, 1967), 286-99.

the local art market; they were produced in great number and in the cheapest media to meet the increasing demand for them. What is their essential quality if not "the naive idealization of home life, the love for children and the pure cult of womanhood that speak to us from them,"<sup>93</sup> those very values that were being bred within the privacy of the family palace? The first point Cardinal Dominici makes about the education of children in his tract on the family written at the beginning of the fifteenth century (and dedicated to a woman) is that children should be surrounded with pictures of child saints and young virgins.<sup>94</sup> According to the cardinal a child's initial learning process is through the eyes, and the underlying assumption that this experience involves the child in a subject-object identification with pictures may have important implications for our understanding of the fascination with children in Florentine art. Is it possible to understand in these terms such developments in Florentine art as the juvenescence of formerly venerable older men like King David, who from bearded Old Testament king in the medieval tradition becomes the youthful symbol of the political vigor of the city,<sup>95</sup> or like Saint John the Baptist, heretofore represented as a hairy semibarbarian, who now sheds so many years in the course of a century that the patron saint of the city himself ends up being represented as a mere baby playing alongside the Christ child under the protective care of the Mother of God?<sup>96</sup> How else is one to understand the strikingly peculiar iconography of much of Florentine Renaissance art?

Perhaps it is nothing but a happily appropriate coincidence (for this argument) that the building inaugurating the Renaissance in architecture was Brunelleschi's Ospedale degli Innocenti, an orphanage for abandoned children; but it is certainly not coincidence that the interior arrangements were planned with the most remarkable care and sensitivity to make the buildings suitable to their function. Further study of the gradual evolution of the internal organization of the Innocenti as an orphanage will most likely reveal a number of innovative features designed to bring the life of the children more in line with the domestic temper of the times.<sup>97</sup> It was probably as original for its institutional organization as for its architecture. And if the new sense of domesticity and fascination with children surfaced to the level of public sensibilities

<sup>93</sup> Wilhelm von Bode, *Florentine Sculptors of the Renaissance* (London, 1908), 143.

<sup>94</sup> Giovanni Dominici, *Regola del governo di cura familiare* (Florence, 1860), 154.

<sup>95</sup> Charles Seymour, *Michelangelo's David. A Search for Identity* (Pittsburgh, 1967).

<sup>96</sup> Marilyn Aronberg Lavin, "Giovannino Battista: A Study in Renaissance Religious Symbolism," *Art Bulletin*, 37 (1955): 85-101.

<sup>97</sup> For the volume to commemorate the recent restoration of the Ospedale, A. Piccini has made the first step to reconstruct the functional organization of the buildings: G. Morozzi and A. Piccini, *Il restauro dello spedale di Santa Maria degli Innocenti, 1966-1970* (Florence, 1971). The documentation for the administration of this orphanage in the fifteenth century is considerable; and it merits study.

in the Innocenti, the same concern also may have penetrated the inner sanctum of the cloister in a most intriguing way—at least we might so judge from the testimony of Francesco di Tommaso Giovanni, a well-off property owner on the via Maggio. In 1452 he recorded giving his daughter, a nun in the convent of Monticelli, a wooden Christ child, with two changes of elegant clothes, three velvet hats, a tabernacle and altar, and “other little things for him.”<sup>98</sup> The mind boggles trying to imagine what Suor Angelica did with her doll and her Lord and his wardrobe in the sanctity of her convent, yet is this not to be seen as another of the varied manifestations of the fascination with children so characteristic of Florentine culture? Perhaps there is in part a demographic explanation, as David Herlihy has suggested, for the enhanced importance of the mother within the home and a consequent change in child-rearing practices.<sup>99</sup> But perhaps also the vast internal domestic space of the palace is to be understood ultimately as the objectification of what Eric Erikson has called the spatial mode of woman, so that by heightening her “sense of vital inner potential” and opening her life to those “modes of activity which include and integrate her natural dispositions,” the wider stage of domestic life brought altogether new cultural forces into play.<sup>100</sup> At any rate we shall very likely need more psychological penetration into the palace and into domestic life if we are ever to understand much of Florentine civilization.

IN A SENSE the Florentine palace sums up a civilization. Representing the new esthetic consciousness of the patrician and at the same time reflecting the changed conditions of family life that accompany the social transformations of the fourteenth century, the palace is the expression of Renaissance individualism both as a monument to its proud builder and as his castle where he could escape into the luxurious privacy of domestic life. And the furnishings with which he enjoyed that privacy, from pottery to religious pictures, reflect the values of domestic privacy and intimacy that he cultivated within. If the Renaissance begins to fade away in the later sixteenth century, so in fact did the palace have to adapt to new conditions and functions. In fact perhaps nothing better brings home the points I have tried to make about Renaissance palaces

<sup>98</sup> ASF, Carte strozziane, ser. 2, no. 16 bis (*ricordi* of Francesco di Tommaso di Francesco Giovanni, 1444–58), fol. 16r: “Ricordo che insino a dì . . . di giugno 1452 donai a Suora Angelicha mia figlia monacha in Monticelli al lato alla Porta di San Piero Gattolini 1° bambino di legno con 2 veste, 1° di chermise piano con fermiglio di perle e 1 di velluto allesandro con fregi d'oro, e 3 berette di velluto chermise e 1° ghirlanda di frangia rossa grossa e 1 tabernacolo di legno dipinto e con 1 altaruzo e con pali d'altare e veli e altre cosette per lui; avemolo da mona Lapa de' Damiani da Pisa el quale donò alla Mea dicendo volendo per dare a Suora Angelica.”

<sup>99</sup> David Herlihy, “Vieiller à Florence au Quattrocento,” *Annales: E.S.C.*, 24 (1969): 1338–52.

<sup>100</sup> “Womanhood and the Inner Space,” in Erikson's *Identity, Youth and Crisis* (New York, 1968), 261–84.

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## The American Civil War Considered as a Crisis in Law and Order

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PHILLIP S. PALUDAN

DESPITE THOUSANDS OF VOLUMES written about the Civil War we still know almost nothing about one of the central questions that that struggle poses: why did men rush to fight for their endangered country? Indeed why did they believe that secession endangered it? We know with some precision why the South seceded. The answer is obvious at first glance and remains clear upon deeper investigation—the South seceded because it saw in the election of Lincoln a threat to the survival of slavery, the foundation for the Southern way of life. Tradition, psychology, and economics all spoke clearly the same message—without slavery we cannot survive. And so secession came.

But a description of the decision for secession is not a description of why the war came. Although the South prepared for war there would be none unless the North contested the Southern action. Lincoln's assertion that "both parties deprecated war; but one of them would make war rather than let the nation survive; and the other would accept war rather than let it perish,"<sup>1</sup> is imprecise and thus misleading. The decision to make war for the Union was made not in Richmond or Montgomery but in Washington, and Boston and New York, and Indianapolis, and Columbus, and Springfield. This decision was made by Lincoln and by thousands of men who throughout the secession winter and especially after Sumter rushed to the colors. To understand why the war came we must look not at secession but at the Northern response to it.

In that response, as we shall see, the concept of law and order loomed large. The concept is a complex one, contemporary political rhetoric to the contrary notwithstanding. The order of a society depends on more than the rigid enforcement of all of its laws. It depends on maintaining an enduring consensus about a people's fundamental goals and beliefs, and hence on the success of the institutions created to secure this consensus. Fundamental

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<sup>1</sup> Abraham Lincoln, "Second Inaugural Address," in *Collected Works of Abraham Lincoln*, ed. Roy P. Basler, 8 (New Brunswick, 1953): 332.

to social stability is the family with its role of passing on ideals and attitudes about the nature of man, the relationship between man and God, and the duties, roles, and responsibilities of individuals to themselves and to others. In inculcating these ideas the family is supported by a whole environment including schools, churches, and informal and formal groups. Together these social institutions engender a collection of usually unexamined beliefs that act as society's silent, internal police. They say, "You must," "Thou shalt or shalt not," or "This is not done," and they operate wherever men carry their consciences.<sup>2</sup>

Although order depends most fundamentally on these social institutions, any society that advances beyond a primitive level requires the creation of a political order and of institutions that secure it. These institutions provide the means for resolving intergroup conflicts, for utilizing the power of government to achieve the ambitions of society, and for preserving whatever freedom its citizens demand. As the political theorist Carl Friedrich points out, political order is "a term suited to designate the political situation of the community in which component parts, or units, are arranged in such a way that the actions required for the attainment of the purposes of the community will be taken."<sup>3</sup>

In discussing the issue of law and order in the Civil War era it is useful to take Friedrich's definition as a focus. Specifically, order in the nineteenth-century United States refers to a condition in which the people were convinced that those institutions were secure that stabilized the protean nature of their society, restrained the potential for conflict in an environment that encouraged avarice, harmonized the diversity of opinions and influences fostered by a free society, and gave them a voice in determining their future. It is my contention that secession and the firing on Sumter provoked a crisis in which all these things seemed threatened and that, because of the widespread involvement of Northern citizens in the creation and maintenance of legal and political order, saving the Union was not merely an abstract issue but a matter of compelling personal concern.

An investigation of the reasons for this intense personal interest in the survival of the Union reveals serious shortcomings in existing descriptions of the Northern decision to fight for the Union. The traditional way that historians have looked at the reasons the North went to war has been to ask "What did 'Union' mean to Northerners who fought for it?" Thus we have studies of the idea of Union by Paul Nagel and of American nationalism by Merle Curti, Hans Kohn, and Yehoshua Arieli. All of these works describe in one fashion or another the ideas people had about the United States that made them willing to fight to keep it united. Through these

<sup>2</sup> J. O. Hertzler, *Social Institutions* (Lincoln, Nebr., 1946); James S. Campbell, Joseph R. Sahid, and David P. Stang, eds., *Law and Order Reconsidered: Report of the Task Force on Law and Law Enforcement* (New York, 1970), 3-11.

<sup>3</sup> Carl Friedrich, "The Dialectic of Political Order and Freedom," in Paul J. Kuntz, ed., *The Concept of Order* (Seattle, 1968), 346; see also Hertzler, *Social Institutions*, 113-24.



works we discover the many images that came to Northerners when they said "Union" or "United States." Northerners visualized a large, prosperous, magisterial land whose continued success depended on unity. They saw the nation as "the last best hope" of democracy in the world, a country with a mission to demonstrate that a nation "dedicated to the proposition that all men are created equal" might work. They believed that here individualism and nationalism were mutually agreeable, not in conflict; that the nation did not submerge but rather expanded the individual by maximizing his opportunities to achieve his dreams.<sup>4</sup> Such inquiries are thoughtful analyses of the issue. But they share what I believe are enfeebling faults. First, insofar as they deal directly with the question, they analyze why the common man went to war by reporting what intellectuals wrote about an idea of national union. Second, they focus their studies of nationalism in the United States too much on the nation as a whole and insufficiently on local experience.<sup>5</sup>

The first of these faults is the least serious. There are, of course, obvious differences between the attitudes of Emerson, Lowell, Hawthorne, and Melville and those of the average American of their time—differences in values seen in the criticism that these men leveled against the excessive materialism of the years from the 1830s to the 1860s. Still, both intellectuals and common people shared a common experience, and while some intellectuals might deplore and common people revel in the potentials for acquisition, we may see the fact and dimensions of materialism in the culture of the time. Emerson's observations of that world are expressed in language more refined than the common man might use, but the crude materialism of Emerson's world is not altered by his intellectual approach. Yet the concept of the Union held by intellectuals is really only secondhand information on why those who fought, fought. Without study of a vital aspect of the daily experience of the future soldiers—their daily involvement with the processes of governing—we should suspect the adequacy of such observations.

The weakness that arises from too nationalistic a focus is that the nation these historians describe was a federal union of states with a national government singularly inactive, a prevailing constitutional philosophy that

<sup>4</sup> Paul Nagel, *One Nation Indivisible: The Union in American Thought* (New York, 1964); Merle Curti, *The Roots of American Loyalty* (New York, 1946); Hans Kohn, *American Nationalism: An Interpretive Essay* (New York, 1957); Yehoshua Arieli, *Individualism and Nationalism in American Ideology* (Cambridge, Mass., 1964).

<sup>5</sup> Both Kohn and Curti point out the states' rights and localistic interests of the North but persist in discussing ideas about nationalism rather than considering the relationship between the way the people lived and their beliefs about nationhood. Kenneth M. Stampp, *And the War Came: The North and the Secession Crisis* (Baton Rouge, 1950), is an able study of the Northern reaction to secession but confines itself to the secession winter and fails to examine deeply the sources of the sentiments of 1860–61. In addition Stampp discounts the importance of legal-constitutional concern, an error that seriously impedes an understanding of the period he studies. Instead of examining the reasons for constitutional arguments, he assumes that such arguments were "philosophical rationalizations . . . decorated with seductive words" and "self-deception" (p. 31).

discouraged national activity, and a population that feared its excesses. "States' rights and sovereignty" was a potent rallying cry whose validity and attraction was not diminished but rather demonstrated by the fact that all sections used it when it suited their purposes.<sup>6</sup> In addition the excessively national orientation defies even one of the most tenacious of nineteenth-century images of nationhood. What many Americans admired about their nation was its federal nature, the tradition that kept in local hands the administration of local problems and that gave the people control over their own destiny. This is seen most clearly in discussions over why the nation could and should expand. To assertions that expansion would create tyranny, supporters of manifest destiny replied with discourses on the merits of federalism. Nationalist Edward Everett well expressed their arguments. "By the wise and happy partition of powers between the national and state governments, in virtue of which the national government is relieved of all the odium of administration, and the state governments are spared the conflicts of foreign politics, all bounds seem removed from the possible extension of our country, but the geographical limits of the continent."<sup>7</sup>

In addition the trouble with emphasizing the nation when discussing pre-Civil War nationalism is that such an emphasis misconstrues the nature of loyalty and allegiance. It implies that loyalty to the nation precludes other loyalties, that the dilemma of Robert E. Lee about whether to join his state or his nation describes a necessary dichotomy. But from what social sciences suggest about loyalty it is likely that Lee and those attracted by his agonizing decision saw the problem too simply. Morton Grodzins argues at length that

direct national loyalty [is] a misnomer. It does not exist. Loyalties are to specific groups, specific goals, specific programs of action. Populations are loyal to nation as a by-product of satisfaction achieved within non-national groups, because the nation is believed to symbolize and sustain those groups. From this point of view, one is loyal not to nation, but to family, business, religion, friends. One fights for the joys of his pinochle club when he is said to fight for his country.

Other authors insist that multiple loyalties are possible and indeed that loyalties to things familiar and nearby are the imperative foundations for attachments to larger and more general and hence more vague ideals and entities. Page Smith, discussing the American Revolution, insists that the creation of local communities that attracted the intense devotion of the colonists was indispensable for generating the "power of common action" that resulted in successful rebellion. Martin Buber adds his insight con-

<sup>6</sup> See Arthur M. Schlesinger, Sr., "The State Rights Fetish," in his *New Viewpoints in American History* (New York, 1935), 220-44.

<sup>7</sup> Edward Everett, "The Circumstances Favorable to the Progress of Literature in America," Aug. 26, 1824, in Everett, *Orations and Speeches on Various Occasions*, 1 (Boston, 1850): 33, as quoted in Albert K. Weinberg, *Manifest Destiny* (Baltimore, 1931), 106. See also Frederick Merk, *Manifest Destiny and Mission in American History* (New York, 1963), 26-27.

cerning the inextricable connection between local and national loyalty with the observation that "a nation is a community to the degree that it is a community of communities." Such observations were predicted in the nineteenth century by Tocqueville. With characteristic insight he observed, "Public spirit in the Union is, in a sense, only summing up of provincial patriotism. Every citizen of the United States may be said to transfer the concern inspired in him by his little republic into his love of the common motherland."<sup>8</sup>

If we are to understand the reason that the North went to war for the Union we need to follow such suggestions and direct our attention toward the local experience, toward the environments in which the majority of Northerners lived. To focus locally is to ask the question about why men fought for the Union in a different and I hope more useful and precise way. The question changes from the general "Why fight for the Union?" to "What was there in the daily experience of most Northerners that made them sensitive and responsive to those images the Union evoked?"

As the North reacted to the secession crisis one theme was repeated constantly, and it suggests a crucial fact about Northern society. Again and again newspaper editors and political leaders discussed the degree to which secession was likely to produce disorder, anarchy, and a general disrespect for democratic government. The future president Andrew Johnson pictured for his congressional colleagues "this Union divided into thirty-three petty governments, with a little prince in one, a little potentate in another, a little aristocracy in a third, a little democracy in a fourth and a republic somewhere else . . . with quarreling and warring amongst the little petty powers which would result in anarchy." Congressman Zachariah Chandler announced that if secession were tolerated "I shall arrange for emigration to some country where they have a government. I would rather join the Comanches; I will never live under a government that has not the power to enforce its laws." The conservative Philadelphia *North American* called secession "Lawlessness on a Gigantic Scale" and remarked, "The world must regard with profound astonishment the spectacle of national lawlessness which the southern States of this Union now exhibit. . . . Resistance to law, . . . contempt for order . . . defiant rebellion against the entire structure which we call the United States government." The Boston *Traveller* argued that had the founders of the country provided for peaceable secession they "would have organized anarchy." Allow South-

<sup>8</sup> Morton Grodzins, *The Loyal and Disloyal* (Chicago, 1956), 29; Page Smith, *As a City upon a Hill* (New York, 1966), 14; Martin Buber, *The Writings of Martin Buber*, ed. Will Herberg (New York, 1956), 130; Alexis de Tocqueville, *Democracy in America*, ed. J. P. Mayer and Max Lerner, tr. George Lawrence (New York, 1966), 147. See also David M. Potter, "The Historian's Use of Nationalism and Vice Versa," in his *The South and the Sectional Conflict* (Baton Rouge, 1968), 48; and Harold Guetzkow, *Multiple Loyalties* (Princeton, 1955), 37-39. It was David Potter in the above essay who first pointed to the relevance of works by Grodzins and Guetzkow to the Civil War. His aim in doing so was to suggest that historians consider the possibility of Southern nationalism confronting Northern nationalism rather than seeing Southerners as crabbed sectionalists endangering a national consensus.

ern states to secede, the Douglas-supporting *Burlington Weekly Sentinel* said, and "the Union with all its glory . . . high hopes . . . power, with all its interest has gone and stands in history as another monument of the inability of man to govern himself under the forms of constitutional law."<sup>9</sup>

These warnings and fears, intense as they were, still did not sound the depths of outrage and concern over the threat of lawlessness. It took the firing on Sumter to do that. Until early April men were still debating the question of secession with hope that its potential for disorder might be defused. Many who feared the precedent of successful lawbreaking that secession portended sought to solve the problem by transforming the crime into a lawful act. Newspapers, private citizens, and public figures asked for constitutional amendments that would legalize secession.<sup>10</sup>

The cannonades from Charleston ended such equivocation. Now the issue was not the validity of one constitutional view as opposed to another but the need to uphold government and the rule of law against forceful disruption. Earlier warnings of anarchy were multiplied and intensified. From Chicago: "Without a Union that is free, without a Constitution that can be enforced, without an authority to command respect and obedience . . . our Republic ceases to be a government, our freedom will be quickly supplanted by anarchy and despotism." From Madison, Wisconsin: "This contest is not so much about territorial limits as to demonstrate whether we have government or not." From Indianapolis: "We are fighting for the existence of our own government, and not for the destruction of that at Montgomery." Roxbury, Massachusetts: "Every man instinctively feels that the moment has at last arrived for crushing treason and asserting the supremacy of law and the constitution." Cincinnati: "If [the] doctrine of secession as illustrated and enforced by [its] practice, is true, then there is no such thing as government authority or social obligation. . . . *A surrender to Secession is the suicide of government.*" Philadelphia: "Establish the authority of the Constitution and laws over violence and anarchy." The Republican parties of Indiana, Ohio, and Illinois rallied around the cry "enforce the laws." Two hundred thousand people at-

<sup>9</sup> Andrew Johnson, "The Constitutionality and Rightfulness of Secession," Dec. 18-19, 1860, in *Speeches of Andrew Johnson*, comp. Frank Moore (Boston, 1866), 150, as quoted in Lloyd Paul Stryker, *Andrew Johnson: A Study in Courage* (New York, 1929), 60; Zachariah Chandler: *An Outline Sketch of His Life and Public Services* (Detroit, 1880), 192-93; newspaper quotations from Howard Cecil Perkins, ed., *Northern Editorials on Secession* (New York, 1942), 1: 172-73, 195-96, 520-22. See also Frank Moore, ed., *Rebellion Record* (New York, 1962), 1: 82-102; Arthur C. Cole, *Era of the Civil War, 1848-1870* (Springfield, Ill., 1919), 253-55; Kenneth M. Stampp, *Indiana Politics during the Civil War* (Indianapolis, 1949), 58-60; Eugene Roseboom, *The Civil War Era, 1850-1873* (Columbus, 1944), 376-79; *New York Tribune*, Apr. 17, 1861; George B. McClellan, *McClellan's Own Story* (New York, 1887), 31-32; James Russell Lowell, "E Pluribus Unum," *Atlantic Monthly*, Jan. 1861, p. 238; A. P. Peabody, "Loyalty," *North American Review*, Jan. 1862, p. 156; and Stampp, *And the War Came*, 33-34, 54, 75, 200, 220-25.

<sup>10</sup> See Allan Nevins, *War for the Union* (New York, 1959), 1: 35; Sidney George Fisher, *A Philadelphia Perspective: The Diary of Sidney George Fisher*, ed. Nicholas B. Warnwright (Philadelphia, 1967), 375-77; Frank L. Klement, *The Limits of Dissent: Clement Vallandigham and the Civil War* (Lexington, 1970), 54; and *New York World*, Dec. 15, 1860.

tended a rally at Union Square in New York City on April 20, 1860, and listened for hours to speakers from throughout the North asserting that the rule of law was at stake. Orators such as the New York Democrat John A. Dix, the Whig attorney William Evarts, the city's mayor and future Copperhead Fernando Wood, the Ohio politician Robert Schenck, the Oregon senator Edward Baker, and many others supported the contention of the lifelong Democrat, Robert J. Walker that "we must resist and subdue [secession] or our government will be but an organized anarchy." On April 17 the New York *Tribune* had summarized the prevailing sentiment: "We have a civil war on our hands. There is no use looking away from the fact. For this year the Chief Business of the American people must be proving that they have a Government, and that freedom is not another name for anarchy."<sup>11</sup>

Did secession in fact threaten the order of society in the North? Not this particular secession; a section different in so many ways from the North might justify its departure by pointing to the conflicts that its presence produced. Whatever the reality, however, the factor motivating the Northern response was what men then believed secession might mean and what they foresaw as its consequences. The idea of secession, applied generally, suggested that conflicts between parties should be settled not by harmonizing differences in the service of higher ends but by ignoring those ends in the service of the quarrel of the moment. What community was safe if such a pattern were established and endorsed? But despite this fear, it is possible that peaceful secession would have been tolerated if some regular procedure for division had been adopted or if a way of giving the action some semblance of legality and regularity had been discovered and utilized. The firing on Sumter eliminated this possibility. The issue was now no longer the means of division or the justice of the cause of secession or the right of the national government to use its power to hold a state that sought to depart. The issue after Sumter was, can the country permit force to settle its disputes, cannons to resolve its differences? No people so dependent for success and stability on respecting and adhering to the processes of self-government, in the absence of any other compulsion, could afford to say yes. It is the purpose of the remainder of this article to suggest what in their experience compelled this answer.

NORTHERN DETERMINATION TO UPHOLD government and order against the threat of anarchy was not simply rhetoric. Both speakers and listeners responded to such sentiments naturally, for in talking about the need for order and stable government they were discussing the topic about which the vast majority of Northerners were experts in practice if not in theory. In raising the issue of law and order the speakers struck Northerners quite

<sup>11</sup> Newspapers excerpted in Perkins, *Northern Editorials on Secession*, 2: 808-45.

literally where they lived. No issue, with the possible exception of acquiring wealth, had attracted so much energy and debate in the prewar years. From the Mayflower Compact and the organization of colonial governments, through the writing of the Articles of Confederation, the creation of the Constitution, the ratification of thirty-two state constitutions from 1776 to 1860 in the states that did not secede (fifteen in the last fifteen years before secession), the debates and elections over constitutions that failed, the countless creations of county, town, and city governments, these Americans had been engaged in government-making.<sup>12</sup> Add to this the millions of words expended debating constitutional questions in the prewar era, the numerous court cases that attracted national attention, the almost constant elections in the country with their concomitant discussions of governmental issues, and it is possible to describe the prewar years as a time of continual concern with questions of government, order, and law.

Although the South shared this history and shared also some of the characteristics of Northern society to be described, Southern fears of losing slavery ultimately overwhelmed countercurrents of loyalty and unionism and a respect for law and order. Had the North been similarly threatened it is probable that Northerners would have risked political disorder to preserve societal order. Many Southerners made this choice reluctantly and seem to have worried about the political disruption secession might provoke. Instead of following their secessionist ideas to logical conclusions, they made sure that no provision for secession would exist in the Confederate constitution. Forsaking uncharted paths they wrote a document that copied in many ways the Constitution of the United States. And again and again they insisted that they were not violating the Philadelphia document but rather upholding its true principles against the lawbreakers of the North. As Confederate President Jefferson Davis told his constituents, "The Constitution formed by our forefathers is that of these Confederate states."<sup>13</sup> Surely the South shared with the North a devotion to the established legal order that only a threat to its way of life could shake.

Yet there were differences in attitudes toward and experience with law and government that may help explain the diminished unionism of the South. Charles Sydnor points to Southern views of law that emphasized unwritten rules of conduct and elevated private over public law codes, the practice of dueling being the most obvious example. He notes the impact of slavery in making every master, and most white men, a law unto themselves when it came to dealing with blacks. Southerners also had fewer opportunities for involvement in the legal-political process than did Northerners. There were fewer towns per capita in the South, less rotation in of-

<sup>12</sup> See Francis Newton Thorpe, comp., *The Federal and State Constitutions, Colonial Charters and Other Organic Laws* (Washington, 1909).

<sup>13</sup> Jefferson Davis, "Inaugural Address of the President of the Provisional Government," Feb. 18, 1861, in Dunbar Rowland, ed., *Jefferson Davis, Constitutionalist*, 5 (Jackson, Miss., 1923): 53.

fice, and lower participation in voting—all factors worth exploring to test the degree of personal involvement in issues of order, government, and law in the South. That task, however, must be put off until another time. The focus here is on why the North fought to save the Union, not why Southerners tried to divide it.<sup>14</sup>

The avalanche of oratory debating questions of law, government, and the Constitution had special meaning for Northerners. It was not merely theorizing about ideals but was a practical debate about the way they lived. Government in the pre-Civil War North was not “them”; it was “us.” The national government was days if not weeks away, and its constitutional powers were strictly limited. It regulated interstate commerce, ran the post office, dealt with Indians, conducted such foreign policy as there was, and paid and administered an army and navy that totaled around 28,000 men as of 1860. In 1861 there were about 36,500 paid civilian employees of the national government, and approximately 30,000 of these were local postmasters. The national government did not tax the public at large. It had no powers in matters of health, education, welfare, morals, sanitation, safety, or local transportation. In short, practically every activity that affected the lives of Americans was the province of either state or local government—and more often than not it was local. An observer in 1850 described the structure of government:

The President has one postmaster in every village; but the inhabitants of that village choose their own selectmen, their own assessors of taxes, their own school-committee, their own overseers of the poor, their own surveyors of highways, and the incumbents of half a dozen other little offices corresponding to those which, in bureaucratic governments, are filled by appointment of the sovereign. In all these posts, which are really important public trusts, the villagers are trained to the management of affairs, and acquire a comprehensiveness of view, a practical administrative talent, and a knowledge of business. . . . This training is very general; for owing to our republican liking for rotation in office, the incumbents of these humble posts are changed every year or two.<sup>15</sup>

Nowhere was this description more true than in the states of the Middle West. Indeed the recent frontier experiences of that region guaranteed a widespread personal involvement in questions of government, law, and order. In the two generations before the Civil War, wilderness and Indian

<sup>14</sup> On slavery, the South, and the law, see Kenneth M. Stampp, *The Peculiar Institution* (New York, 1956), 192–236; Charles Sydnor, “The Southerner and the Laws,” *Journal of Southern History*, 6 (1940): 3–23; Walter Dean Burnham, “The Changing Shape of the American Political Universe,” *American Political Science Review*, 59 (1965): 7–21; and Stanley Elkins and Eric McKittrick, “A Meaning for Turner’s Frontier,” *Political Science Quarterly*, 69 (1954): 340–42, 567–75. This article by Elkins and McKittrick was the stimulus for my thinking about the relationship between government-making and Northern support for the war. The authors, focusing on the nature of the frontier experience, do not draw any conclusions about the coming of the war.

<sup>15</sup> Francis Bowen, “The Rebellion against the Magyars,” *North American Review*, Oct. 1850, p. 502, as quoted in Leonard D. White, *The Jacksonians* (New York, 1954), 511–12. See White’s discussion of local government on these pages. See also *The Statistical History of the United States from Colonial Times to the Present* (Stamford, 1965), 710, 737.

territory had been transformed into states: Ohio, 1803; Indiana, 1816; Illinois, 1818; Michigan, 1837; Iowa, 1846; Wisconsin, 1848; Minnesota, 1858. Such state-making had been preceded by community-making on a vast scale. The early years of settlement in these states lend some support to the popular image of a wild, uncivilized frontier. The predominant pattern, however, was that of a successful struggle for community order, a struggle without much opposition once the permanent settlers arrived. These pioneers carried in their baggage the experience with and devotion to the government institutions of the East. Indeed many of those who came to the West were conservative members of Eastern communities who wanted to re-establish the sort of order they believed was being undermined by liberal domination of their homelands. Their commitment to stability and order can be seen in the fact that they founded churches and schools in the first days of community building. Henry Ward Beecher noted in the 1850s that the frontier settlers "drive schools along with them as settlers drive flocks. They have herds of churches, academies, lyceums; and their religious and educational institutions go lowing along the western plains as Jacob's herds lowed along the Syrian hills."<sup>16</sup>

The institutions the settlers of the Midwest founded were copies of those they had known in the East. The New England model was widely emulated, and it ensured the formation of stable communities, as whole communities often moved together, locating their towns on land that was already plotted and organized into townships. Such activity minimized mad scrambles for land. In some cases settlers found that the apparatus of county and town government preceded them into a region, and they were bound by law and inclination to adopt that organization.<sup>17</sup>

Even in regions lacking such controlled settlement a strong respect for the necessity of an ordered way of doing things tended to prevail. Settlers created their own law and enforced it. The most colorful example of such

<sup>16</sup> Henry Ward Beecher, as quoted in Ray Allen Billington, *The Frontier and American Culture* (n.p., 1965), 7-12. See also Louis B. Wright, *Culture on the Moving Frontier* (Bloomington, 1955), *passim*; Richard C. Wade, *The Urban Frontier* (Cambridge, Mass., 1959), 315-17; and Smith, *City upon a Hill*, ch. 3. The German visitor Fredrika Bremer observed in 1853, "Whenever Americans establish themselves, the first buildings they erect, after their dwelling house and places of business are schools and churches." *The Homes of the New World: Impressions of America*, tr. Mary Howitt (New York, 1853), 2: 135. The war, of course, would generate the Copperhead movement in the Midwest. This fact, however, does not significantly weaken the contention that involvement in the processes of government generated a willingness to fight when the Union was endangered. The region shared the patriotic outpouring spawned by Sumter. Until economic depression struck the region in late 1861-62, future Copperhead leaders like Clement Vallandigham and Samuel Medary were lonely men. Even Vallandigham's opposition to the Lincoln administration rested in part on a concern for the Union. This Ohio Copperhead opposed the president, believing that the attempted coercion of the South guaranteed permanent disunion. See two books by Frank L. Klement, *The Copperheads in the Middle West* (Chicago, 1960), 18, 208; and *Limits of Dissent*, 62-68.

<sup>17</sup> Merle Curti, *The Making of an American Community* (Stanford, 1959), 261, 296-97; Albert Shaw, *Local Government in Illinois* (Baltimore, 1883), 5-19; George E. Howard, *Introduction to Local Constitutional History* (Baltimore, 1889), 135-238; Smith, *City upon a Hill*, 11-19; Thomas D. Clark, *Frontier America* (New York, 1969), 306-07, 598.



action was the claim club. Claim clubs were organizations of settlers who wished to secure title to land that had yet to be organized by national law. In Illinois, Wisconsin, Iowa, Minnesota, Nebraska, and other territories the clubs sprang up, manifesting the willingness and capacity of Americans to be their own lawmakers. The clubs' success was noted by one Methodist circuit rider who observed a Wisconsin settlement in 1835.

They had, in the absence of all other law, met and made a law for themselves. They have surveyed the township & ascertained that section 16, the school section, was within the grove, & they staked it off & appointed commissioners to take care of it. . . . They had also meted and bounded every mans wood land, allowing each family 40 acres of timber. . . . There was an understanding in the country, equivalent to a law of the land, that the settlers should sustain each other against the speculator & no settler should bid on anothers land.<sup>18</sup>

The experience of Iowa was typical. Settlers there rushed into territorial lands before the government survey was completed. The land was not officially for sale until 1838 but squatters were there in 1837. Such disrespect for federal law did not signal an uncontrolled, wide-open struggle of the rule of the strongest. True to Northern experience the settlers organized their claim clubs to regularize procedures of land acquisition and to maintain stable prices. These clubs represented more than merely claim protectors. As Thomas D. Clark points out, "These settler organizations had an even broader implication than their function in protecting land claims; they helped to maintain law and order in the frontier community."<sup>19</sup>

Similar experiences took place in the mining regions of the West. Miners who rushed to California often arrived before federal or state law had organized the areas. Again these Americans exercised their lawmaking talents. Miners gathered together and drew up regulations that often began, "Whereas, this district is deficient in mining laws and regulations, and disputes have arisen: therefore, we, the miners of ——— district, in convention assembled, do pledge ourselves to abide by the following laws." By 1866 there were over eleven hundred of these self-governing districts in the eight Western states and territories. A Senate committee investigating these laws observed "this great system established by the people in their primary capacities . . . evidencing by the highest possible testimony the peculiar genius of the American people for founding empire and order."<sup>20</sup>

The relevance of this well-known material to the question of fighting the Civil War is this: again and again Americans proved to themselves that they were lawmakers—that the law and order of their communities was their personal responsibility and depended on their actions and efforts.

<sup>18</sup> Quoted in Daniel Boorstin, *The Americans: The National Experience* (New York, 1965), 75; see also 72–78. And see James Willard Hurst, *Law and the Conditions of Freedom in the Nineteenth Century United States* (Madison, 1967), 3–5.

<sup>19</sup> Clark, *Frontier America*, 594.

<sup>20</sup> Quoted in Boorstin, *Americans: National Experience*, 79.

Their connection with the preservation of that order was intimate, vital, and compelling. This is made even more clear when we consider the way in which Americans gave passionate attention not only to the creation of an ordered community but to the legal battles that continued in the towns after they had been established.

Court terms at county seats in the prewar years were gala social events. People drove or walked into town in large numbers. Big wagons filled with women and children and loaded with provisions entered the town from every direction. People followed their favorite lawyers like modern sports stars and demanded of them not only courtroom performance but speeches in the evenings after the courts had closed. The people knew these lawyers, and it was eminently to the advantage of lawyers to be well known and popular, as practically all cases were decided by juries. The system of legal education that prevailed at the time further encouraged a familiar and popular image of the law. Lawyers were preponderantly local men who received their legal training at the hands of neighbors who were established attorneys. They thus became as familiar with and sensitive to local customs and ideas as they were with Blackstone, Coke, and Littleton, for their success was more dependent on such informal awareness than on formal knowledge. In addition opposing lawyers and judges all knew each other well, usually living together in the same boarding house during the court term. Plaintiffs and defendants were often well acquainted, and all figures in most cases were familiar to the people of the area. The jury system, intense popular interest, local training, and the social intimacy produced by a nation that was essentially a nation of small towns and farmers thus all encouraged a law that tended to mirror the judgment and mores of the town or locality. Page Smith's conclusion rings true: "In the small town law was what the community had ordained, growing directly out of the needs and aspirations of the people. It was not something remote, alien, imposed from without."<sup>21</sup>

Popular involvement in questions of the legal order of course transcended concern with judicial proceedings. More than simply jurors and witnesses, these people themselves were the makers of the law they observed being contested. The creation of communities had meant more than providing a police and judicial function; it meant establishing and manning the institutions that made the community function. For every community that was created there were problems of education, health, housing, and municipal services to be solved and offices to be sought and staffed. The extent of involvement the citizens demonstrated and their interest in the survival and prospering of their governmental order was the measure of community success.

The frontier regions, some of them settled only fifty years before, some

<sup>21</sup> Smith, *City upon a Hill*, 131. See also Albert J. Beveridge, *Abraham Lincoln, 1809-1858* (Boston, 1928), 1: 528-40; and *Historical Statistics of the United States*, 14.

in the process of settlement in 1860, demanded the most widespread involvement. From earliest cabin raisings, to the creation and maintenance of towns, to the sustaining of large cities, people took part actively. Men without prior experience were drafted as officials or rushed to file for office as eagerly as they filed their land claims. For example, in Hamilton County, Ohio, an election was called for delegates to the constitutional convention of 1802. Of the ninety-four candidates who filed for ten positions, twenty-six received between 121 and 1,635 votes. "Everybody," one Indiana pioneer wrote, "expected at some time to be a candidate for something; or that his uncle would be; or his cousin, or his cousin's wife's cousin's friend would be; so that everybody and everybody's relation's friend, were forever electioneering." Such involvement in politics continued unabated into the decades before the war. As Henry Clyde Hubbart writes of the older Middle West, "The people of the free West in the forties and fifties were a political people. This was true in all senses of the term politics whether the word be held to mean political ideas and philosophy; or social attitudes . . . or in a narrower sense political party programs or activities; or in a still narrower sense, the designs and manipulations of politicians and petty factions." Large numbers of Americans, then, took a personal and active part in creating the institutions of government that made civilized life possible in the West. Commenting on what they see as the crucial element in the frontier experience, Stanley Elkins and Eric McKittrick conclude, "Here was a society in which the setting up of institutions was a common experience."<sup>22</sup>

Concern for the government and politics was not confined to the frontier areas of the West. Indicators of involvement in such questions appear in the East as well. In the first place, the founding of towns and the consequent opportunities and demands for participation were not limited to the West. Five of the twenty-seven towns that in 1851 made up Oneida County in New York had been founded between 1827 and 1846. Between 1823 and 1836 there were four towns established in that state's Herkimer County. Erie County, New York, saw the founding of new towns in 1851, 1852, 1853, and 1857. Bucks County, Pennsylvania, just outside Philadelphia, had newly created towns up to 1838, and Venango County in Pennsylvania organized townships in 1845 and 1850 and would continue to do so up to 1876. Towns that had been long established would often rededicate themselves to maintaining the communities they had and to carrying out the ideals of their founding. Again and again, then, Americans in the nonseceding states involved themselves in government-making, considered the problems of self-rule, and thereby made government their own responsibility.<sup>23</sup>

<sup>22</sup> Bayard Rush Hall, in James A. Woodburn, ed., *The New Purchase* (Princeton, 1916), 178; Henry Clyde Hubbart, *The Older Middle West, 1840-1880* (New York, 1936), 10; Elkins and McKittrick, "A Meaning for Turner's Frontier," 331-36.

<sup>23</sup> Pomeroy Jones, *Annals and Recollections of Oneida County* (Rome, N.Y., 1851), 13;

Involvement in the governing and political process was not limited to forming governments. Rotation in office apparently was frequent. Herkimer County in twenty-six years re-elected a member of Congress only three times; of twelve state senators, only one man repeated in thirteen years. The town of Caroline in New York saw twenty-six men in the office of town supervisor in a forty-nine-year period. The longest tenure was four years, and only two men held the job that long. The town of Queensbury, New York, had fifty-nine different justices of the peace from 1795 to 1873 and 143 different constables from 1766 to 1873. Town records contain thirteen pages listing seventy men each as highway commissioners from 1766 to 1873, most of them single termers. Similarly great rotation in office occurred in the offices of town and ward supervisor in Buffalo from 1854 and in Venango County, Pennsylvania. A sample of New England towns suggests similar facts of rotation. Worcester, Massachusetts, between 1848 and 1859 had seven different mayors and forty-nine different aldermen out of seventy-two possible positions. Ward supervisor positions show the same pattern. Taking only two wards we see twenty-two different supervisors for thirty-six possible jobs in Ward One and twenty-six different supervisors out of thirty-six possibilities in Ward Four. Roxbury, Massachusetts, rotated officeholders with similar frequency.<sup>24</sup>

One did not have to be an officeholder to maintain an intense interest in public affairs. Visitors noted how passionately Americans were involved in politics, the turbulence and intensity of elections, and the apparent bitterness of party strife. Their observations are borne out by statistics. For the years 1848 to 1872 Walter Dean Burnham estimates the mean turnout for presidential elections to be an impressive 75.1 per cent. (By way of comparison, the national turnout in the 1968 election was 61.6 per cent.) In off-year elections an estimated 65.2 per cent of those eligible voted. (The 1970 off-year elections for the House brought 44.9 per cent of the eligible voters to the polls.) When the South is removed from the sample the figures are more impressive still. Between 1868 and 1880 the mean turnout in presidential elections was 82.6 per cent in the North. In Michigan between 1854 and 1872 the mean turnout in presidential years was 84.8 per cent, in off-years 78.1 per cent. For Ohio from 1857 to 1879 these figures read 89.0 per cent and 78.4 per cent. In New York from 1834 to 1858 the mean turnout in presidential years was 84.8 per cent and in off-year elections 81.5 per cent. Further evidence of involvement is seen in the fact that

Nathaniel Benton, *A History of Herkimer County* (Albany, N.Y., 1856), 389; Cresfield Johnson, *Centennial History of Erie County, New York* (Buffalo, 1876), 446, 454; W. W. H. Davis, *History of Bucks County, Pennsylvania* (Doylestown, Pa., 1876), 687, 689; *History of Venango County, Pennsylvania* (Chicago, 1890), 689, 702, 732, 734, 740, *passim*.

<sup>24</sup> Benton, *Herkimer County*, 483, 487-90; *Landmarks of Tompkins County, New York* (Syracuse, 1894), 287; A. W. Holden, *History of the Town of Queensbury* (Albany, N.Y., 1874), 64-66, 79-82, 88-100; Johnson, *Erie County*, 456; *Venango County*, 141-52; *City Documents of Worcester* (Worcester, Mass., 1859), 147-48; *Roxbury City Documents* (Roxbury, Mass., 1854), 138-41; *Roxbury City Documents* (Roxbury, Mass., 1860), 231-42. See also Bayrd Still, "Patterns of Mid-Nineteenth Century Urbanization in the Middle West," *Mississippi Valley Historical Review*, 28 (1941): 187-206.

there was a very small amount of roll-off in the voting process. (Roll-off is the tendency to vote only for major offices and to ignore lesser ones.) Between 1857 and 1879 in Ohio the mean roll-off was 0.6 per cent. New York from 1834 to 1858 witnessed a 1.6 per cent roll-off. From 1854 to 1872 Michigan's mean roll-off was 0.9 per cent. Additional testimony to the intensity of political concern is seen in two notable statistics. First, there was very little split-ticket voting. Political preferences tended to be strong and tenacious. Certainly mid-nineteenth-century voters hoped that the best man would win, but they tended to think that the best men were devoted to Democratic, Whig, or Republican principles. Second, there was very little party switching, with the understandable exception of the election of 1840. When either of the major parties won large majorities, these came not as the result of changes in political allegiances but because of abstentions by the voters of one party. Either one voted for his party or he did not vote; it was a rare man who viewed politics so dispassionately that he could stomach voting for the opposition party.<sup>25</sup>

In the prewar period, then, Americans made their own governments, enforced their own laws, staffed their own institutions, and gave intense attention to questions of government, politics, and law. There existed compelling personal reasons to be devoted to the preservation of law and order.

THERE WAS VIOLENCE in prewar American society—a great deal of it. Indians, abolitionists, immigrants, Negroes, Mormons, Masons, as well as the WASP majority all received their share. But violence is not necessarily the opposite of law and order. Modern riots and disruptions have provoked and energized a “law-and-order” movement, but that should suggest that violence may be the instrument of stability as well as disorder. In fact, when we look carefully at the violence of the pre-Civil War years, as well as our own, we discover how much of it, though not all, resulted from efforts to preserve, not to destroy, the existing order.<sup>26</sup> Abolitionists were attacked by “gentlemen of property and standing” for threatening the

<sup>25</sup> Fredrick von Raumer, *America and the American People* (New York, 1846), 267–69; Bremer, *Homes of the New World*, 2: 149–52; Burnham, “Changing Shape of the American Political Universe,” 7–28. Roy Franklin Nichols blames continual political activity for helping to cause the “hyperemotionalism” that brought on the war. *The Disruption of American Democracy* (New York, 1948), ch. 28. I would accept this view, but I would eliminate Nichols's negative connotation and suggest that the emotion with which Northerners viewed politics was natural and indicates the worth, not the evils, of the democratic process.

<sup>26</sup> The modern concern with America's violent past has inspired energetic and fruitful searches for incidents of violence. But one wonders if we ought to claim for ourselves a position as a uniquely violent people. One foreign visitor of the nineteenth century denied that Americans deserved special recognition in this field. Writing in 1846, Fredrick von Raumer cataloged violence against the Mormons, rent strikes in New York, and the Dorr Rebellion in Rhode Island, but he cautioned against too hasty a judgment against democracy because of such incidents. “Those who regard such acts of violence as a natural necessity, an inevitable consequence of republican institutions, take a one-sided and erroneous view of the matter.” There was as much violence in England, Wales, Ireland, and even in Canada during a single year as there was in the United States, and Paris alone, he argued, outdid this country in violence. *America and the American People*, 257–66.

existing economic and racial status quo. Indians were seen as savage threats to the expansion of a prosperous and comparatively well-organized society. Immigrants threatened to inject foreign ideas and practices into a political system that demanded consensus. Mormons outraged the morals of their neighbors and seemed to endanger the prevailing ideology by their exclusiveness. Masons were charged with an anarchistic atheism that would destroy the Christian sinews of society. And Negroes who forgot their assigned place knew that their punishment would not be restrained by charity.<sup>27</sup>

Much of this violence was part of the persistent vigilante tradition in American society. Although hardly the sort of law and order that civil libertarians admire, vigilantism is "as American as cherry pie" and springs from the same sentiment that is the focus of this essay—the belief that individual Americans are responsible for the preservation of stability, that the law is an expression of popular sentiment, and that the people have the duty to maintain it even if procedural due process is not respected. Vigilantism is, as Richard M. Brown argues, socially conservative—an attempt to secure and maintain a society that respects property, stability, and order. To the degree that he feels personally responsible for maintaining order, therefore, every American is potentially a vigilante. Michel Chevalier saw this in 1833 and thought that it was admirable. Local saloonkeepers, he observed, were often the "police commissioner," and the tavern regulars "would in case of necessity be ready to act the part of constables." In a society that lacked a powerful state, he concluded, "This is real self-government; these are the obligations and responsibilities that every citizen takes upon himself when he disarms authority."<sup>28</sup>

Recognition of the strength and pervasiveness of such law-and-order sentiment and of the personal involvement of Americans in the creation and sustenance of government suggests weaknesses in the predominant historiography describing prewar society. David Donald, Stanley Elkins, and Rowland Berthoff have emphasized the unstructured and hence disordered nature of pre-1860 America. Burgeoning popular rule and expanding self-esteem weakened faith in old institutions, Donald argues, and social mobility further weakened society's sinews. Elkins emphasizes the breakdown of key institutions in explaining why Northern intellectuals

<sup>27</sup> Leonard L. Richards, *Gentlemen of Property and Standing* (New York, 1970); Ray Allen Billington, *Protestant Crusade* (New York, 1938); Lorman Ratner, *Antimasonry: The Crusade and the Party* (Englewood Cliffs, 1969).

<sup>28</sup> Richard Maxwell Brown, "The American Vigilante Tradition," in Hugh Davis Graham and Ted Robert Gurr, eds., *Violence in America: Historical and Comparative Perspectives* (Washington, 1969), 121–70; Michel Chevalier, *Society, Manners and Politics in the United States* (Gloucester, Mass., 1967), 321–22. One of the interesting features of vigilantism is how respectful its practitioners often were of due process of law. After apprehending their victims they frequently held trials with appointed judges, juries, prosecutors, and defending attorneys. Brown distinguishes such activity from "instant vigilantism," which dispenses with due process. See also Laurence Veysey, "Freedom and Disorder in American History: An Interpretation," in Veysey, ed., *Law and Resistance* (New York, 1970), 17–19.

attacked slavery with such unrestrained intensity. Quoting Henry James, as does Donald, Elkins describes American society: "No State, in the European sense of the word, and indeed barely a specific national name. No sovereign, no court, no personal loyalty, no aristocracy, no church, no clergy, no army, no diplomatic service, no country gentlemen, no palaces . . . no cathedrals . . . no great universities nor public schools . . . no literature . . . no political society." Americans are "an unsettled people," Berthoff insists, adding to the strictures of Donald, Elkins, and James a family structure weakened by growing industrialism, the explosive fears of immigrant impurities, and the extraordinary mobility of this people.<sup>29</sup>

The weaknesses in the arguments of Donald and Elkins arise from their sources. Both men use extensively the arguments of elites—intellectual, political, and/or economic—men very likely to believe that as control of society slipped from their hands it fell into the hands of incompetents. Certainly Calhoun and Webster, like Walter Bagehot, distrusted the untutored masses, and it is not surprising to find Henry James, just returned from years in London society, believing that a nation lacking a church, aristocracy, clergy, country gentlemen, cathedrals, and English public schools was about to atomize. As for Elkins, it is reasonable to doubt that one can describe the stability of a whole society by analyzing, however brilliantly, the ideas of Concord, Massachusetts, of Transcendentalists whose emphasis was the intensely aware, free individual seeking community with the Oversoul.

Doubtless, Berthoff and Donald (when the latter describes prewar society rather than relying on the judgments of Bagehot, Calhoun, and Webster) do describe essential characteristics of antebellum America. Industrialization did threaten the family and weaken established economic patterns. The lust for wealth encouraged roaring boom towns and boom-town mentalities: a callousness about anything but the acquisition of wealth. Certainly many feared that immigrants threatened the stability of American society, and there was a unifying force in seeming to defend democracy against an insidious invasion of "popery." Surely America would have been more unified had the national government consistently compelled, by its energy, the admiration of its people. Mobility did uproot Americans and not only transformed communities left behind but influenced the form of those that were created. And there is a strong element of truth in Berthoff's suggestion that the Union garnered devotion because it was an "overarching abstraction" that compensated for the breakdown of families, churches, and old elites. But there is more, I believe, to the fight for the Union, and more to prewar society than this.<sup>30</sup>

<sup>29</sup> David Donald, "An Excess of Democracy," in his *Lincoln Reconsidered* (New York, 1956), 235; Stanley M. Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959), 140-56; Rowland Berthoff, *An Unsettled People: Social Order and Disorder in American History* (New York, 1971), 175-233.

<sup>30</sup> On the issue of the order of prewar society Clinton Rossiter comments, "Several scholars,

What is omitted in all these discussions is the factor that must be included in any description of the structure of society and the meaning of nationhood: the daily experience of the people with government. The weakness of current historical literature on the subject is its failure to consider common experience as an important element of social unity, its failure to discuss the ubiquitous personal familiarity of Americans with the institutions of law and order.

But evidence abounds to suggest the importance of this experience—primarily in the record of daily activities but also in the observations of visitors and of the nation's literary figures as well. Despite the protean nature of prewar society visitors noted again and again the ease with which Americans preserved and developed the tools of self-rule. They were struck especially by the facility with which the American people created the communities and associations they needed. "These people associate as easily as they breathe," Fredrika Bremer noted. The need for American settlers to work together, encouraged by the absence of government energy, produced a capacity for self-generated unity that overcame the natural centrifugal tendencies of equality, Tocqueville observed. After over twenty years in this country, the German-American political thinker Francis Lieber was still struck by "the thousandfold evidences of an all pervading associative spirit in all moral and practical spheres."<sup>31</sup>

What they were describing of course was the fact of democratic government—the constantly demonstrated ability of the people to make government and use it for their purposes. On the national level, as representatives from Maine tried to solve the issue of Southern slavery and senators from Alabama sought to establish a government for Kansas territory, in short, as national legislators wrestled with moral issues fomenting in places unfamiliar to them, democratic government was demonstrating its limitations. But in the range of life most people lived, self-government was creating abilities and commitments that would ensure that democratic government would endure in the United States whatever its national infirmities. Men were developing a sense that government was them and that they were responsible for order.

This personal experience with self-government was of course local, as it must be. How could one feel that he was a meaningful part of a government of thirty million people? How could one feel that his wishes were known and respected in a national government that elected presidents

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notably Stanley Elkins and Rowland Berthoff, have found so many signs of disorder in this period that they have written forcefully a shattering 'institutional breakdown.' Much as their researches command respect I cannot agree with so dismal a view. They have exaggerated the orderliness of eighteenth century America, underestimated both the tenacity of the family and importance of voluntary associations in the nineteenth century, made a false equation of proliferation with anarchy (as Elkins does in the case of churches) and somehow assumed that man on the move is man completely naked and exposed." *The American Quest, 1790-1860* (New York, 1971), 170.

<sup>31</sup> Bremer, *Homes of the New World*, 2: 153; Tocqueville, *Democracy in America*, 481-84; Francis Lieber, *On Civil Liberty and Self Government* (Philadelphia, 1851), 129.



through electoral colleges, senators through state legislatures, and in which the only popularly elected national officials, members of the House of Representatives, represented, at the minimum, thirty thousand people? Men will always view larger concepts and institutions through the prism of their own experience.

And yet when the Union was attacked Northerners equated an attack on the national government with a threat to the self-government they had experienced. Why? The question of course defies absolute answer, but some speculation is in order. Surely important answers have been suggested by the work of Nagel, Curti, Kohn, and Arieli: men thought of themselves as a nation for the reasons these scholars suggest. But we must add to these reasons what the private experience of Northerners contributed. Among other things the blossoming national economy undoubtedly provided many of them with personal experience of connections with other states, other regions. And migration certainly was a factor. Many citizens of the West had been born in other parts of the country; their experience of what the nation comprehended was thus broader than their present locality.

But what is striking is the fact that throughout the disunion crisis of 1860-61 men spoke not merely of a national economy or of the ties of sentiment that bound them to their birthplace but constantly and passionately of the destruction of self-government should secession succeed. They were thus seriously concerned with the preservation of the institutions of government that they were a part of, and they linked their experience with that government to the survival of the Union. Local personal experience was somehow bound to the preservation of national institutions. How?

First of all, they knew that the Union was a federal union, that local government performed administrative functions that Washington could not, and should not, supply. Local government was an inextricable part of the nation's governing process. Functions of local and national government were divided, and debates raged over state versus national rights and powers. Yet the division of power and responsibility existed not for its own sake but because men believed that a national government could not function, nor could it remain the government of a free people, if it took to itself the governing of a vast continent. Men wanted to preserve local government so that the nation could function and continue to be free.

Second, and perhaps of greatest importance in establishing the connection between local self-government and the survival of the Union, was this fact: not only was local government an administrative necessity; it was the fundamental characteristic of this nation. The country had been founded with the ideal of self-rule in mind, had fought a revolution to secure it, and had created a constitution that respected it. Americans endorsed and validated this national ideal every time they established institutions of self-government.

Americans were not attached to a place; constant migration demonstrated

that. They were not devoted to the land; the image of the land as real estate, the continuing land speculation attested to that. What made them Americans was that they ruled themselves wherever they went. The Scotsman Alexander McKay saw this with notable insight. What distinguished this people, he observed, was "the feeling which they cherish towards their institutions." Europeans loved the land that they and their ancestors had occupied for centuries. But "the American exhibits little or none of the local attachments which distinguish the European. His feelings are more centered upon his institutions than his mere country. . . . His affections have more to do with the social and political system with which he is connected than with the soil he inhabits." Europeans tended to be miserable when separated for long periods from their birthplace, but "give the American his institutions and he cares but little where you place him." McKay admitted that in places like New England there was strong local feeling but what was "astonishing" was "how readily even there an American makes up his mind to try his fortunes elsewhere, particularly if he contemplates removal to another part of the Union, no matter how remote . . . providing the flag of his country waves over it, and republican institutions accompany him on his wanderings."<sup>32</sup> Local institutions of democratic self-government were thus a nationalizing force, and devotion to them was the imperative bond of union.

Of course not every Northern soldier would go to war against the South with the words "law and order" on his lips. Many would enlist in the excitement of the moment. Many would seek the cheap glory of a short war and sign up for ninety days to march under the banner "the Union forever." A response of simple outrage at being attacked was natural enough and was probably widespread. Many believed that the time had come when war might purge the nation of many of its corrupting impurities—the willingness to value material wealth over nobility of character, the inclination to serve personal selfishness rather than the good of society. Republicans naturally were unwilling to destroy their party and repudiate the principles on which they had been elected by yielding to Southern threats and then rebel gunfire. The vast majority accepted the assertion of Richard Henry Dana that the North should not "buy the right to carry on the government, by any concession to slavery." For these reasons and others men went to war.<sup>33</sup>

Yet admitting these expressions of anti-Southern sentiment does not weaken the argument so far advanced for the importance of the idea of law and order in generating a willingness to fight. The source of much of

<sup>32</sup> Alexander McKay, *The Western World, or, Travels in the United States in 1846-47* (London, 1850), 3: ch. 11, quoted in Allan Nevins, ed., *America through British Eyes* (New York, 1948), 260-61.

<sup>33</sup> Richard Henry Dana, Jr., *Speeches in Stirring Times*, ed. Richard Henry Dana, III (Boston, 1910), 145; George M. Fredrickson, *The Inner Civil War* (New York, 1965), 36-50; Otto Eisenschimel and Ralph Newman, eds., *The American Iliad* (Indianapolis, 1947), 31-37; Eric Foner, *Free Soil, Free Labor, Free Men* (New York, 1970), 219-25.

this sentiment was a widespread fear that the institutions of self-government that maintained ordered liberty were threatened by slavery and the South. To describe the incidents of the 1850s that spawned or encouraged anti-Southern feelings is practically to catalog apparent threats by slavery on such Northern institutions.

The outrage against the Fugitive Slave Law was provoked in part by the Northern belief that this law defied local custom and traditions of self-government. The Kansas crisis was often described as proof that local government might be despoiled by slavery and its supporters. When Presidents Pierce and Buchanan supported the proslavery government in Kansas, despite the Free-Soil majority in the territory, their action was taken by many as a sign that the corrupting hand of slavery had captured the nation's executive office. When the Supreme Court produced the Dred Scott decision opponents saw evidence that courts were not immune from the same corruption. When Charles Sumner was attacked in Congress the event was described as more than the beating of one fire-eater by another; it was declared to be one more Southern attack on the principle of free speech in a free government and hence on Congress itself. The impact of these events would lead many of the North's conservative legal thinkers, men devoted to the preservation of existing legal and governmental institutions, to take anti-Southern positions even though they deplored the extremism of abolitionism. A potent source of anti-Southern sentiment was thus a widespread fear that slavery and its proponents endangered the institutions of self-government of the nation. Such a threat, of course, would shake Northerners profoundly, for it involved institutions that were an inextricable part of their experience as citizens of a democracy.<sup>34</sup>

Examining the strengths of democracy on the eve of secession one perceptive author hit the mark: More than any other government, wrote Henry Flanders, democracy identified the citizen with his government and thus instilled a powerful patriotism. The citizen in such a state was "an indirect but influential agent in the administration of its affairs, watches with eager interest its course and whenever difficulty or danger impends, with something more than a sense of duty or spirit of loyalty, acts boldly and greatly in its service." Such people were deeply devoted to the law, he continued, for "in doing homage to law, they do homage to themselves, the creators and preservers of law." Three years later, with the war raging, Andrew Preston Peabody was equally struck by the way in which self-government created patriotic citizens. Men who made the laws themselves felt personally responsible for them and their survival. Pea-

<sup>34</sup> *Ableman v. Booth* 21 Howard 506 (1859); Cecil [Sidney George Fisher], *Kansas and the Constitution* (Boston, 1856), 50-58; U.S. Congress, House, *Kansas Affairs*, 34th Cong., 1st sess., 1855-56, H. Rept. 200, serial set, no. 869; Joel Parker, *The True Issue and the Duty of the Whigs* (Cambridge, Mass., 1856), 5-11; Phillip S. Paludan, "Law and Equal Rights: The Civil War Encounter" (Ph.D. dissertation, University of Illinois, 1968).

body's language was ornate, but the meaning for the war was precise: "He in whom resides an aliquot portion of the sovereignty" he wrote, "will bear his kingly estate in mind on the numerous occasions in daily life on which he might else forget even his manhood."<sup>35</sup>

Urging the energetic prosecution of the war James Russell Lowell had observed that "our Constitution claims our allegiance because it is law and order." Northerners did not forget their responsibility for that law and order. Foreign observers might have doubted that the nation with the least powerful national government in the world, a nation apparently so centrifugal, could and would find soldiers for a struggle to maintain unity, but those who knew the nation were not surprised. Exulting in the proof of strength that was indubitable by 1864 an obscure writer in the *Atlantic Monthly* remarked, "The bubble of Republicanism, which was to display such alacrity at bursting, is not the childish thing it was once deemed. . . . We have proved that we are a nation equal to the task of self-discipline and self-control."<sup>36</sup> The daily experience of Americans with self-government, with fashioning and maintaining law and order, had done its work well.

<sup>35</sup> Henry Flanders, "British Strictures on Republican Institutions," *North American Review*, July 1859, pp. 104-07; Andrew Preston Peabody, "Mill on Representative Government," *North American Review*, July 1862, p. 232.

<sup>36</sup> James Russell Lowell, "Loyalty," *North American Review*, Jan. 1862, p. 156; T. J. Trowbridge, "We Are a Nation," *Atlantic Monthly*, Dec. 1864, p. 771.

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## Buenos Aires as a Commercial-Bureaucratic City, 1880-1910: Characteristics of a City's Orientation

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JAMES R. SCOBIE

ARGENTINA'S CAPITAL CITY and major port grew and prospered in the late nineteenth century far more than most other world cities. Its principal functions consisted of administration and commerce. For a period the development of Buenos Aires largely determined the course of national evolution, and the city virtually became the nation. That phase of rapid urban expansion failed, however, to evolve into sustained national growth, and in retrospect it may have hindered the overall development of Argentina. This article describes some internal processes of the expansion of Buenos Aires and tentatively suggests the relationship of the resultant characteristics to national development and possibly to cities elsewhere.

Europe's unprecedented demand for hides, wool, cereals, and meat from the fertile pampas region; the availability of foreign capital for railroads, port works, utilities, urban construction, and land speculation; and the massive immigration of European laborers at the peak of their productive years—all of these stimulated Argentine economic expansion at the end of the nineteenth century and nurtured the development of Buenos Aires as a commercial-bureaucratic city. Between 1880 and 1910 the value of Argentina's exports, between 96 and 98 per cent agricultural, increased more than sixfold. By 1913 over eight billion dollars (at 1950 prices) in foreign capital had been invested in Argentina; in one boom year, 1889, Argentina absorbed between 40 and 50 per cent of all British

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capital invested outside the United Kingdom.<sup>1</sup> European immigrants flooded into Argentina to meet the demand for labor. Over four million Europeans entered Argentina in these thirty years, slightly more than the total Argentine population in 1895, and of those nearly 60 per cent remained in Argentina.<sup>2</sup>

These inputs redounded to the benefit of Buenos Aires and its immediate environs. The population of the city and province of Buenos Aires rose from 28 to 46 per cent of the national total between the first Argentine census in 1869 and the third in 1914, with an expansion from 500,000 inhabitants to 3,600,000. Twenty per cent of the national population resided in the federal district alone.<sup>3</sup> The political pre-eminence that Buenos Aires first enjoyed as viceregal capital in the late eighteenth century gained economic backing as the city became the country's major port, railhead, and financial emporium. For the world Argentina meant Buenos Aires. The other provinces remained vestiges of a colonial past, useful primarily for the production of crops or animals. The ambitious and talented, attracted by the opportunity and promise of this primate city, shook provincial dust off their shoes as quickly as possible.

The effect of these developments on Buenos Aires was reflected in a population growth from 300,000 to 1,300,000 and in a startling spread that spilled beyond the 400 blocks (2.5 square miles) built up by 1880 and reached to the limits of the federal district's 73.5 square miles by 1910 (see map). Census data clearly document the city's dependence on immigration to achieve this growth. The age pyramids and percentage distributions of the city's population constructed for 1887, 1904, and 1909 (see diagram 1 and table 1) demonstrate the tremendous contribution by the foreign born in the productive-age categories of 15 to 59. Initially the foreign-born influx had been heavily weighted in these categories toward male immigrants: the proportion of males to females stood at 2:1 in 1887. By 1904 and 1909, among foreign born in the 15 to 59 bracket the proportion of males to females had dropped to 1.3:1. Even more significant in an economy heavily dependent on manual labor was that foreign-born males in this highly employable age span outnumbered native born by 3.5:1 in 1887, 2.7:1 in 1904, and 1.7:1 in 1909.

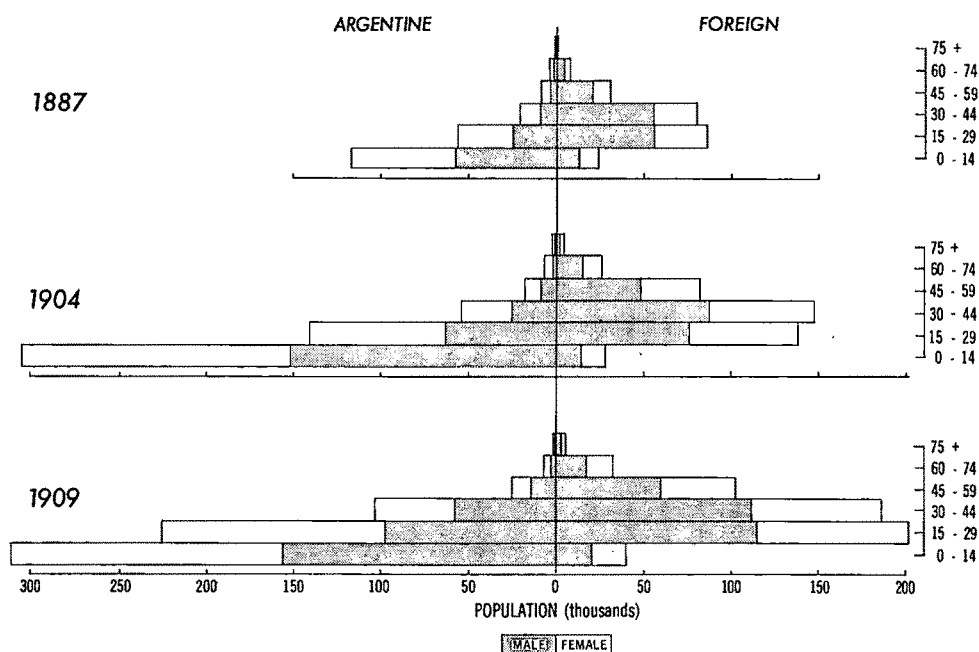
Commerce constituted the principal activity of Buenos Aires. Although the city had to wait until nearly the turn of the century before new

<sup>1</sup> Aldo Ferrer, *The Argentine Economy: An Economic History of Argentina*, tr. Marjory M. Urquidí (Berkeley, 1967), 229; Henry S. Ferns, *Britain and Argentina in the Nineteenth Century* (Oxford, 1960), 397.

<sup>2</sup> James R. Scobie, *Revolution on the Pampas: A Social History of Argentine Wheat, 1860-1910* (Austin, 1964), 169, table 1.

<sup>3</sup> In 1880 the struggle to determine the location of Argentina's national capital ended with legislation that established the federal district—the city of Buenos Aires endowed with generous boundaries to allow for expected urban growth. The subsequent expansion has led to the definition of Greater Buenos Aires with a population in 1970 of more than eight million, in which is included the federal district's three million.

DIAGRAM 1. AGE PYRAMIDS FOR THE CITY OF BUENOS AIRES, 1887, 1904, 1909<sup>a</sup>



<sup>a</sup> Compiled from the municipal censuses of Buenos Aires for 1887, 1904, and 1909.

TABLE 1. AGE DISTRIBUTION OF POPULATION IN BUENOS AIRES, 1887, 1904, 1909, BY PERCENTAGES<sup>a</sup>

|         | 1887             |         | 1904             |         | 1909             |         |
|---------|------------------|---------|------------------|---------|------------------|---------|
| Males   | Argentine        | Foreign | Argentine        | Foreign | Argentine        | Foreign |
| 0-14    | 23.3%            | 5.2%    | 30.8%            | 3.0%    | 23.9%            | 3.1%    |
| 15-29   | 10.1             | 22.6    | 12.8             | 15.4    | 14.8             | 17.5    |
| 30-44   | 3.6              | 22.7    | 5.2              | 17.6    | 8.8              | 17.1    |
| 45-59   | 1.4              | 8.4     | 1.7              | 9.7     | 2.2              | 9.1     |
| 60-74   | .5               | 1.9     | .4               | 2.9     | .4               | 2.6     |
| 75-     | .1               | .2      | .1               | .4      | .1               | .4      |
|         | 39.0%            | 61.0%   | 51.0%            | 49.0%   | 50.2%            | 49.8%   |
|         | 100.0% = 242,800 |         | 100.0% = 492,100 |         | 100.0% = 652,000 |         |
| Females |                  |         |                  |         |                  |         |
| 0-14    | 30.5%            | 5.8%    | 33.6%            | 3.1%    | 26.7%            | 3.3%    |
| 15-29   | 16.4             | 15.9    | 17.0             | 13.4    | 21.8             | 14.5    |
| 30-44   | 6.0              | 13.4    | 6.3              | 13.1    | 7.8              | 12.6    |
| 45-59   | 3.1              | 5.1     | 2.0              | 7.4     | 2.0              | 7.4     |
| 60-74   | 1.4              | 1.8     | .9               | 2.6     | .7               | 2.6     |
| 75-     | .4               | .2      | .2               | .4      | .2               | .4      |
|         | 57.8%            | 42.2%   | 60.0%            | 40.0%   | 59.2%            | 40.8%   |
|         | 100.0% = 189,900 |         | 100.0% = 452,800 |         | 100.0% = 579,700 |         |

<sup>a</sup> Calculations based on *Censo general de la ciudad de Buenos Aires de 1887*, 2: 37-42; de 1904, 2: 37-42; de 1909, 2: 37-42.

port works and channels overcame the handicaps of the estuary's shallow mud flats, the city easily maintained its role as the country's leading port, a position established at its foundation in the late sixteenth century. During the period from 1880 to 1910 approximately half of all overseas tonnage to Argentina entered the country through Buenos Aires, a total four to six times greater than its closest competitor, Rosario. Construction of the country's railroad network, accomplished largely during these same decades, confirmed this position of dominance. Through this railroad system passed both agricultural products destined for Brazil and Europe and manufactured goods from abroad.<sup>4</sup>

Shops and stores, always numerous for the city's size, kept pace with overall population growth and increased from 9,000 in 1887 to nearly 28,000 by 1914. In the three decades between these two censuses, while such establishments tripled in number, their value increased twelvefold. Industry lagged, with a capital investment usually half that in commerce. Most important, industrial facilities and employees were concentrated in meat packing, flour milling, wool washing, food processing, and construction, all of which supported the city's role as the entrepôt for an agricultural hinterland.

The city's dedication to bureaucratic activity cannot be measured as clearly, although its role and functions as the federal district after 1880 implied major involvement in national administration. The country's bureaucracy had long been centered at Buenos Aires. The city had served since 1618 as the seat of a governorship with authority reaching to Santa Fe and Corrientes in the north, and in 1776 it had become the capital of the viceroyalty of the Río de la Plata, which controlled all of Argentina as well as Bolivia, Paraguay, and Uruguay. During the disturbed, early years of independence Buenos Aires continued to be recognized as the center of authority. The final step, definition of a federal district in 1880, merely confirmed an establishment administrative function. Almost all political and economic decisions that affected national development were made in Buenos Aires. Here, after 1862, were the two houses of the national congress as well as the presidency, ministries, and supreme court. Men from the provinces came to the city, drawn by opportunities and offices. Repeatedly these individuals adopted Buenos Aires as their own. After 1880 the congress, largely composed of provincials, lavished funds to transform this large village of low, one-story buildings into Latin America's most elegant metropolis, a Paris of the Southern Hemisphere. National expenditures, devoted in substantial proportion to increasing the city's splendor and commercial-administrative importance, rose accordingly, from 44.5 million gold pesos

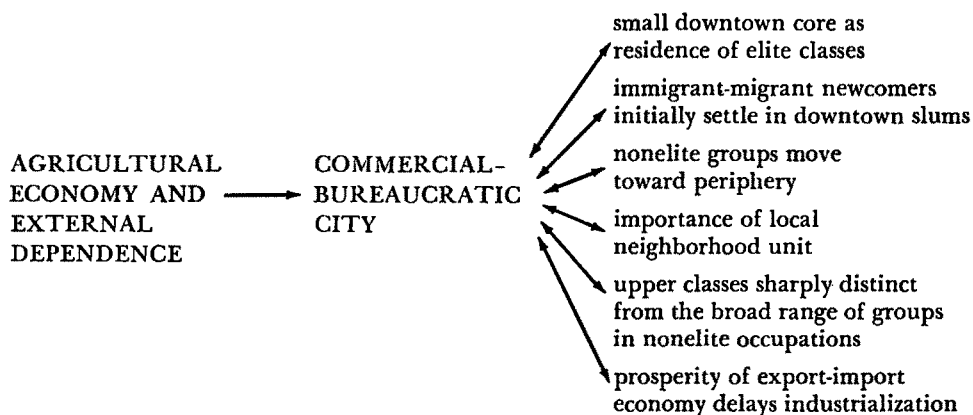
<sup>4</sup> See Scobie, *Revolution on the Pampas*, 40-41, for maps of the railroad network in 1880 and 1910.



spent annually in the first five years of the 1880s to 131 million annually between 1905 and 1909.<sup>5</sup>

Buenos Aires at the end of the nineteenth century can thus be defined as a commercial-bureaucratic city in which almost all economic activity was concentrated in commerce and government or in closely related and subordinated fields.<sup>6</sup> The expansion of the agricultural economy with its heavy dependence on external supplies of labor and capital and its nearly total reliance on outside markets for the sale of agricultural products stimulated the growth of a major administrative and exchange center. The particular cluster of characteristics that developed from the commercial-bureaucratic orientation in turn reinforced that trend. This is not to suggest that Buenos Aires held a monopoly on these urban traits. Cities with other types of specialization clearly possess some of these characteristics. It was, however, the combination of traits brought together for a period of time that helps to characterize this particular orientation. The maturation of Buenos Aires as a commercial-bureaucratic city can be presented in simplified schematic form, as in diagram 2.

DIAGRAM 2. DYNAMICS OF THE COMMERCIAL-BUREAUCRATIC CITY



The six characteristics outlined in this diagram are explored in the following pages as a basis for suggesting the impact of the commercial-bureaucratic experience on Argentine development and the possible application of the commercial-bureaucratic example to the study of other urban centers.

EVEN IN THE 1970s the central business district for the metropolitan area of Greater Buenos Aires, with its more than eight million inhabitants, is

<sup>5</sup> Calculated from tables on government expenditures in Ernesto Tornquist y Cía, *El desarrollo económico de la República Argentina en los últimos cincuenta años* (Buenos Aires, 1920), 279-80.

<sup>6</sup> See appendix, "A Note on the Nature of the Commercial-Bureaucratic City," pp. 1071-73.

limited to an amazingly small area of one hundred blocks (see map). The downtown core has shifted slightly northward since the colonial period but still contains the Plaza de Mayo, the heart of the city from its founding in 1580 (figure 1 shows the Plaza de Mayo and the Avenida de Mayo in 1898).

Characteristic of the Spanish city, and perhaps of the preindustrial city, is the physical location of those persons with prestige, wealth, and power around the principal plaza.<sup>7</sup> From the time of the foundation of Buenos Aires the fort, principal church, and municipal offices occupied positions on the Plaza de Mayo. The most important business and community leaders built their commercial houses and residences around the two open squares of the plaza. The intellectual and social center of the city, with the University of Buenos Aires established in 1821, the theaters, the elegant shops, and the more elite residences, developed at the south edge of the plaza and extended south for five or six blocks. As one moved away from the plaza the wealth and prestige of the residents tended to decrease until one reached open fields and small farm plots. At mid-nineteenth century these outskirts still encircled the city at distances of less than a mile from the plaza.

During the period from 1880 to 1910 the commercial and elite center of the city moved northward. By the end of the century it extended eight or nine blocks north and west from the Plaza de Mayo and only one or two blocks south. The colonial-style architecture, which gathered family and servants around three interior patios (see floor plan of colonial-style house in diagram 3), no longer satisfied the wealthy. This earlier type of architecture presented a wall to the outside world broken only by a heavy wooden door and two or three barred windows. Such dwellings sheltered the family and its way of life from outsiders, even from neighbors. By the end of the nineteenth century, however, the rich increasingly aspired to lavish and ostentatious three- and four-storied mansions. To acquire necessary space they abandoned much of the area south of the plaza to tenements or slums and built up the district of Barrio Norte,<sup>8</sup> fifteen to twenty-five blocks northwest of the plaza (figure 2 shows an established street of elite residences, Avenida Alvear, around 1900).

Despite this slight northward adjustment the affluent clung to the city's center, in large measure because of the extraordinary spatial concentra-

<sup>7</sup> Particularly useful in understanding Latin American urban development is the extensive work done by Richard M. Morse. His bibliographical articles in the *Latin American Research Review* are especially pertinent: "Recent Research on Latin American Urbanization: A Selective Survey with Commentary," vol. 1, no. 1 (1965): 35-74; "Trends and Issues in Latin American Urban Research, 1965-1970," vol. 6, nos. 1 and 2 (1971): 3-52, 19-75, respectively. Also helpful is Morse's "Some Characteristics of Latin American Urban History," *AHR*, 67 (1961-62): 317-38.

<sup>8</sup> The use of the word "barrio" in the name of this district, which covered approximately sixty city blocks, has nothing to do with the small neighborhood unit of two to five blocks discussed later on pages 1052-56.

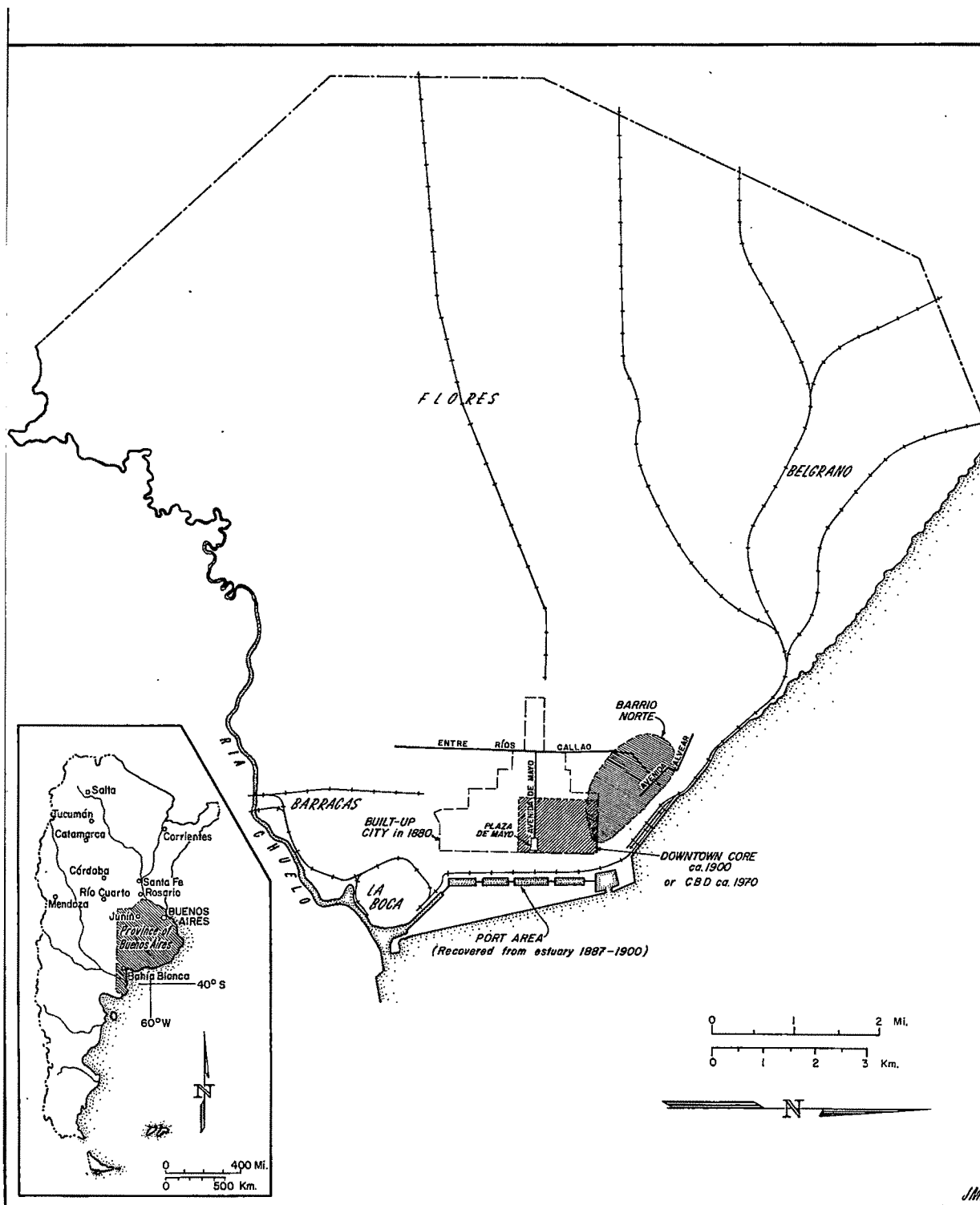
tion of commercial, financial, administrative, social, and intellectual activities in the downtown core. In a commercial-bureaucratic city, housing and services designed primarily for the elite classes tend to be concentrated at the core, partly for the convenience and economy of those classes and partly because the smallness of the elite allows for such compactness. Buenos Aires has often been compared to Goliath's head, and many have attributed Argentina's problems to the fact that a sprawling nation is dominated by a single primate city. But as the *Revista Municipal* noted in 1910, "The comparison also has application within the city of Buenos Aires itself, which suffers from the same defect of an enormous extension served by a tiny nucleus at its center where an attempt is made to concentrate all urban activities."<sup>9</sup> The financial affairs of the nation—in the stock exchange, cereal exchange, and the country's twenty-two major banks—were conducted along a few blocks of San Martín and Bartolomé Mitre, two blocks from the Plaza de Mayo (figure 3 shows a section of this financial district around 1910). When the city's first large department stores, Gath y Chaves and A la Ciudad de Londres, opened at the end of the nineteenth century, each occupied several-storied buildings only a block from the plaza. The national congress, executive office building, ministries, law courts, and municipal and national offices were all located on the plaza or within a block's distance.<sup>10</sup> Other activities also crowded into the core. Wholesale trade remained concentrated in the blocks adjoining the plaza, despite increasing problems of storage and traffic. Not until the decade of the 1910s did the law and medical faculties move from the core to a location twenty blocks northwest of the plaza. Theaters and social clubs likewise remained close to the plaza. The first two elite clubs, the Club del Progreso and Club del Plata, both established in the 1850s, were located just to the south of the plaza. When these were supplanted in the 1880s by two new prestige clubs, the Jockey Club and the Círculo de Armas, the preferred locations had shifted several blocks to the north. The opening in 1908 of the magnificent Colón Opera House nine blocks to the northwest of the plaza served to enlarge slightly the social and cultural core but did not change the pull that the center exerted on the city's well-to-do.

While the concentration of these urban and frequently national activities may well have contributed to the elite's desire to remain near the downtown core, the fact that this core provided services, offices, and often residences primarily for the upper classes but not for the mass of the *porteño* populace<sup>11</sup> explains how such concentration was feasible. The

<sup>9</sup> *Revista Municipal*, Aug. 15, 1910, p. 11.

<sup>10</sup> A new building for the national congress was erected at the opposite end of the mile-long Avenida de Mayo from the Plaza de Mayo. The congress moved its sessions from the old building on the Plaza de Mayo to this new location in 1906.

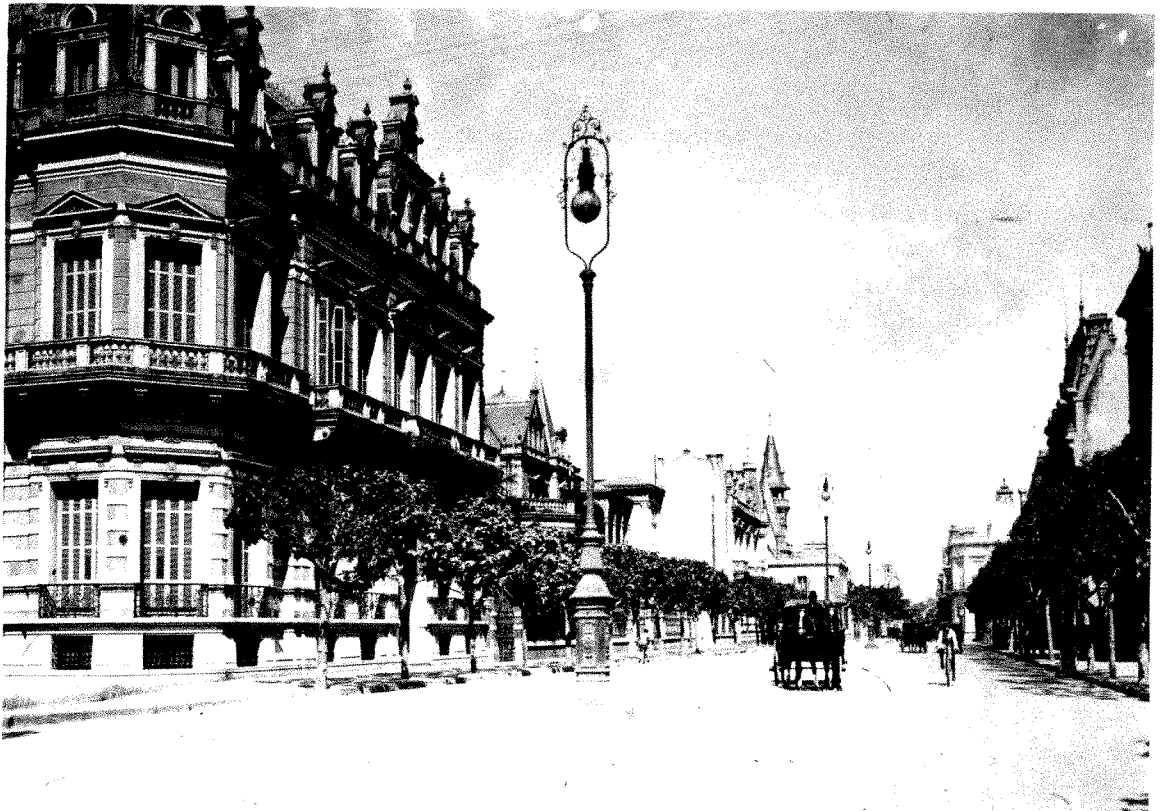
<sup>11</sup> The term *porteño* is used as a noun or adjective to describe association with the city of Buenos Aires.



THE FEDERAL DISTRICT OR THE CITY OF BUENOS AIRES, 1880-1910 (WITH INSET OF ARGENTINA)



*Fig. 1.* The Plaza de Mayo and the recently completed Avenida de Mayo in 1898, looking west from the Casa Rosada, or executive office building. Photograph courtesy of the Archivo General de la Nación, Archivo Gráfico, Colección Sociedad Fotográfica Argentina de Aficionados (hereafter AG, SFAA).



*Fig. 2.* An elite residential area along Avenida Alvear in Barrio Norte, ca. 1900. Photograph courtesy of the Archivo General de la Nación, AG, SFAA.



Fig. 3. The financial district, ca. 1910, corner of Bartolomé Mitre and Reconquista looking west along Bartolomé Mitre. Photograph courtesy of the Archivo General de la Nación, AG, SFAA.

number of *porteño* men in the upper classes increased from 7,000 in 1887, to 11,000 in 1895, to 16,000 in 1904, to 20,000 in 1909, and to 27,000 by 1914, but remained constant at 4 to 5 per cent of the total employed male population in the city (see table 2 and the discussion of social structure, pp. 1056–64).<sup>12</sup> When these figures are divided by a factor of at least four to compensate for extended families, the evidence suggests that the upper classes were constituted by fewer than 2,000 families in 1887 and approximately 7,000 families by 1914. Such numbers clearly could be accommodated in areas north of the Plaza de Mayo and in Barrio Norte without creating major pressures or problems. In 1910, therefore, the elite still looked to the downtown core as its place of business and pleasure and frequently as home. For these individuals this was the city.

<sup>12</sup> The principal sources for census materials used in this article are the published reports of national and municipal censuses: *Censo nacional de 1869* (Buenos Aires, 1872); *Censo general de la ciudad de Buenos Aires de 1887*, 2 vols. (Buenos Aires, 1889); *Censo nacional de 1895*, 3 vols. (Buenos Aires, 1898); *Censo general de la ciudad de Buenos Aires de 1904* (Buenos Aires, 1906); *Censo general de la ciudad de Buenos Aires de 1909*, 3 vols. (Buenos Aires, 1910); *Censo nacional de 1914*, 10 vols. (Buenos Aires, 1916–17).

DESPITE THE PRESTIGE attached to the center of a commercial-bureaucratic city, pockets of slum settlement inhabited largely by recent arrivals develop at the heart of the city in a fashion similar to that in many other types of urban centers. In these slum settlements the newcomers benefit from low rents in high-density housing and closeness to work while they acquire familiarity with the city and its ways. Thus in Buenos Aires the downtown area served as an initial place of residence for many of the immigrants who entered Argentina.

The *porteño* population growth at the end of the nineteenth century was extraordinary. To a population of 300,000 in 1880 was added one million by 1910. A trickle of migration from the Argentine provinces; a rapid fall in the death rate after 1900 because of improved public health and sanitation; a continued high birthrate, especially among immigrant women; and, most important, a massive influx of immigrants from Southern Europe—all of these contributed to the demographic explosion. The largest group, the recently arrived European laborers, gravitated to the urban core and settled in slum housing. Some of these tenements, or *conventillos*, were interspersed in the same block as the homes of more accommodated groups; others were concentrated in areas abandoned by the wealthy in their northward push.

The only two censuses that record *conventillo* dwellers, those of 1887 and 1904, indicate a concentration of tenement settlement adjacent to the Plaza de Mayo. Twenty-seven per cent of the 193,000 individuals in the three census districts surrounding the Plaza de Mayo, an area extending sixteen blocks west, thirteen blocks south, and eight blocks north of the plaza, occupied slum housing in 1904. The smaller census districts used in 1887 reveal that this proportion increased to one-third or more in the areas immediately to the south of the plaza and along the waterfront southeast and northeast. The number of slum dwellers declined as one moved away from the downtown core and virtually disappeared at a distance of forty blocks from the plaza. According to the 1887 census, foreign born constituted 72 per cent of the slums' inhabitants. If one could allow for the children born in Argentina to foreign parents as well as for recently arrived migrants from the provinces, the percentage of new arrivals more likely approximated 90 or 95 per cent.

These slums shared the characteristics of high-density, low-income settlements of inner cities elsewhere. The *porteño* slum dated from the mid-nineteenth century when the increasing need for manual laborers at the city's center and port and the rising tide of immigrants showed that workers and their families could be profitably crammed into small spaces. As old colonial-style buildings constructed around two or three interior patios deteriorated and became less desirable as single-family dwellings, rooms that opened off the patios were rented to families or groups of single men. Gradually property owners devised more profitable ways to divide up

space. In the highest density type of construction a corridor ran from the street entrance to the rear of the building. Partitions and extensions on both sides of the corridor filled most of the patio space with rooms measuring roughly twelve feet by twelve. Thus the colonial-style house that had been typically occupied by a family of twenty-five (fifteen members and ten servants) served 340 persons when subdivided (see diagram 3). Profits were so high—estimated at 3 to 4 per cent per month—that by the late 1860s buildings were constructed specifically for this purpose.<sup>13</sup>

Descriptions from the beginning of the twentieth century depict the conditions of *conventillo* life.<sup>14</sup> The one-room dwelling with forty to one hundred cubic yards of air served a family with two to five children or a group of four or five single men. At the door to each room stood a pile of wooden boxes. One generally held a basin for washing; another a charcoal brazier on which to cook the daily watery stew, or *puchero*; and garbage accumulated in a third. Two or three iron cots, a pine table, a few wooden chairs, an old trunk, perhaps a sewing machine, and more boxes completed the furnishings. Light came from the open door and one window, from an oil or gas lamp, or occasionally from a bare electric light bulb. On the once-whitewashed walls were tacked pictures of popular heroes, generals, or kings torn from magazines, an image of the Madonna and a couple of saints, perhaps a faded photograph of family members in Europe. The women often eked out miserable incomes by taking in laundry and washing and drying it in the patios. Others ironed or sewed on a piecework basis. Some men worked here: in one corner a shoemaker might ply his trade, in another a man might bend over a small table repairing watches. The great majority of men left at 4:30 on summer mornings and at 6:00 on winter days and returned twelve hours later after strenuous labor on the docks and construction sites.

Municipal authorities waged a constant struggle with the *conventillo* owners, "who are the richest and most respected men of Buenos Aires."<sup>15</sup> By 1900 regulations ruled that *conventillos* had to be above street level, receive a yearly whitewashing, have a concrete patio at least fifteen feet wide, contain latrines and baths, and include one window plus a door in each room. Rooms, if anything, decreased in size, baths remained a rarity, and a latrine served as many as seventy persons. But there was an undeniable improvement in the water supply and in sewage disposal, which was reflected in the falling death rate at the city's center.

Newly arrived laborers in the city thus adjusted to the environment of Buenos Aires largely through the *conventillos* located in or near the down-

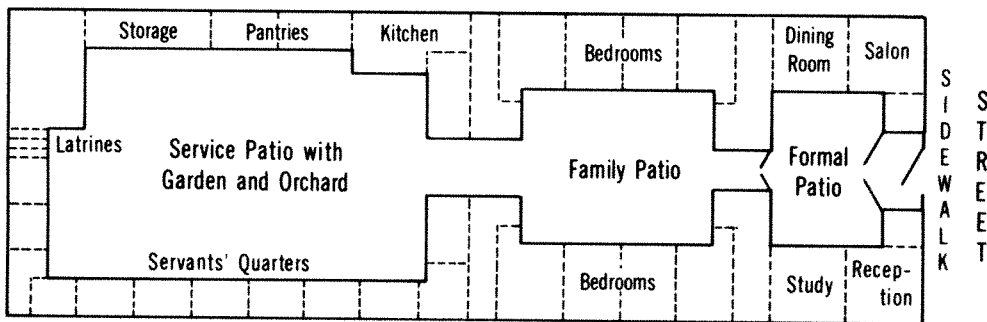
<sup>13</sup> *La Prensa*, Sept. 8, 1901, p. 4.

<sup>14</sup> Particularly vivid descriptions of *conventillo* life and conditions are found in *Boletín del Departamento Nacional del Trabajo*, no. 5 (1908): 230-40; Adrián Patroni, *Los trabajadores en la Argentina* (Buenos Aires, 1898), 126-33; Guillermo Rawson, "Estudio sobre las casas de inquilinato en Buenos Aires," *Escritos y discursos*, 2 vols. (Buenos Aires, 1891), 2: 110-14, 142-44; and Samuel Gáché, *Les logements ouvriers à Buenos-Ayres* (Paris, 1900).

<sup>15</sup> *La Prensa*, Jan. 3, 1874, p. 1.

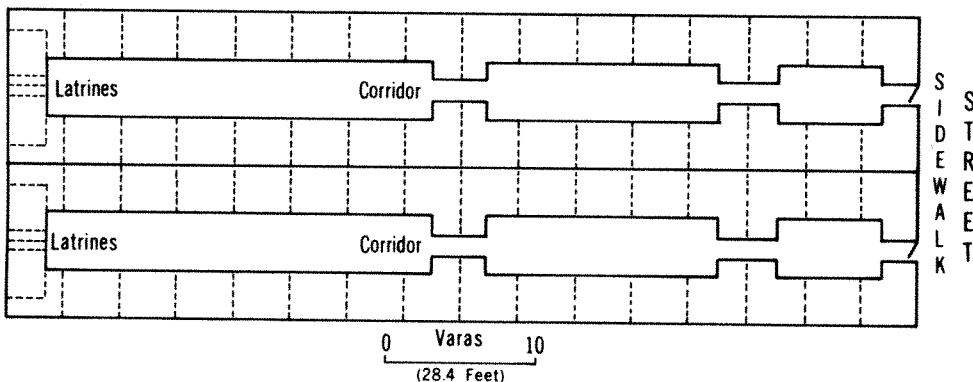


DIAGRAM 3. COLONIAL-STYLE HOUSE AND ITS CONVERSION INTO A CONVENTILLO, CITY OF BUENOS AIRES



COLONIAL-STYLE HOUSE: Built on lot 20 varas by 60 (56.8 feet by 170.4), occupied normally by 25 persons (15 members of the family and 10 servants).

CONVENTILLO: Formed on the same city lot, but with 68 rooms, each normally housing 5 inhabitants, or a total of 340 persons.



town core. Some of the poor and humble, particularly the native-born migrants from the interior provinces, also ringed the city's outskirts with their shacks and thus continued a pattern established in the colonial period. Swampy or frequently flooded areas, public domain slated for port works, and localities far removed from transportation facilities provided these squatters with land. The huts might vary according to location and time from straw-thatched roofs and mud-plastered walls to corrugated iron sheets and flattened oil cans, but they all shared remoteness from rent and tax collectors (figures 4 and 5 show shacks in the port area and the interior of a hut around 1900). The graphic term of *cinta negra*, or "black belt," did not emerge until years later, but the description printed in the *Revista Municipal* in 1896 vividly suggests the environment of these outlying squatter settlements.

The stream bed of Maldonado, the marshes of Flores, the slaughterhouse district, the Riachuelo stream, La Boca district, and the lagoons in the port zone surround the central city like a chain, the links of which are formed by morasses,

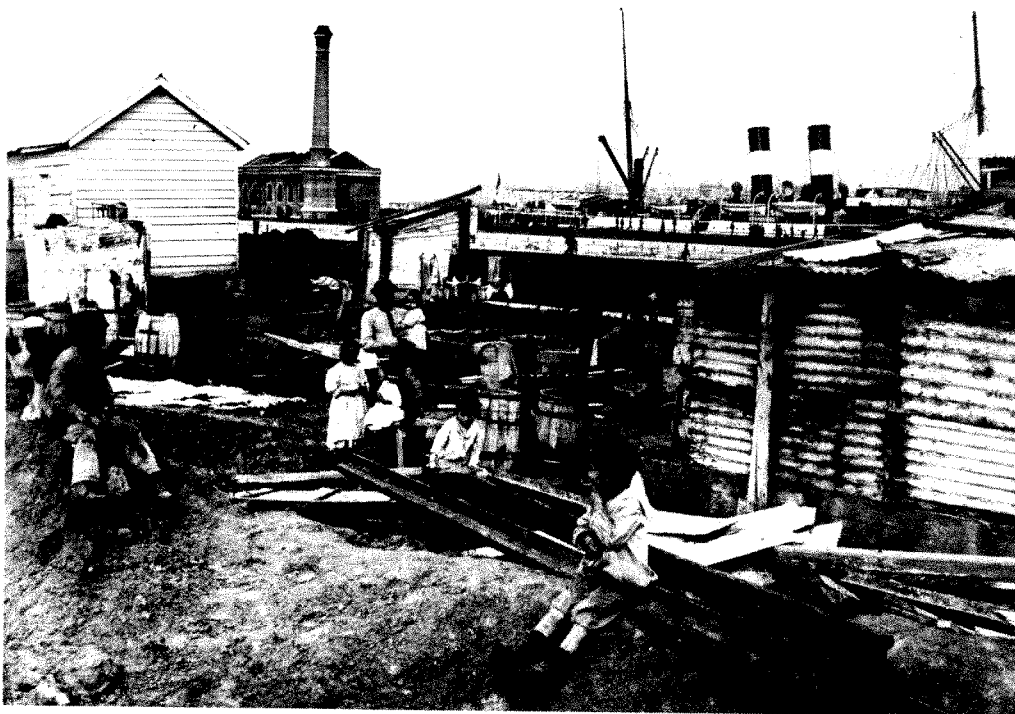
bogs, pools of stagnant water, and piles of garbage, reinforced by a rosary of workshops, small plants, and other establishments that have no means to dispose of their wastes without endangering public health.<sup>16</sup>

THE CITY'S MOST IMPRESSIVE spatial growth occurred in the areas between the remote and undesirable outskirts and the downtown core. This striking transformation of the landscape emerges graphically in two photographs (figures 6 and 7), taken from the same spot but at an interval of thirty years, of the main avenue leading south from the downtown core toward Barracas. The initial experience of Buenos Aires as a "walking" city had been to push outward in a rough semicircle from its center at the Plaza de Mayo. The building of railroads in the 1860s and of streetcar lines after 1870 distorted this expansion with three prongs, one south from the center toward the coastal trade and fishing operations in La Boca, one due west from the plaza to Flores, and the other north along the estuary to Belgrano. The subsequent addition of two more major railroad lines in the northwestern section of the federal district further attracted population in this direction. Built-up blocks gradually replaced the small farms and open fields that until the 1880s had separated these villages from the central city.

The city's highest population density in the 1880s occurred in an area well served by streetcar lines, a band several blocks wide and some fifteen blocks long located eight to ten blocks west of the Plaza de Mayo. This band moved outward, expanding slightly in width and length, until by 1910 it stabilized immediately west of the streets of Entre Ríos/Callao, fifteen blocks from the plaza. From this area and from the *conventillos* that were located even closer to the downtown core came the men and women who helped extend Buenos Aires from a one-mile walking radius in 1880 to built-up zones scattered throughout the federal district by 1910, some as much as ten miles from the Plaza de Mayo.

In the commercial-bureaucratic environment the masses of blue-collar-white-collar workers, not drawn together by concentrations of industry and lacking the class consciousness and the access to lineage, education, and wealth monopolized by the upper classes, seek their own kind of upward mobility and security in ownership of individual house lots and homes remote from the downtown core. The outward expansion of Buenos Aires thus owed much to the push of the common man toward better living conditions and cheaper land or rents on the urban periphery. Once established by several years' residence in a *conventillo* or in other high-density housing near the downtown core, the immigrant or migrant

<sup>16</sup> *Revista Municipal*, Jan. 8, 1896, p. 2. A commentary on the *cinta negra* appears in Alcides Greca, *Problemas del urbanismo en la República Argentina* (Santa Fe, 1939), 34-35.



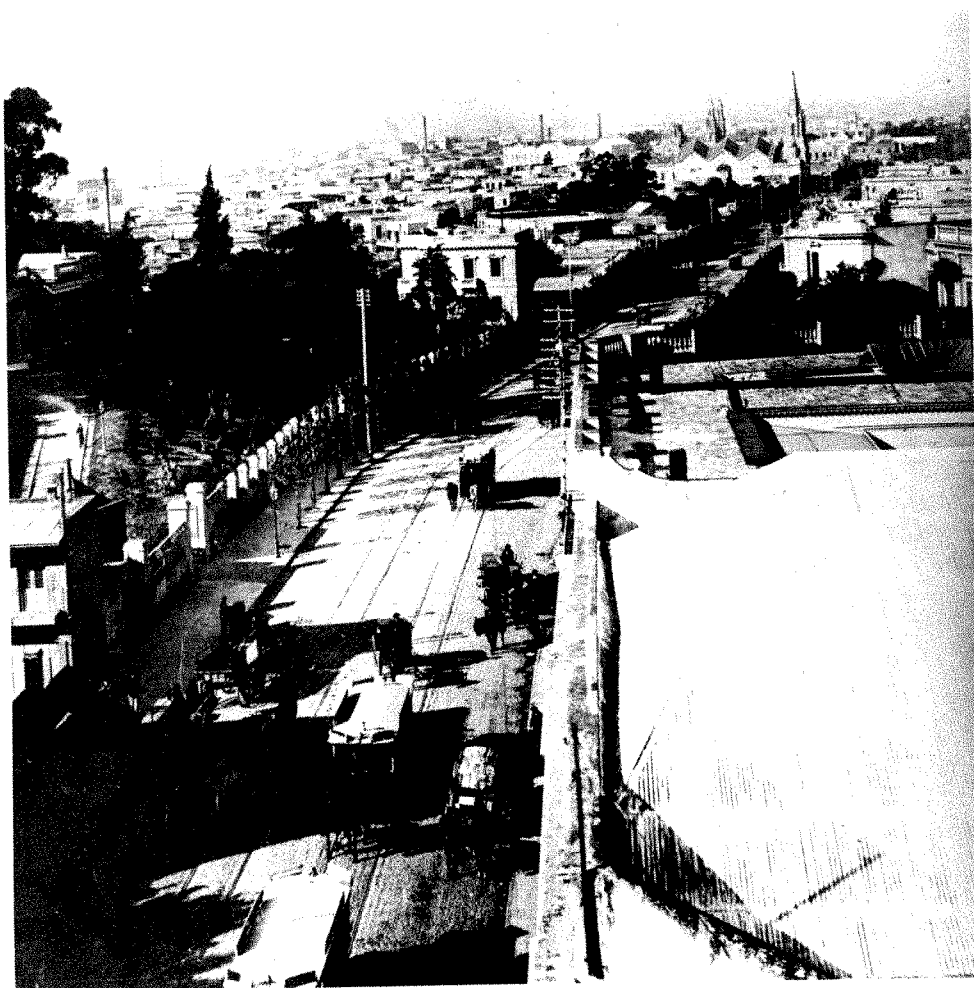
*Fig. 4.* Squatter shacks in the port area of Buenos Aires, ca. 1905. Photograph courtesy of the Archivo General de la Nación, AG, SFAA.



*Fig. 5.* Interior of a squatter hut, ca. 1900. Photograph courtesy of the Archivo General de la Nación, AG, Colección Witcomb.



*Fig. 6.* A view taken ca. 1875, looking south toward Barracas along Calle Larga (subsequently renamed Avenida Montes de Oca), the main street linking this suburb to the central part of Buenos Aires. Photograph courtesy of the Archivo General de la Nación, AG, Colección Witcomb.



*Fig. 7.* A view taken in 1905, from the same spot as figure 6, looking south toward Barracas along Avenida Montes de Oca. Photograph courtesy of the Archivo General de la Nación, AG.

laborer began to aspire for his plot of land on the outskirts.<sup>17</sup> So did the sons and daughters, the second generation now tied to Argentina and to Buenos Aires by birth, education, and environment. These were the individuals who sought security and mobility in one- to three-room houses along the dirt streets that sprouted for six or seven blocks on each side of streetcar or railroad lines. As with the squatter settlements, however, cheap lands and easy terms of payment also brought penalties in the form of streets that became quagmires of mud when it rained, stagnant ponds of slaughterhouse residue, a nearly total lack of police protection, and the noise, dust, smoke, and smell of tanneries, distilleries, brick factories, and sawmills.

Two major economic booms, 1884-89 and 1905-12, encouraged the construction of modest worker dwellings as well as of public works, warehouses, railroads, mansions, and chalets. Real wages for the unmarried laborer, especially the skilled worker, or for the family with several teen-aged and consequently employable children, increased slightly between 1905 and 1912. Fortunate workers could put aside as much as two or three days of wages per month. Home ownership thus came within the grasp of a number of workers. A suburban lot at the beginning of the twentieth century could be paid for with two hundred days of labor by a skilled mechanic, and a one-room house built for the equivalent of one hundred days of work, totaling an investment of roughly twelve to fifteen years of savings.<sup>18</sup> Auctioneers and landowners amassed fortunes from the sale of low-lying, often marginal lands on the city's fringe, but after 1900 they provided opportunities to purchase tiny lots with small monthly payments over six, eight, even twelve years. The rapid expansion of population and housing in census district number 1, which covered the whole southwest corner of the federal district with its nearly twenty-one square miles, gave evidence of those who advanced their social standing. Population had increased in this district from 17,000 in 1904, to 48,000 by 1909, and to 103,000 by 1914; of the 7,600 houses in 1909, 4,500 had three rooms or less; by 1914, out of a total of 13,900 houses, 9,300 fell into that modest category.

The move to the outskirts, occasionally by renting but more often through ownership of lot and house, brought personal improvements. The modest home, especially if built by the owner himself on holidays and Sundays, typically began with one room located five to ten yards back from the street. At one end was the kitchen, consisting merely of a brazier and a

<sup>17</sup> Additional investigation is needed to link this dream of land on the outskirts to the social and reform ideologies present in Buenos Aires. The suggestions that emerge from Sam B. Warner, Jr., *Streetcar Suburbs: The Process of Growth in Boston, 1870-1900* (Cambridge, Mass., 1962), cast light on this process in the United States and raise questions that still need to be answered in the case of Buenos Aires.

<sup>18</sup> Calculated on the basis of a daily wage of 2.50 gold pesos, the average for a skilled workman in the period from 1904 to 1912, and a monthly savings of two days' wages. Average suburban lots ranged in price from 200 to 500 gold pesos, while a one-room house could be built for the equivalent of 200 to 250 gold pesos.

wooden shelf with some pans and dishes. Toward the back of the lot a well was sunk to the depth of fifteen or twenty feet, while close to the house, and unfortunately too close to the well, was a latrine. At that stage it might appear that little had been gained over the *conventillo*, and indeed the conviviality of close neighbors, the bustle of downtown streets, and the closeness to work had been lost. But within a decade, with the lot paid for and the labor of his oldest children to swell the family income, this rising member among *porteño* blue-collar-white-collar workers could add two more rooms, usually bedrooms. In the second decade, with his younger daughters aspiring to some degree of social advancement, he might build a living room on the front of the house and bring his front door and windows flush with the recently constructed sidewalk.<sup>19</sup> Improved transportation, especially electrification and unification of the streetcar system between 1900 and 1910, speeded the outward movement. The *conventillo* provided a place to adapt to Argentine culture and customs. But one had to leave the *conventillo* in order to move upwards socially. Each immigrant wave—the French in the 1850s, the Italians in the 1870s, and the Spanish in the first decade of the twentieth century—occupied downtown *conventillos* for a decade or so. Once the adjustment had been made, the newcomer or his children were usually on their way toward the outskirts where lands or rents were cheap and where the dream of being a landowner and householder might be realized.

BECAUSE OF THE TENDENCY of a commercial-bureaucratic city to sprawl outward as the common man searches for cheap land and because the principal function of the downtown core is to serve the upper classes, the local neighborhood acquires particular significance and importance for the mass of the population. The outward expansion of Buenos Aires was really built around tiny neighborhoods that provided the social, psychological, and economic focus for the common man in much the same fashion that the downtown core served the upper classes. No censuses or statistical reports survey the neighborhood unit, since the *barrio* rested merely on the relationships and attitudes of its inhabitants. Only where isolation or ethnic characteristics imposed visible boundaries or distinctive atmosphere could the components of the *barrio* be measured. Beyond that the researcher must rely on local reminiscences and draw conclusions from still existent *barrio* phenomena in the city.<sup>20</sup>

<sup>19</sup> Manuel de Oliveira Lima, *En la Argentina: Impresiones de 1918-19*, tr. Valentín Diego (Montevideo, 1920), 99-100.

<sup>20</sup> Treatment of some of these characteristics in an outlying and isolated *barrio* appears in the recollections of Lucas Benítez, "*Los Olivos*:" *Barracas al Norte, 1895-1960* (Buenos Aires, 1965). Contemporary newspaper accounts of conditions in the compact settlements or ghettos of Russian Jews and of Syrians, or *turcos*, add further insights. It is nearly impossible, however, to present the *barrio* phenomenon in precise, measurable terms. Some understanding of its existence emerges from examining the manuscript census returns for 1869 and 1895 (see

The area of a barrio was small (see diagram 4). It rarely exceeded five blocks and more usually consisted of two or three; yet its population might range from fifty persons to three thousand. The elementary school provided a common denominator for a group of barrios, but school districts did not outline the barrio's area. Churches likewise served large sections of the city, and a parish might encompass from five to as many as fifty barrios. Butcher shops, grocery stores, and bakeries afforded meeting places for women and stimulated the sense of neighborhood. The grocery store, or *almacen*, especially in outlying areas, served as credit institution, post office, and social center. Here the women shopped, and the men gathered to exchange small talk or to read a newspaper. The area from which a store drew clientele provided, however, only one of several measurements to define the barrio. In more developed areas bars or cafés gave another basic link. For the workingman these were his clubs. The social communication and importance attached to these cafés by common people were as vital as membership in the Jockey Club was to the *porteño* elite.

The attachments to the barrio, however, existed mainly at a surface level. The barrio brought with it little sense of cooperative effort or close personal contact between its inhabitants. Neighbors might gossip at the local store counter, or on hot summer evenings they might pull chairs out to their sidewalk entrance and exchange salutations and perhaps a few pleasantries with adjoining households. But the patio style of architecture and the sheltered and guarded home life that had characterized the affluent classes until the 1880s continued to be followed by the artisan, skilled-labor, and white-collar masses (figure 8 shows the street and house fronts of an established and prosperous barrio around 1900). While the three-patio design was not strictly adhered to, the result was a home that enclosed its members rather than turning them outward toward the street. Surveyors still used the earlier measure of ten *varas*, or 8.66 meters (28.4 feet), as a standard frontage. House lots, even when carved out of open fields, emerged as elongated rectangles with a depth three to six times the frontage. The owner tended to enclose this lot with an adobe or brick wall, which ran along the boundary lines, and to construct his building so the front door would open on the sidewalk. The inhabitants of a barrio, therefore, knew each other superficially and only infrequently entered one another's homes. Significantly, common action or cooperation to secure sewers, gas, electricity, or other local improvements occurred rarely and only when generated throughout a parish or group of barrios by some business or political spokesman.

The barrio, nevertheless, brought a type of village atmosphere to the

note 21 below). Its characteristics can be grasped by those who have lived in such small neighborhood units. Why does one patronize a grocery store at one end of the block, yet never enter another at the opposite end of the same block? Or why does one always go to one of two equidistant cafés? The barrio's description, therefore, owes much to personal experiences and local reminiscences.

DIAGRAM 4. SCHEMATIC PLAN OF A TYPICAL PORTEÑO BARRIO

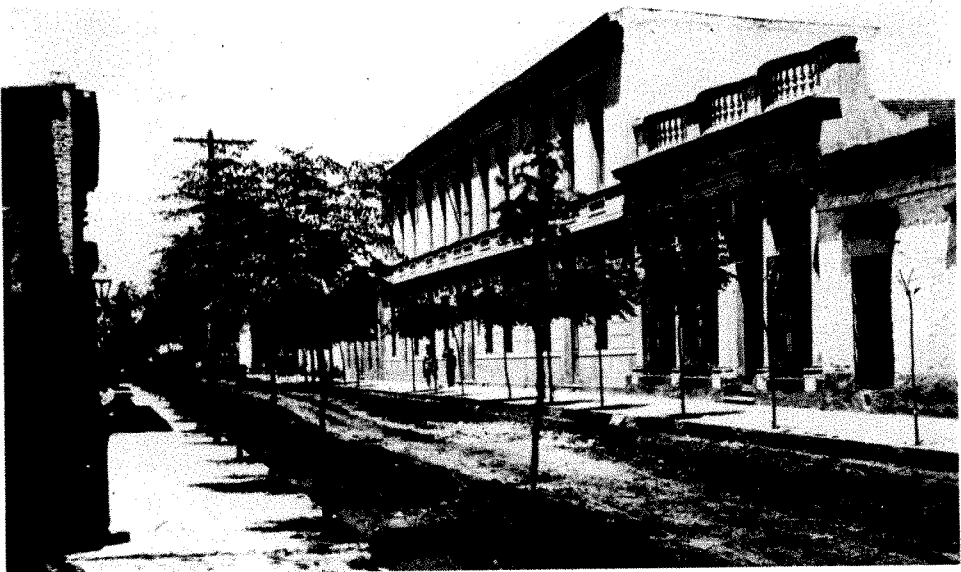
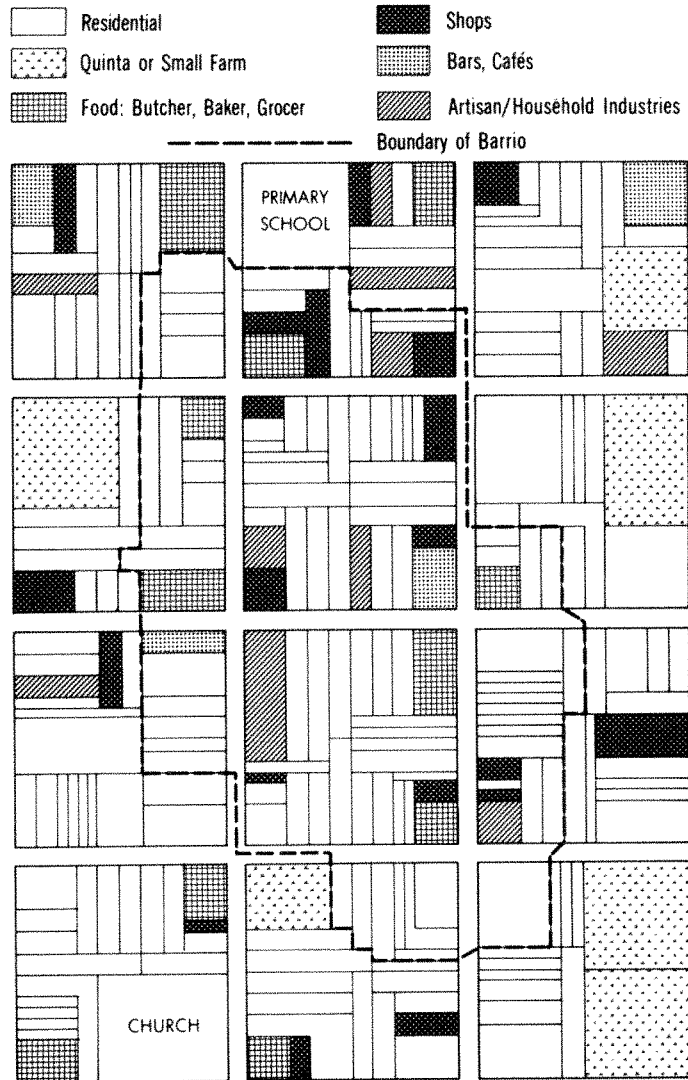


Fig. 8. An established barrio, ca. 1900, with sidewalks and substantial housing.  
Photograph courtesy of the Archivo General de la Nación, AG.



city. Before 1910 the place of work for shopkeepers and most skilled laborers and artisans was located in the same *barrio* as their residences. Others, particularly white-collar workers and day laborers, traveled to the central business district or port area to work but turned to the *barrio* for all other needs. The *barrio* protected the immigrant and native alike from the impersonality and tensions of a metropolis. In much the same way that the *conventillo* bridged the gap from the Old World to the New for many immigrants, so the *barrio* afforded attachments, amusements, and amenities in tiny units over the whole city. Individuals belonged to a particular *barrio* and only secondarily and in minor degree to a parish or the city. Many persons, especially women, rarely left the several blocks of their neighborhood.

The *barrio* changed continually with the rapid expansion of the city. It first began to display an ethnic character during the massive European immigration to the Río de la Plata that started in the 1860s. Within *barrios* subneighborhoods developed, often consisting merely of several adjacent households of Piedmontese, Neapolitan, or Genoese immigrants or of Asturian, Catalan, or Basque newcomers. Occupational groups tended to reproduce a similar pattern of subneighborhoods. Unpublished census returns tell of clusters of unskilled manual laborers, of artisans and skilled laborers, and of white-collar employees.<sup>21</sup>

By 1910 the *barrio* had reached the height of its psychological, social, and economic importance. In the vast reaches of Buenos Aires there remained innumerable small units, often possessing imprecise or changing boundaries and loyalties. The neighborhood workshop, grocery store, butcher shop, bakery, and café continued to play important roles in the lives of *barrio* dwellers. Local gossip, children's play, and elementary schooling still provided contact and satisfaction. One could stroll along shade-covered and mud-rutted streets within a few blocks of any of the city's thoroughfares and feel years removed from the metropolis. But the city had begun to supplement the *barrio*. New forces—increased educational facilities, broadening occupational opportunities, improved transportation, large downtown stores, and popularized entertainment—drew individuals out of the *barrio*. With the low, ten-centavo fare on streetcars, resulting from electrification and unification of the streetcar system in the first decade of the twentieth century, the common man could range further afield in search of work. He also on occasion took his family outside the *barrio*. The laboring masses, although only when dressed in their best

<sup>21</sup> These characteristics do not emerge in the published census returns because the size of census districts is too large, ranging from the smallest district in 1869 and 1887, which encompassed forty blocks or one-quarter of a square mile, to the largest district in 1895, 1904, 1909, and 1914, covering twenty-one square miles. Visual inspection of the manuscript census returns for 1869 and 1895, just recently uncovered in the Archivo General de la Nación, provides the basis for these statements. I am starting systematic analysis of this data, especially for the blocks closest to the Plaza de Mayo, in order to establish the spatial and social characteristics of the city's growth.

clothes, began to stroll the downtown avenues. The huge sums spent on horse races and other forms of gambling, the gradual improvement of the *porteño* parks, the audiences drawn to circuses, musical reviews, theatrical farces, lectures, and the increasingly popular Sunday pastime of marches and demonstrations all suggest that the *barrio* was losing some influence in the lives of its residents.

But even with such changes the *barrio* retained certain aspects of its vitality. In the Buenos Aires of the 1970s the downtown center attracts *porteños* of many backgrounds and social classes for business or amusement. The growth of secondary centers in the suburbs provides additional and often more convenient places for the concentration of marketing and entertainment facilities. But most city dwellers still attach themselves to an area of several blocks, which contains the institutions they most frequently use and where they feel at home.

THE DEDICATION TO TRADE and administration within a city requires a broad spread of occupations from manual through blue-collar-white-collar. The outward sprawl of the commercial-bureaucratic city, the absence of concentrated industrial zones, and the opportunity for social mobility throughout a whole range of occupational categories tend to postpone the sense of belonging to a particular class or classes among nonelite groups. The small numbers of elite individuals needed to control land, commerce, and government are closely interrelated by family ties. They possess, furthermore, a virtual monopoly on higher education and sizable fortunes as well as a clear class consciousness separating them from the rest of society.

In Latin America the application of a three-class model—upper, middle, and lower—has long troubled historians as well as political scientists and sociologists.<sup>22</sup> Although such categories seem to have some value once industry has developed or subgroups become conscious of common interests, the categories tend to confuse understanding of late nineteenth-century and early twentieth-century social realities. Even today these categories have little relevance to Paraguay, Haiti, and several of the Central American republics. More appropriate to the late nineteenth-century *porteño* scene, therefore, are terms that point to the sharp social distinction between those with family, education, and wealth, namely the upper classes, and the vast range of occupational strata filled by the common man or worker.

The upper classes possessed class consciousness and saw themselves

<sup>22</sup> John J. Johnson, *Political Change in Latin America: The Emergence of the Middle Sectors* (Stanford, 1958), has applied the model with some misgivings and difficulties to the Latin American scene. Works such as Gino Germani, *Estructura social de la Argentina* (Buenos Aires, 1955), and his *Política y sociedad en una época de transición de la sociedad tradicional a la sociedad de las masas* (Buenos Aires, 1962); Claudio Veliz, ed., *Obstacles to Change in Latin America* (New York, 1965); and Irving L. Horowitz, ed., *Masses in Latin America* (New York, 1970), suggest further problems with the application of such a model.

as those who controlled Argentina. Below them, however, it is difficult to discover any homogeneity, common interests, group identification, or even any realistic subdivisions. The common man in Buenos Aires knew he did not belong to the upper classes, but he rarely considered himself a member of any class category.<sup>23</sup> He fitted into a particular occupational stratum. Between strata there might be considerable differentiation in training, income, and status. But there also existed considerable mobility.

In the Argentine context, therefore, the best classifier of social structure is occupation. It reflects the primary factor of differentiation, the family, which in turn largely determines other components such as education and wealth. Occupation also can be readily extracted from published census reports.

The professional, administrative, and owner groups of society constituted the upper classes. Within this elite fell the lawyers and other professionals such as physicians, architects, engineers, and accountants; students at university and *colegio* (roughly high school-junior college) level; ranchers and cattlemen; owners of large industrial and commercial establishments; and heads of sections or offices in the government bureaucracy.<sup>24</sup> The occupational breakdown for the male population over fourteen years of age taken from the censuses of 1887, 1895, 1904, 1909, and 1914 (see table 2) indicates that between 4 and 5 per cent qualified as upper class in the city.<sup>25</sup>

<sup>23</sup> By the end of the period under discussion an awareness or consciousness of class had begun to appear in certain highly unionized trades and occupations in the city of Buenos Aires, as evidenced by strike manifestoes, anarchist propaganda, labor department reports, Socialist party activity, and other signs of labor solidarity. See Hobart A. Spalding, Jr., ed., *La clase trabajadora argentina: Documentos para su historia, 1890-1912* (Buenos Aires, 1970), and Sebastián Marotta, *El movimiento sindical argentino*, 3 vols. (Buenos Aires, 1960-70).

<sup>24</sup> Overlaps, discrepancies, and disagreement inevitably arise when one manipulates gross figures that provide no indication of the most easily quantifiable factor, namely wealth or income. For that reason I have counted only those categories that can be accepted as elite or prestige positions in early twentieth-century Argentina. From the extensive list of occupations given in the 1914 national census (4: 201-12) I have selected the following as upper class: *criadores, cabañeros, estancieros, hacendados, invernadores, banqueros, capitalistas, empresarios, exportadores, gerentes, importadores, clérigos, sacerdotes, abogados, contadores públicos, escribanos, procuradores, dentistas, médicos, químicos, estudiantes secundarios y universitarios, profesores de enseñanza secundaria y universitaria, agrimensores, agrónomos, doctores, escritores, ingenieros, matemáticos, periodistas, péritos*. These categories or easily recognizable substitutes appear in all the preceding censuses. In such a list, however, appears no specific enumeration of heads of government offices or owners of major industrial and commercial establishments, since there is no reliable measure from census data to establish the percentage or number of such positions as distinct from government employees, artisans, or shopkeepers in general. Several assumptions are made in justification of this omission: that the numbers of top bureaucrats and wealthy owners, as developed from other sources such as almanacs and city directories, were small and did not amount, even in 1914, to more than one thousand individuals; that inclusion of individuals in categories on the borderline of elite status such as *contadores públicos, procuradores, and periodistas* balance some of those omitted in administrative and owner functions; and, most important, that given the role of professional titles and landed wealth in Argentina, a large proportion of the elite administrative-owner category will appear under headings of *banqueros, capitalistas, empresarios, abogados, escribanos, and ingenieros* or as *estancieros* and *hacendados*.

<sup>25</sup> Women have been omitted from these calculations since a valid sense of the social structure as related to occupation in Argentina can only be secured at this time from male em-

TABLE 2. PERCENTAGES OF TOTAL EMPLOYED MALE POPULATION OVER  
FOURTEEN YEARS OF AGE IN THE CITY OF BUENOS AIRES,  
DISTRIBUTED BY SOCIAL STRUCTURE<sup>a</sup>

|   | 1887    | 1895    | 1904    | 1909    | 1914    |
|---|---------|---------|---------|---------|---------|
| Elite or Upper Classes <sup>b</sup>                                       | 4.3%    | 4.5%    | 5.1%    | 4.3%    | 4.5%    |
| Blue-Collar-White-Collar <sup>b</sup>                                     | 63.3    | 66.7    | 69.9    | 72.7    | 71.1    |
| Manual Labor <sup>b</sup>   | 32.4    | 28.8    | 25.0    | 23.0    | 24.4    |
|   | 100.0%  | 100.0%  | 100.0%  | 100.0%  | 100.0%  |
| N =   | 156,900 | 234,000 | 312,800 | 462,300 | 597,800 |
| less secondary and<br>university students not<br>listed in census in 1887 | -2,700  |         |         |         |         |
|   | 154,200 |         |         |         |         |
| plus individuals listed as<br>"unclassified" in census                    | +20,300 | +18,100 | +21,800 | +25,700 | +29,100 |
| Total numbers of<br>individuals registered<br>by census                   | 174,500 | 252,100 | 334,600 | 488,000 | 626,900 |

<sup>a</sup> Calculations based on *Censo general de la ciudad de Buenos Aires de 1887*, 2: 43-47, 541-42; *de 1904*, 55-63; *de 1909*, 1: 53-60; *Censo nacional de 1895*, 2: 47-50; *de 1914*, 4: 201-12.

<sup>b</sup> See footnotes 24, 26, and 27 and pages 1056-64 for descriptions of these three categories of the social structure.

TABLE 3. FOREIGN-BORN MALES AS PERCENTAGE OF TOTAL MALE POPULATION  
OVER FOURTEEN YEARS OF AGE EMPLOYED IN MAJOR CLASSIFICATIONS  
OF THE SOCIAL STRUCTURE<sup>a</sup>

|                                       | 1887 | 1895 | 1904 | 1909 | 1914 |
|---------------------------------------|------|------|------|------|------|
| Elite or Upper Classes <sup>b</sup>   | 25%  | 31%  | 26%  | 29%  | 19%  |
| Blue-Collar-White-Collar <sup>b</sup> | 79%  | 70%  | 67%  | 64%  | 65%  |
| Manual Labor <sup>b</sup>             | 79%  | 82%  | 79%  | 68%  | 86%  |

<sup>a</sup> Calculations are based on the same sources as in table 2. The base for each percentage entry is the absolute figure that could be calculated for that entry in table 2.

<sup>b</sup> For descriptions of these three categories of the social structure, see footnotes 24, 26, and 27, and pages 1056-64.

Distinctions among groups below the upper classes are more difficult to make without resorting to the nearly four hundred nonelite occupational

ployment. With the exception of some primary school teachers and a few clerical jobs, all employed women fell into the lower reaches of the occupational scale—as seamstresses, laundry women, and servants. As mentioned earlier, it is well to remember that the number of males in the upper classes does not equal the number of independent households. Because of extended families among the elite the number of males in these classes should be divided by at least four in order to estimate households.

categories listed in the national census of 1914. For the purposes of this study the only distinction made among nonelite occupations is that of the manual dependent laborers whose outstanding characteristic was physical toil. These individuals at the bottom of the occupational ladder included stevedores, porters, unskilled constructions workers, dockmen, cartmen, house servants, street cleaners, and day laborers generally.<sup>26</sup> Once this purely manual labor group, numbering between 20 and 30 per cent of the employed males in the city, is removed, there remains a wide range of semiskilled and skilled laborers, artisans, small shopkeepers, machinists, and white-collar employees in government and commerce. Consequently within these two-thirds of the employed males in the city, broadly categorized as blue-collar-white-collar, existed such variations as bookkeepers and carpenters, primary school teachers and butchers, grocery store owners and streetcar conductors, or government clerks and bakers.<sup>27</sup>

These statistics further suggest that the foreign born posed no threat to Argentine social structure. The native born predominated in the upper classes. Foreigners among the upper classes entered Argentina as managers, directors, or technicians, and consequently claimed that status on the basis of family, education, and wealth. In the period from 1880 to 1910 roughly one-quarter of the upper classes were foreign born. On the other hand, immigrants comprised approximately 80 per cent of the unskilled labor force and two-thirds of the blue-collar-white-collar sector (see table 3). By 1914 the Italian and Spanish immigrants who entered the system at the bottom outnumbered the native born as manual laborers by a ratio of 6:1. Further up the scale the proportion of foreigner to native had gradually declined from 4:1 in 1887 to 2:1 by 1914, largely due to the inclusion of sons of immigrants in the ranks of the native born.

Considerable aspiration and mobility existed within the broad spectrum of nonelite occupations. As long as urban construction and the agricultural export market boomed, the upward climb in three generations from an illiterate, immigrant stevedore to an Argentine-born bank clerk with a few years of *colegio* education seems to have occurred quite frequently.<sup>28</sup> Only in extremely rare cases, however, by dint of parental

<sup>26</sup> A list of occupations with at least one thousand males (asterisk when over ten thousand) over fourteen years of age in the 1914 census registers *carreros, cocheros, \*personal de servicio, marinos, militares, \*jornaleros, \*peones, vendedores ambulantes*.

<sup>27</sup> A list of blue-collar-white-collar occupations with at least one thousand males (asterisk when over ten thousand) over fourteen years of age in the 1914 census registers *agricultores, jardineros, \*albañiles, aparadores, aprendices, carboneros, carniceros, \*carpinteros, confiteros, constructores, curtidores, ebanistas, electricistas, encuadernadores, estibadores, foguistas, fundidores, gasistas, \*herrerros, hojalateros, industriales, lecheros, maquinistas, marmoleros, \*mecánicos, mosaiqueros, muebleros, operarios, panaderos, peluqueros, \*pintores, \*sastres, sombrereros, talabateros, tipógrafos, yeseros, \*zapateros, \*comerciantes, corredores, dependientes, \*empleados de comercio, repartidores, tenedores, conductores, empleados de ferrocarril, empleados de tranvía, guardas de tranvía, motormen, telegrafistas, rentistas, \*empleados de gobierno, jubilados, maestros de escuela primaria, dibujantes, escultores, músicos, \*empleados en general*.

<sup>28</sup> The absence of comparable data from the Argentine censuses and the large interval between the two national censuses for which manuscript returns are available (1869 and 1895) may



*Fig. 9.* Recently arrived European immigrants receiving a free meal at the immigrants' asylum, ca. 1910. Photograph courtesy of the Archivo General de la Nación, AG, SFAA.



*Fig. 10.* A view of the Avenida de Mayo, corner of Bolívar just off the Plaza de Mayo, in 1900. Photograph courtesy of the Archivo General de la Nación, AG, SFAA.



Fig. 11. A workingmen's streetcar in 1912. Photograph courtesy of the Archivo General de la Nación, AG.

sacrifice toward education, by extraordinary financial success, or by an advantageous marriage could such a rapid climb boost an individual across the barrier into the upper classes. As noted above, the upper-class, foreign-born individual almost always possessed those necessary class attributes before entering Argentina.

The common man of Buenos Aires in the late nineteenth century sought to display his material advancement. Appearance, especially in clothing, became exceedingly important in order to show the world that one was "on the way up." A Spanish visitor in 1904 noted that one virtually did not see "workers" on the streets.<sup>29</sup> Clothes literally made the man. As reported in a revealing newspaper account in 1902, the well-dressed man could go anywhere, but appear in a worker's blouse at the halls of congress or at a bank and all doors would shut.<sup>30</sup> The differences in concern with dress between those "just off the boat" and those who had begun "to climb the ladder" are graphically shown by the views of recently arrived immigrants in the dining hall of the immigrants' asylum and of pedestrians a block from the Plaza de Mayo along the Avenida de Mayo (figures 9 and 10). Everyone sought to hide the link with manual labor. In the first decade of the twentieth century special workingmen's cars, in effect open

preclude quantified evidence along the lines suggested for the United States by Stephan Thernstrom in *Poverty and Progress: Social Mobility in a Nineteenth Century City* (Cambridge, Mass., 1964).

<sup>29</sup> Federico Rahola, *Sangre nueva: Impresiones de un viaje a la América del Sud* (Barcelona, 1905), 83.

<sup>30</sup> *La Prensa*, Aug. 12, 1902, p. 4.

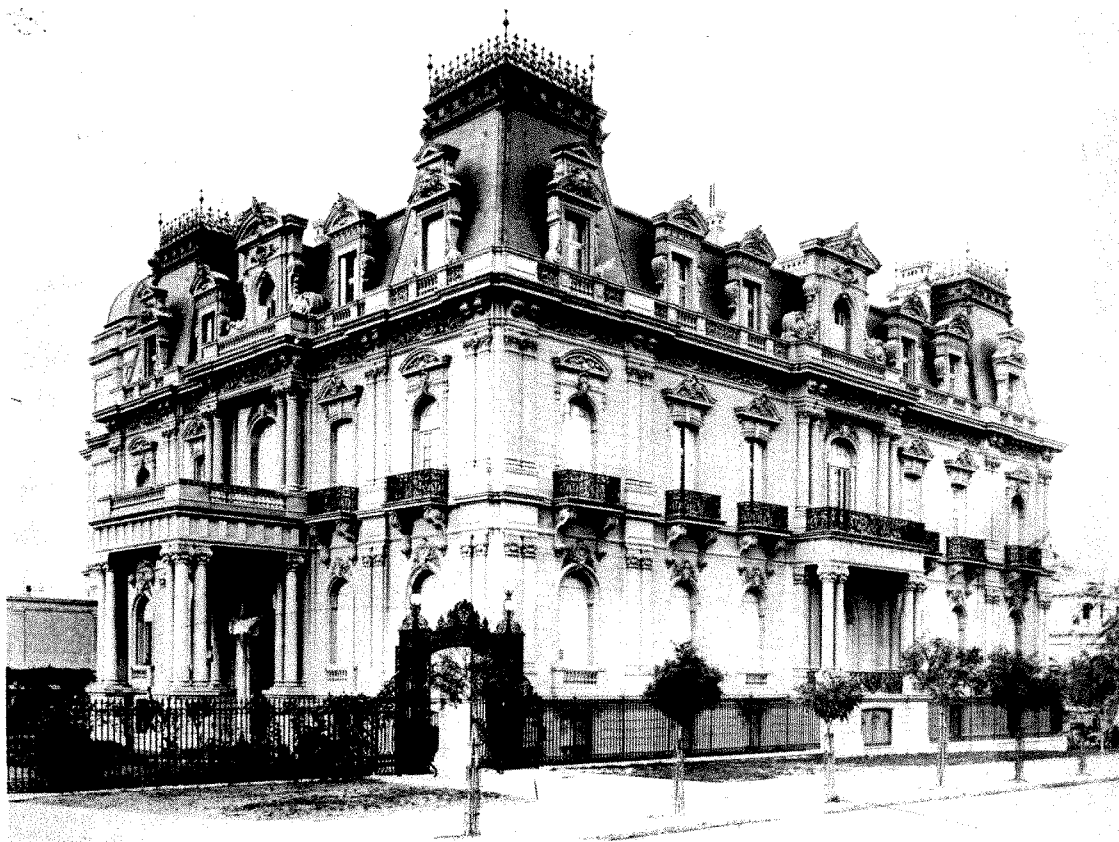
vehicles equipped only with wooden benches or sometimes just flatcars, began to be coupled to regular streetcars during the early morning or late afternoon hours. On these one could ride at half price. Often, however, these cars went nearly empty, or even when they were filled the passengers rarely sported workingmen's garb but rather the traditional coat and tie (note the dress of workers in figure 11). Even Peronist efforts in the 1940s and 1950s to dignify labor with one's hands encountered subtle resistance among construction workers who arrived at and left their jobs in coats and ties, with their work pants and blouses neatly tucked away in briefcases. This spirit found further expression in the egalitarianism evident among the widely different nonelite occupations. In the public mind and in the numerous contemporary and popular *sainetes*, or one-act plays, no one was innately superior to another and anyone might rise to the top. Often the *sainetes* suggested that in this type of "democracy" advancement came not so much by means of drudgery, economy, or the use of one's hands as by luck and wits.

In contrast to the egalitarianism and materialism proclaimed at the workingman's level, among the upper classes these values and the search for economic advancement had to be disguised. The fortune created by the ingenuity of that one-in-a-million immigrant acquired respectability only in the hands of his children or preferably his grandchildren. Commercial wealth presented a better face when transformed into cattle or sheep ranches. Even a corporation lawyer or congressman was supposed to be more interested in Fénelon or Monet than in fees or constituents.<sup>31</sup>

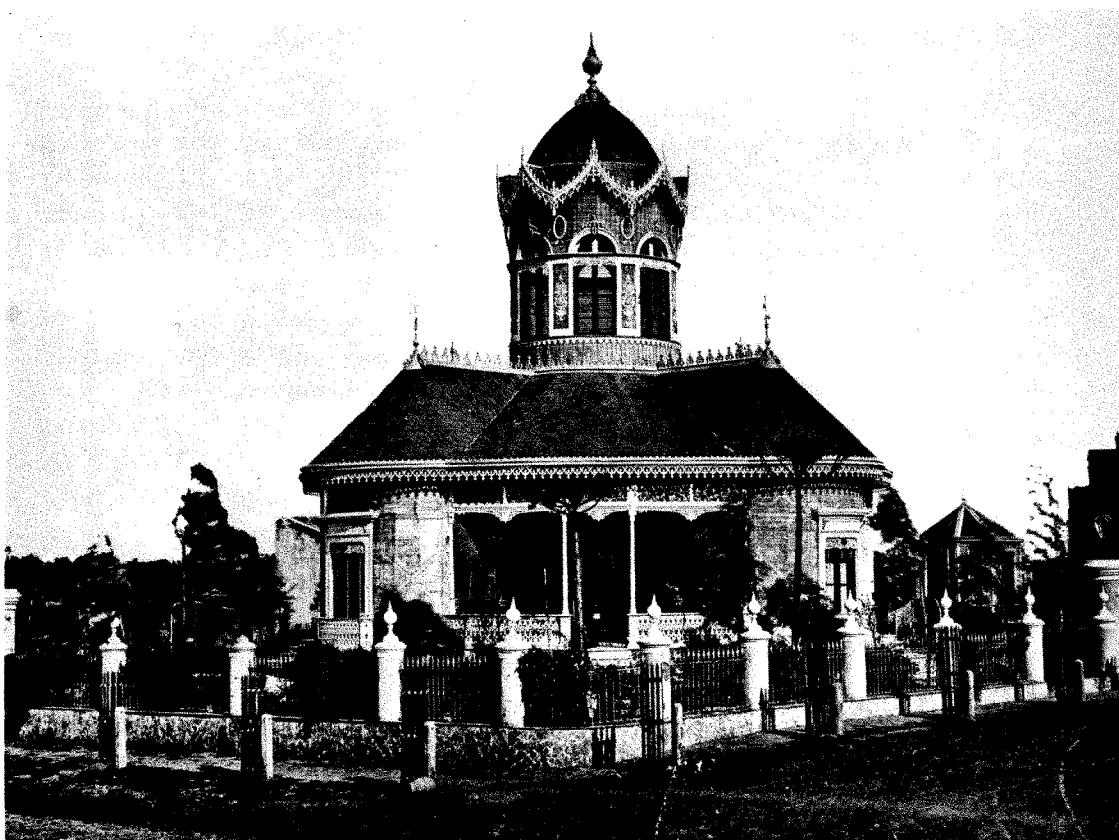
Yet while the means of acquiring wealth had to be camouflaged, its possession was increasingly and ostentatiously paraded. The simple style of the Argentine elite until the 1870s was swallowed up by the *petit hôtel* and the palace. Those persons whose fortunes had been inflated by the rising value of land built one mansion on their cattle ranches, another on an elegant residential street in Barrio Norte, and a summer house along the estuary north of the city (figures 12 and 13 show typical examples of a palace and a summer home built in the late nineteenth century). The modest and closely guarded family life of earlier years gave way to a gaudy social whirl, highlighted by sumptuous feasts and splendid parties. The golden wedding anniversary of the Guerricos in 1906 titillated even a blasé *porteño* society: hundreds of workmen transformed the family garden into one vast salon fully equipped with gas heating, electric lights, and running water; twelve hundred guests, including the president, sat down to a feast that started off with tiny woodcocks stuffed with *pâté de foie* and chestnuts and reached its climax with the invariable

<sup>31</sup> Suggestive of these attitudes are the recollections found in Ramón Columba, *El congreso que yo he visto*, 3 vols. (2d ed.; Buenos Aires, 1953-55), 1: 53; Antonio J. Pérez Amuchastegui, *Mentalidades argentinas, 1860-1930* (Buenos Aires, 1965); and Julia V. Bunge, *Vida: Época maravillosa, 1903-1911* (Buenos Aires, 1965).





*Fig. 12.* The palace of the Peña family built in 1903 in Barrio Norte at Arenales and Maipú.  
 Photograph courtesy of the Archivo General de la Nación, AG, SFAA.



Moët Chandon at dessert.<sup>32</sup> In dress, art collections, furniture, table service, and carriages the Argentine wealthy appeared determined to copy the nobility of Europe. Despite Catholic precepts few of this class felt much relation to the hordes of dirty, uneducated immigrants who swarmed in the downtown slums. Reform ideologies, freshly introduced from Europe, flowed in genteel discussions over brandy and coffee at the Jockey Club or were aired on the floor of congress. But most of the upper classes viewed laborers as little more than work animals. Characteristically the Sociedad Rural Argentina, the association of the country's major livestock growers, established a prize in 1896 to be awarded for the design of a ship's hold that could transport cattle on the hoof to Europe and return loaded with immigrants.<sup>33</sup>

By the 1930s the growth of industry and the increasing political activity of such groups as labor unions helped to promote a social structure in Buenos Aires more similar to the three-class model. The city, nevertheless, had been characterized by the sharp social distinction of the upper classes from the broad mass of common people long after economic prosperity suggested a more thoroughly bourgeois society. Attitudes toward class, manual labor, materialism, and egalitarianism sprang from Latin or Hispanic backgrounds, but they also received reinforcement from the commercial-bureaucratic heritage.

SINCE THE COMMERCIAL-BUREAUCRATIC CITY relies for its stimulus and growth largely on an externally dependent agricultural economy, it follows that continuation of the commercial-bureaucratic orientation is linked to the prosperous expansion of agricultural exports and the importation of manufactured goods, capital, and immigrants. A final characteristic of this type of city, therefore, is that the very prosperity of the exchange arrangements postpones industrialization.

Despite the bustle and prosperity of the Buenos Aires environment in 1910, nothing stimulated industrialization, encouraged import substitution, or prodded landowners or merchants to invest capital in factories. The political structure of the city as well as that of the nation remained firmly in the hands of upper classes whose traditions and wealth came from landowning, commerce, or politics. Immigrant labor dedicated itself to retail commerce, crafts, construction work, harvest labor, or menial services. Foreign and domestic investment entered real estate and commerce, was lent to national, provincial, or municipal governments, or financed railroads, utilities, and port works. The exchange of wool, cereals, meats, and hides for manufactured items appeared to be good business, at least until the 1930s. The barriers against foreign imports, essential to industrial development, could not be built as long

<sup>32</sup> *La Prensa*, Aug. 14, 1906, p. 8.

<sup>33</sup> *La Agricultura*, Aug. 20, 1896, p. 620.

as Argentina and Buenos Aires remained so dependent on foreign markets for the sale of agricultural products and the purchase of consumer goods.

Certain industries flourished, but only those endeavors related to Argentina's export business received major inputs of capital and technology. Flour mills and meat-packing plants, along with a few consumer-goods industries such as breweries and manufacturers of shoes and biscuits, represented the only establishments employing skilled operatives, complex machinery, and electrical energy. Furthermore many of these plants were owned by foreign corporations or were largely dependent on foreign credit. The bulk of so-called industry remained scattered around the city, in small home workshops employing fewer than ten laborers and owned by immigrants who brought with them a small hoard of skills and tools. Although reserves of technical and entrepreneurial abilities were thus accumulated and safeguarded, no forces promoted modernized and diversified industry. Tax structures, tariff rates, and credit operations all conspired against an independent and influential industrial class.

In addition to these economic forces the very success of the commercial-bureaucratic orientation fostered values that, although not anti-industrial, did little to stimulate industrialization. Manual labor continued to be held in disrepute. Aside from the upper classes there existed little cohesiveness or awareness of group interests among the population. Family and personal relationships predominated. The immigrant continued to be looked upon as a manual laborer and little else, even by the second-generation children of immigrant parents.

Such values remained unshaken by the massive European immigration to Argentina. Immigrants contributed labor but politically remained outside the system. The dream of Italian and Spanish laborers was *hacer la América*, or the achievement of rapid economic success in the New World, not political expression. In addition local police and election officials actively discouraged naturalization by the foreign born. Skillful obstruction by means of red tape and procrastination voided the liberal naturalization laws and the flowery rhetoric voiced in the halls of congress. Finally there was the undeniable fact that the foreigner gained virtually no privileges by naturalization. As a resident alien he could work or conduct business on an equal footing with the native-born Argentine. He could move around the country, marry, and own property as freely as any Argentine citizen. In the city of Buenos Aires he could even vote in municipal elections. And as a foreigner he was not subject to the military service required of all adult males after the turn of the century. Hence, despite exhortations by the Socialist party and other groups, naturalized citizens in Buenos Aires in 1904, 1909, and 1914 represented only 2, 2.5, and 4 per cent respectively of the city's foreign-born male population—in striking contrast to figures that frequently reached 50 per cent in United States cities.

In like manner the school system, while spreading the benefits of one

or two years of primary education to most children in Buenos Aires, continued to be highly elitist in structure. At the beginning of the twentieth century only 5 per cent of those pupils who began first grade completed the six elementary years. One explanation for this can be found in the predominant attitude expressed by a national congressman in 1907: "The child is in school—and in a very healthy one—when he is in the workshop and has a tool in his hand."<sup>34</sup> Among the common people only parents who had financial resources and who valued education enormously could push a child through the narrow funnel of primary education into the still narrower funnel of the *colegio* and finally into the professional preparation of the university. The cost itself of such education might not be prohibitive, but few could forego their children's services until the completion of *colegio* at the age of eighteen or nineteen or the university at twenty-three or twenty-four.

The educational system also reinforced elite traditions. For the mass of school children who completed only the first or second grade, a principal thrust of the curriculum lay in the indoctrination of Argentine patriotism and values. As the nineteenth century drew to a close and legislators and educators became more concerned about the flood of European immigrants entering the country, the symbols of flag, nation, independence, and Argentine greatness acquired a predominant position. As one Spanish observer noted, the whole purpose of the Argentine school seemed to be the preparation of the child to celebrate the national independence days of May 25 and July 9.<sup>35</sup> Beyond these initial years of primary school, which also gave pupils a rudimentary degree of literacy, the training became humanistic and classical. Preparation then was geared to entrance into the *colegio*, where literature, philosophy, and languages received particular attention. Technical education tended to be for orphans or other wards of the state, and applied sciences and technology continued to receive low priority in Argentina until well after 1910. Those who emerged from *colegio* training in Buenos Aires prior to 1910 were perhaps the best read and most literate students in the Western Hemisphere, but they had received relatively little orientation or stimulus toward questions involving innovation or the everyday world around them.

The upper classes also received significant assistance from the sons and daughters of immigrant parents in maintaining existing values. Most immigrants arrived in Argentina between the ages of twenty and forty. Their spouses usually came from the same country of origin or were Argentines born of immigrant parents. Most immigrants were of peasant backgrounds or came from the lower levels of the urban working classes in Italy and Spain. Argentine values thus echoed certain aspects of their

<sup>34</sup> Antonio Piñero, June 26, 1907, Cámara de Diputados, *Sesiones, 1907*, 1: 138.

<sup>35</sup> Adolfo Posada, *La República Argentina* (Madrid, 1912), 224.

own cultural baggage: the dominance of the male, admiration of virility, and acceptance of the gulf between the elite and the common people. In Buenos Aires the vast majority of immigrant men engaged in grinding manual labor twelve hours a day, six days a week, while women took on monotonous piecework, laundering, ironing, or domestic service in addition to raising a family. The children of these immigrants received great incentives and pressures from the host culture to conform to Argentine ways, even more so if the mother were Argentine born. Epithets of *gringo* or *gallego* might bounce harmlessly off the immigrants' ears, but they sank their shafts deeply into many of the children.<sup>36</sup> If the father mutilated Spanish with his Gallic or Genoese phrases, the son roared with laughter at the *sainetes* that caricatured the clumsy immigrant. The son also admirably slurred the local slang. If the father proudly wore a beret, enjoyed an afternoon at the *bocce* court with his cronies, or hungered for *paella* to celebrate a special occasion, it was certain that his son sought in dress, amusements, and food to avoid anything that might identify him as a foreigner and, if possible, as a laborer. Even the grinding toil of the father seemed to find rejection in the continued denigration of manual labor and the apparent reliance on other methods of getting ahead—the long shot, the assist from friends, or the stroke of luck. At the beginning of the twentieth century newspapers and even government officials began to express concern at one symptom—the tremendous surge of gambling on lotteries, horse races, and other sports. In the decade of the 1890s, at a moment when the Argentine economy was anything but flourishing, the *porteño* public had invested nearly half a billion pesos in gambling, or approximately one-fifth of the country's total exports for the decade.<sup>37</sup>

These values supplemented the powerful economic incentives that encouraged and rewarded *porteños* in their administrative and trade functions. Because of the very success of these values in the period from 1880 to 1910, the commercial-bureaucratic orientation helped the export-import economy further delay the need for industrialization.

THE UNPARALLELED AND PREVIOUSLY UNEXPLOITED productivity of Argentina's pampas region in the late nineteenth century redounded to *porteño* benefit and not to the growth of the rural or hinterland regions. The success of the accompanying commercial-bureaucratic orientation at Buenos Aires reinforced certain spatial and mental characteristics and helped en-

<sup>36</sup> In the Argentine context the *gringo*, or Italian, represented society's drudge, determined to make a fortune in America and consequently unable to enjoy any of life's pleasures. The *gallego*, or generically the Spaniard, was usually characterized as an inoffensive and somewhat stupid workhorse. Also see Gladys Onega, *La inmigración en la literatura argentina* (Buenos Aires, 1969).

<sup>37</sup> *La Prensa*, Aug. 19, 1901, p. 5.

sure that Argentina remain locked into an agricultural economy and external dependence at least until the 1930s. The reliance on a tiny downtown core that largely continued as the elite's place of residence; the location of tenement housing in the central part of the city; the outward spread of the common man toward the city's fringe; the extreme importance to urban life of the local neighborhood and its institutions; the marked separation of the upper classes from the broad spectrum of blue-collar-white-collar and manual laborers; the delay of industrialization brought on by the advantages of exchanging agricultural goods for manufactured commodities—all these effectively contributed to the prosperity, pleasantness, and stability of the *porteño* environment from 1880 to 1910. Not until the world depression of the early 1930s emphasized the impact of declining yields from the undercapitalized rural sector, accelerated the drying-up of outside sources of immigrants and capital, and caused a decline in European demand for pampas products did the shortcomings of such an orientation become apparent.

Despite attempts since 1930 at state planning, subsidization of industry, and regional diversification, the commercial-bureaucratic heritage has proved hard to dispel. The city of Buenos Aires still shows its effects. Settlement patterns in the federal district have not basically changed since 1910. The central business district provides commercial, financial, administrative, cultural, and amusement services for the upper and upper-middle classes in the same small area. *Conventillos* survive downtown, while shanty towns, or *villas miserias*, ring the outskirts. Increasingly the wealthy have moved out to the northern river bank beyond the federal district, although many cluster in Barrio Norte. Industry has concentrated on the south side and beyond the federal district to the west and the south, but small plants and workshops remain scattered throughout the city, even close to the Plaza de Mayo. The barrio has lost considerable psychological and economic significance in an age when work, school, and play push inhabitants outward, but for several million *porteños* it still represents the basic outlook of their daily life. The city, with its overwhelming commercial, administrative, and now industrial preponderance, has drawn talents, resources, and products to itself. This success has exacted a price in strained railroads and congested port facilities. The head continues to rest on an extended but underdeveloped body, while new economic zones or other urban areas appear slowly or assume secondary roles. Even industry, accepted since 1930 as one of Argentina's needs, has received limited inputs of capital and technology.

Despite the growth of Buenos Aires as a metropolis, the city's social structure and values have changed slowly. The class system has become more complex, and a middle sector has emerged. But these middle groups lack unity and continue to imitate the upper classes. From another perspective political authorities fear the laboring class and seek to repress it. Attitudes toward manual labor, social distinctions, or materialism

recall those of sixty years ago. The social and educational patterns reflect much of the values of an earlier age.

To a marked degree the experience of Buenos Aires, springing out of Argentina's agricultural economy and external dependence, has been replicated in other Argentine cities. In the late nineteenth century population had begun to move from the countryside toward the towns and cities. The urban-rural ratio (based on 2,000 inhabitants as the smallest urban unit) changed from 25:75 per cent in 1869 to 53:47 per cent by 1914. Ports such as Rosario or Bahía Blanca (223,000 and 44,000 population, respectively); provincial capitals turned railroad centers such as Córdoba, Tucumán, or Mendoza (135,000, 91,000, and 59,000); administrative capitals and provincial markets such as Salta or Catamarca (28,000 and 13,000); or small rail and local trade nuclei such as Río Cuarto or Junín (30,000 and 21,000)—all these by 1914 shared strikingly similar demographic, physical, and ecological patterns. Each received migrants and raw materials from its immediate area and transmitted them along with capital toward the coast, primarily toward Buenos Aires. Each had its small elite of landowners, merchants, bureaucrats, and professionals. In each the residences and activities of the elite centered on the plaza, while nonelite groups spread outward in search of cheaper lands. The commercial-bureaucratic outlook dominated the secondary cities in Argentina and made them way stations for channeling goods, services, and capital toward Buenos Aires.

The timing and indeed the initial prosperity of the rapid growth of Buenos Aires from 1880 to 1910 contributed to the nation's contemporary problems. Buenos Aires entered the twentieth century as a cosmopolitan metropolis drawing from the provinces and from its position between the pampas and Europe. The city's monopoly of human resources, its social structure, and its lack of an industrial base owed much to the way the Argentine economy had developed around exploitation of the pampas. Economic success available to the upper classes and in more limited fashion to the common man brought an air of complacency that hung over Buenos Aires for several decades. The promise of continued expansion that *porteño* growth had forecast began to dissipate, however, by 1930. A static per capita gross national product, a sterile political conflict, and subsequently a people's loss of faith in their future revealed the handicaps of agriculture/external dependency for the nation and of a commercial-bureaucratic orientation for the cities. The nation as a whole had not received an adequate share of the gains from the spectacular growth of its cities, especially from that of its capital and principal port. That city's orientation during its late nineteenth-century period of rapid expansion had further ill prepared Argentina for changing patterns of world trade, for industrialization, or for popular rule.<sup>38</sup>

<sup>38</sup> Philip M. Hauser suggests in slightly different terms that in underdeveloped nations such as a single major city frequently grows excessively because of its position as entrepôt between the

THE EFFECTS OF AN agricultural economy and external dependence on urban growth undoubtedly have taken many different forms. This article describes one result, characterized as commercial-bureaucratic and experienced at Buenos Aires in the late nineteenth century. I have not embarked on the extensive research needed to compare or contrast this particular experience with those of other cities that emerged under similar economic conditions. The experience of Buenos Aires, however, doubtless has parallels elsewhere.<sup>39</sup> In order to suggest possibilities for further study, I conclude with a resumé of generalized characteristics that develop from the agricultural and external dependency features of a region's economy and from the self-reinforcing aspects of the commercial-bureaucratic orientation.

First, the elite tends to reside at the city's core. Commercial activities and political administration of the city and hinterland, as well as the financial, social, and intellectual services for the elite, remain extraordinarily concentrated at this core. Second, newcomers to the city cluster for several years in high-density slums at the city's center, often interspersed with or adjacent to the residences of the well-to-do. Third, in the absence of large industrial establishments, the tendency of nonelite groups is to push outward, seeking cheap lands, individual homes, and often ownership—that is, upward mobility—in outlying areas. As a result the city, barring geographical obstacles, sprawls rather than growing upward or developing particularly concentrated settlements. Fourth, in the outward thrust numerous self-contained neighborhood units develop, which provide economic and social services for the nonelite inhabitants. Many individuals find employment in small workshops and stores within the neighborhood where they reside. All inhabitants form social and psychological ties to neighborhood institutions and identify first with their individual neighborhood rather than with larger political or religious units or with the city itself. Fifth, in social structure the upper classes remain distinct from the common people. Considerable mobility exists among the multitude of manual, blue-collar, and white-collar occupations. The upper classes occasionally recruit from below on the basis of wealth, education, or family connections, but they permit no challenge to the existing values. And sixth, as long as world markets absorb the area's agricultural products and manufactured imports remain available, no

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"colony" and the imperial country. Disruption of the imperial system often leads to some loss of the city's basic economic function. "To the extent that this has occurred, such cities must await further national economic growth adequately to support their present size." "Urbanization: An Overview," in Philip M. Hauser and Leo F. Schnore, eds., *The Study of Urbanization* (New York, 1965), 36.

<sup>39</sup> One such parallel appears in the "commercial city" described by John W. McCarty, "Australian Capital Cities in the Nineteenth Century," *Australian Economic History Review*, 10 (1970): 107-37. In addition to Australia, the study of certain urban centers in the Southern United States and in South Africa might yield interesting parallels or contrasts to the *porteño* experience.



pressures develop to industrialize or to alter the profitable dedication of human resources within the city to trade and administration.

Further research into other urban experiences may reveal if these characteristics of the Argentine case appear in other cities that develop within an agricultural hinterland, serve as the major commercial and administrative center for their region, and depend overwhelmingly on immigration, foreign capital, and foreign markets for expansion. Such investigation and comparison may also clarify whether the commercial-bureaucratic orientation holds back a city and its hinterland from adjusting to a changing world environment. In the Argentine case, at least, this orientation seems to have served as a brake on national development.

#### APPENDIX:

##### A Note on the Nature of the Commercial-Bureaucratic City

AT THIS STAGE of research it would be imprudent to assign precise values to the degree of commercial and administrative dedication required to qualify a city as commercial-bureaucratic. Two indicators available in many nineteenth- and twentieth-century urban sources—the amounts of labor and capital involved in various sectors of the economy—suggest that such indicators could be developed. As a preliminary step several measurements that apply in Buenos Aires are advanced for consideration as variables to be refined and extended by research in cities elsewhere.

From the published Argentine census reports it is difficult and at times impossible to extract any reliable estimate of numbers employed in the various sectors of the economy. I have not attempted to isolate industrial workers, since artisans, independent skilled laborers, and industrial workers are lumped together within categories and these categories change from one census to the next. For commercial and administrative employees there is somewhat more continuity, although omissions and changed categories suggest caution in the use of these statistics. A rough measure of occupations in commerce and bureaucracy gives the following results for the city of Buenos Aires.

TABLE 4. PERCENTAGE OF MALES OVER FOURTEEN YEARS OF AGE EMPLOYED IN GOVERNMENT AND COMMERCE<sup>a</sup>

|            | 1887    | 1895    | 1904    | 1909    | 1914               |
|------------|---------|---------|---------|---------|--------------------|
| Government | 7.6%    | 5.7%    | 12.3%   | 8.6%    | 8.8%               |
| Commerce   | 20.5%   | 23.1%   | 27.1%   | 23.7%   | 14.2% <sup>b</sup> |
| N =        | 156,900 | 234,000 | 312,800 | 462,300 | 597,800            |

<sup>a</sup> See table 2 for explanation of sources and of N.

<sup>b</sup> The 1914 figure for commerce seems particularly suspect since 9,000 individuals shifted from commercial to skilled labor categories in the 1914 reporting and several major categories used in 1909 disappeared completely. Furthermore, despite the recession of 1913-14, the numbers of both *comerciantes* and *empleados de comercio* increased from 1909 to 1914, thus suggesting some increase in commercial establishments.

Capital invested in various areas of the economy affords another rough measure of relative importance of sectors. In Buenos Aires the amount of capital invested in industry, including processing of agricultural commodities for export, amounted to 45 per cent in 1909 and 55 per cent in 1914 of that invested in commerce.

TABLE 5. TOTAL CAPITAL INVESTED IN INDUSTRY AND COMMERCE<sup>a</sup>  
(in millions of pesos, *moneda nacional*, m\$<sub>n</sub>)

|   | 1895             | 1904              | 1909  | 1914  |
|---|------------------|-------------------|-------|-------|
| Industry                                | 95 <sup>b</sup>  | .... <sup>c</sup> | 324   | 548   |
| Commerce                                | 167 <sup>b</sup> | 521               | 724   | 996   |
| Industry as a Percentage<br>of Commerce | 56.7%            | .... <sup>c</sup> | 44.8% | 55.0% |

<sup>a</sup> Calculations based on *Censo nacional de 1895*, 3: 273, 365; *Censo general de la ciudad de Buenos Aires de 1904*, 138-49, 164-75; *de 1909*, 1: 117-22, 143-47; and *Censo nacional de 1914*, 7: 115-20; 8: 141-45.

<sup>b</sup> Because of fluctuations in the gold equivalent of the m\$<sub>n</sub> prior to 1903, the absolute values given in the 1895 census of 144 million m\$<sub>n</sub> for industry and 254 million m\$<sub>n</sub> for commerce have been multiplied by a correction factor—m\$<sub>n</sub> required to purchase a gold ounce after 1902 divided by m\$<sub>n</sub> required in 1895 (227/344 or .66)—in order to make them comparable as absolute figures with 1904, 1909, and 1914.

<sup>c</sup> The 1904 census gives 99 million m\$<sub>n</sub> as the total capital invested in industry. This total appears grossly undervalued in light of the 1909 figures. One might achieve a more accurate figure by assuming, since there is no evidence to the contrary, that the ratio of capital invested in industry (C), to the value of goods produced by industry (V), remained constant in periods (1) 1904, (2) 1909, and (3) 1914. Since the 1904 census gives a reliable value for goods produced, one can apply the average of the 1909 and 1914 ratios to calculate C<sub>1</sub>; thus,

$$C_1 = V_{1/2} (C_2/V_2 + C_3/V_3)$$

This calculation gives a value of 205 million m\$<sub>n</sub> for capital invested in industry, or 39.4 per cent of that invested in commerce in 1904. This also correlates better with the corrected 1895 figure of 95 million m\$<sub>n</sub> invested in industry.

In conjunction with the relative distribution of capital between industry and commerce, it is also desirable to examine the distribution of

capital within the secondary sector. At Buenos Aires in 1914, activities important to an industrializing economy, such as metallurgy, textiles, and chemicals, received respectively 10.7, 5.8, and 2.5 per cent of total capital invested in industry. At the same time 31.7 per cent of the total was invested in activities directly related to the agricultural export-oriented economy, such as food processing, burlap bags for grain, tanning, and grain elevators.

TABLE 6. DISTRIBUTION OF CAPITAL INVESTMENT IN  
INDUSTRY BY PERCENTAGES<sup>a</sup>

|                 | 1895             | 1904            | 1914             |
|-----------------|------------------|-----------------|------------------|
| Food Processing | 18.9%            | 16.3%           | 23.6%            |
| Clothing        | 18.6             | 16.3            | 14.3             |
| Construction    | 12.3             | 7.9             | 14.3             |
| Furniture       | 10.5             | 7.6             | 6.7              |
| Ornamentation   | 3.6              | 3.4             | 1.3              |
| Metals          | 8.9              | 12.9            | 10.7             |
| Chemical        | 4.3              | 3.2             | 2.5              |
| Printing        | 4.3              | 6.8             | 4.7              |
| Textiles        | 3.3              | 6.8             | 5.8              |
| Burlap Bags     | .5               | 2.6             | 4.3              |
| Cigarettes      | 7.4              | 5.4             | 6.7              |
| Tanning         | 4.6              | 5.1             | 1.8              |
| Grain Elevators | 1.9              | 3.9             | 2.0              |
| Miscellaneous   | .9               | 1.8             | 1.3              |
|                 | 100.0%           | 100.0%          | 100.0%           |
| N =             | 144 <sup>b</sup> | 99 <sup>c</sup> | 447 <sup>d</sup> |

<sup>a</sup> Calculations based on *Censo nacional de 1895*, 3: 273, 365; *Censo general de la ciudad de Buenos Aires de 1904*, 164-75; and *Censo nacional de 1914*, 7: 115-20.

<sup>b</sup> Expressed in millions *moneda nacional*, m\$n. The correction factor mentioned in table 5 does not affect the percentage calculations, so the value of 144 million m\$n given for industry in the 1895 census is used.

<sup>c</sup> Expressed in millions m\$n. Despite the undervaluation of capital invested in industry noted in table 5, the value given in the 1904 census is used in these calculations on the assumption that the basis for determining the value of capital distributed to various activities within the industrial sector remained constant. Percentages, therefore, can validly be calculated on the basis of the 99 million m\$n figure.

<sup>d</sup> Expressed in millions m\$n. Electricity and gas, valued at 101 million m\$n, was included in the census total for capital invested in industry in 1914; it has been removed in this table in order to make categories and percentages comparable with 1904 and 1895.

In an exploratory fashion, therefore, I suggest that these measures applicable to Buenos Aires in the period from 1880 to 1910 might be used as a starting point and, with refinement in other case studies, be expanded in order to secure a more precise definition of the commercial-bureaucratic city.

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# New Perspectives on the Republican Party, 1877-1913

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A Review Article by LEWIS L. GOULD

JOHN BRAEMAN. *Albert J. Beveridge: American Nationalist*. Chicago: University of Chicago Press. 1971. Pp. x, 370. \$12.50.

RICHARD JENSEN. *The Winning of the Midwest: Social and Political Conflict, 1888-1896*. Chicago: University of Chicago Press. 1971. Pp. xvii, 357. \$12.50.

HORACE SAMUEL MERRILL and MARION GALBRAITH MERRILL. *The Republican Command, 1897-1913*. [Lexington:] University Press of Kentucky. 1971. Pp. xi, 360. \$12.50.

RICHARD E. WELCH, JR. *George Frisbie Hoar and the Half-Breed Republicans*. Cambridge, Mass.: Harvard University Press. 1971. Pp. 364. \$14.00.

WITH THE EXCEPTION of Theodore Roosevelt's presidency the four decades between the election of Rutherford B. Hayes and the disaster of 1912 have not impressed historians as a glorious period in the history of the Republican party. They recognize that the GOP rose to majority status between 1876 and 1896 and that the party dominated American politics in the thirty-five years after the election of 1896, but the scholarly appraisal of the Republican record has been generally negative. The party's reputation as an agent of capitalism, its conservative position in contemporary affairs, and the general preference among historians for reformers over professional politicians account to some degree for the critical stance usually adopted toward the GOP. In classroom and textbook, men like James G. Blaine, Benjamin Harrison, and William McKinley lingered until recently as figures of fun, objects of moral scorn, or lessons in the dangers of weak presidents. Renewed interest in the Grand Old Party after 1877 has disturbed many stereotypes and refurbished some damaged reputations.<sup>1</sup> Two of the books under review explore the improvement

<sup>1</sup> See, for example, Carl N. Degler, "American Political Parties and the Rise of the City: An Interpretation," *Journal of American History*, 51 (1964): 41-59; Stanley L. Jones, *The Presidential Election of 1896* (Madison, 1964); John G. Sproat, "The Best Men": *Liberal Reformers in the Gilded Age* (New York, 1968); H. Wayne Morgan, *From Hayes to McKinley, National Party Politics, 1877-1896* (Syracuse, 1969); H. Wayne Morgan, ed., *The Gilded Age* (Syracuse,

in party fortunes in the Gilded Age and do much to restore William McKinley to his deserved place as the architect of Republican success. The other studies examine the party in the years of power that followed 1896 and illuminate the problems that confront students of this fascinating era of American political history.

Until Roosevelt's second term Republicans told each other that, unlike the Democrats', theirs was the party of action and change, "the party that does things, instead of one that opposes them."<sup>2</sup> Richard E. Welch's thoughtful biography of George Frisbie Hoar of Massachusetts uses the career of a single senator to examine the real basis for this staple of campaign oratory and to reveal the complexity of the Republican party's internal history in the last quarter of the nineteenth century. In his effort to safeguard Negro voting rights, his concern with the trust problem, and his advocacy of the protective tariff, Hoar exemplified the constructive energies that his party brought to national issues. Its record of achievement was a mixed one, but Welch's analysis avoids the simplistic charges of insincerity and hypocrisy so often leveled at the work of politicians in this period. He rejects the notion of an unbroken descent "from unblemished idealism to corporate reaction" in party history between 1865 and 1900 and correctly notes that "the transformation of the Republican party in the generation after Appomattox was a more subtle process than is usually recognized."<sup>3</sup>

Like other leaders whom Welch groups under the label of Half-Breed, Hoar believed that the Republicans should promote the growth of the economy with a protective tariff and a general encouragement of business enterprise. This attitude represented more than a commitment to corporate wealth. It reflected a vision of a harmonious, interdependent society in which assistance to business raised the level of all groups and did not preclude attention to the human problems of an industrial nation.<sup>4</sup> This was not an easy political creed to sustain, and it gave way after 1900 under the pressures of an increasingly pluralistic economy and political system.

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1970), 171-87. Robert D. Marcus, *Grand Old Party: Political Structure in the Gilded Age, 1880-1896* (New York, 1971), finds little to admire in the Republicans and in American politics generally. The more favorable evaluation of the party before 1877 has aided the reappraisal of the GOP in the Gilded Age. Eric Foner, *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party Before the Civil War* (New York, 1970).

<sup>2</sup> John Milton Hay, "Fifty Years of the Republican Party," in *Addresses of John Hay* (New York, 1906), 300. For other expressions of this theme, see John Coit Spooner to L. D. Coombs, Mar. 29, 1902, John Coit Spooner Papers, Manuscripts Division, Library of Congress; "Address of Joseph G. Cannon," in *Official Proceedings of the Thirteenth Republican National Convention Held in the City of Chicago, June 21, 22, 23, 1904* (Minneapolis, 1904), 183.

<sup>3</sup> Welch, *George Frisbie Hoar*, 336.

<sup>4</sup> A. Maurice Low, *Protection in the United States* (New York, 1904), 42-43; Herbert Croly, *Progressive Democracy* (New York, 1915), 87-102; Gerald W. Wolff, "Mark Hanna's Goal: American Harmony," *Ohio History*, 79 (1970): 138-51. Any investigation of Republican thought, especially with regard to the tariff, should concentrate on the pivotal role of James G. Blaine in the 1880s. Professor R. Hal Williams has begun a political biography of Blaine that will address itself to this problem.

But in the Gilded Age Republicans of Hoar's persuasion could still believe in, and act upon, the principle "that while the demands of social continuity forbade radical experimentation they occasionally required legislative revision."<sup>5</sup>

The treatment of Hoar's participation in the lawmaking process is judicious and balanced. Welch sees that the tariff embraced more than a narrow parochialism and that Hoar and many of his colleagues endorsed protection from ideological conviction as well as self-interest. The discussion of the Federal Elections Bill of 1890 summarizes well the unhappy consequences of Republican failure to enact that reform measure. Welch is sometimes unnecessarily defensive on the actions of the GOP and fails to see how James G. Blaine functioned as a party leader, but the larger strengths of his book make it an excellent political biography.

This study also joins a growing list of books that are reappraising more favorably the policies and character of William McKinley. Hoar's opposition to the expansionism of the McKinley administration forms an important chapter in the narrative, and Welch is perceptive about his subject's attempts to change the nation's foreign policy. While describing the strains upon the relationship between senator and president, Welch realizes that McKinley was an uncommonly effective executive. There is the customary rhetorical obeisance to the erroneous notion that McKinley was weak, followed by remarks on the president's adroit management of the Senate and his deft maneuvers to secure ratification of the Treaty of Paris in 1899. Welch contributes further evidence of McKinley's unusually productive relations with Congress, an area where few presidents have done well. In dealing with Capitol Hill or in shaping public opinion McKinley employed techniques that historians applaud in his successors. Students of the growth of presidential power need to look again at his tenure in office in light of the work of Welch and other scholars. If results rather than mere activity are a valid measure, then McKinley was as strong a president as his immediate successors.<sup>6</sup>

An appreciation of McKinley's political genius is a major theme of Richard Jensen's superb study of Midwestern electoral politics in the 1890s. Well written and closely reasoned, *The Winning of the Midwest* brilliantly combines the techniques of the social scientist and the historian

<sup>5</sup> Welch, *George Frisbie Hoar*, 334.

<sup>6</sup> For the emerging revisionism on McKinley, see John A. S. Grenville and George Berkeley Young, *Politics, Strategy, and American Diplomacy: Studies in Foreign Policy, 1873-1917* (New Haven, 1966), 239-66; Paul S. Holbo, "Presidential Leadership in Foreign Affairs: William McKinley and the Turpie-Foraker Resolution," *AHR*, 72 (1966-67): 1321-35; Paul S. Holbo, "The Convergence of Moods and the Cuban Bond 'Conspiracy' of 1898," *Journal of American History*, 55 (1968): 54-72; Morgan, *From Hayes to McKinley*, 482-93; Edward Ranson, "The Investigation of the War Department, 1898-1899," *The Historian*, 34 (1971): 78-99. Some historians who dislike the results of McKinley's presidency concede his ability. See Walter LaFeber, *The New Empire* (Ithaca, 1963), 327-33. Graham A. Cosmas, *An Army for Empire: The United States Army in the Spanish-American War* (Columbia, Mo., 1971), 102-04, criticizes McKinley for doing too much, not too little, as a war leader.

to produce a convincing narrative of developments in the principal regional battleground of partisan conflict at the end of the nineteenth century. Jensen traces the breakdown of the even balance between the major parties after 1888, the brief Democratic upsurge in the early 1890s, and the decisive Republican triumphs of 1894 and 1896, and he argues that ultimate GOP success rested on the party's pluralistic appeal to a broad range of ethnic groups and economic interests. The Republicans built their majorities in the Midwest not through coercion, fraud, or deception, but with hard work, flexibility, and a close attention to the concerns of the voters.

Lucid chapters on the styles of campaigning, on election practices, and on the religious basis of party affiliation are a part of Jensen's persuasive and sophisticated description of the Midwestern political environment. His work should dispose of the idea that Gilded Age politicians avoided serious issues and debated irrelevancies. The monetary question, the tariff, and prohibition were important because of their direct connection with the problems of an ethnically diverse industrial society. Similarly, politicians were hardly the gray nonentities of legend. A man who picked his way successfully through the hazards of Midwestern public life had survived a testing process that put no premium on mediocrity.

William McKinley's career before his election to the presidency offers a case in point. Jensen follows closely McKinley's rise through Ohio politics in the first half of the 1890s and stresses that he evolved a program of moderation on religious matters and a tolerance of ethnic difference. With its overtones of nationalism, assumption of social harmony, and promise of prosperity, the protective tariff gave McKinley a broadly based issue with which to woo majority support. Tirelessly preaching his message in graceful speeches across his state and the nation, he made himself the party spokesman for the doctrines of protection and pluralism. In 1896 a masterful campaign and the appeal of his policies carried the GOP to victory and dominance. Jensen's account plausibly suggests that McKinley played as large a part in building his party's enduring coalition as Franklin D. Roosevelt did for the Democrats after 1932.

*The Winning of the Midwest* is a tart, forthright work that abounds with stimulating insights. The innovative treatment of the shift in campaign tactics from the militaristic to the merchandizing style illuminates a neglected topic in the history of American elections and presents provocative clues about the decline of partisanship in this century. The impressive analysis of the impact of prohibition on the major parties confirms the need for a satisfactory history of that pervasive source of social discord. There are discerning and incisive remarks about the troubles of the Harrison administration, interesting observations on such prominent second-line figures as James S. Clarkson, and fascinating pages on the larger themes of William Jennings Bryan's candidacy in 1896.

Though clearly sympathetic to the victorious Republicans, Jensen is no mere apologist. His greater allegiance is to pluralistic politics, a point of view that understandably shapes his favorable attitude toward the GOP in the McKinley era. Obviously relishing the rough and tumble world of American politics, Jensen has recreated a decisive period in its development with richness and subtlety.

The review by Horace Samuel Merrill and Marion Galbraith Merrill of the Republican leadership between 1897 and 1913, on the other hand, moves in a much simpler conceptual world. They write from a profoundly cynical view of public life in which corrupt office-holders served dominant business interests and oversaw a docile, kept electorate. The authors find little genuine conviction behind Republican policies and regard the electoral process as a sham contest to mask sinister machinations between organized capital and politicians. Relying heavily on innuendo and implications of misconduct, the Merrills have constructed an indictment of the GOP and predictably render a verdict of guilty. "The party command," they assert, "could have served the nation much better had it performed with less elitism and more democracy."<sup>7</sup>

The resulting narrative reflects the passion and intensity of this position but will leave the knowledgeable reader unpersuaded. In their eagerness to make their case the Merrills repeat themselves,<sup>8</sup> commit errors of fact and citation,<sup>9</sup> and on occasion bend the evidence to prove a point.<sup>10</sup> More important, they have failed to assimilate the work of historians who have eroded older interpretations of politics in the Progressive era. It is no longer sufficient to invoke the specter of "the Republican-business alliance" as an inclusive explanation for party policy in light of studies that demonstrate deep divisions within the business community on specific issues.<sup>11</sup> The handling of factional contests within state Republican organization is equally unsatisfactory. To describe Robert M. La Follette as an angel of virtue and his opponents as members of "the cynical, unsavory Wisconsin Republican hierarchy" shows inadequate awareness of recent scholarship on politics in Wisconsin.<sup>12</sup> Though the Merrills cite some, but by no

<sup>7</sup> Merrill and Merrill, *The Republican Command*, 7.

<sup>8</sup> *Ibid.*, 24-25, 190-91; 111, 177; 28-29, 50, 52, 71, 166; 80, 96-97.

<sup>9</sup> Errors of citation include *ibid.*, 110, 188, 214 n.63. For errors of fact, see *ibid.*, 12, 13, 46-47, 55, 58, 81, 98, 185-86, 234, 244, 278, 285, 317.

<sup>10</sup> The authors use a letter from William Howard Taft in 1922 criticizing Oliver Wendell Holmes, Jr. as evidence of Taft's views in the early 1900s. They treat Elihu Root in similar fashion. *Ibid.*, 100, 110-11. Chiding Roosevelt for not campaigning in 1904, they do not mention the custom against presidents on the stump. *Ibid.*, 174, 179. They note that Republican Charles D. Hilles received private letters in 1911 and "slyly passed" them on to Taft. *Ibid.*, 329. (At the time Hilles was Taft's secretary.) Finally, they cite a letter from Senator Orville H. Platt as evidence of an admission of his ignorance on financial matters and later call this an "elusive device." *Ibid.*, 29, 61.

<sup>11</sup> *Ibid.*, 54. The Merrills cite, but do not grasp the implications of, Robert H. Wiebe, *Businessmen and Reform* (Cambridge, 1962). Unaccountably they do not use Richard M. Abrams, *Conservatism in a Progressive Era: Massachusetts Politics, 1900-1912* (Cambridge, 1964).

<sup>12</sup> Merrill and Merrill, *The Republican Command*, 33. Herbert F. Margulies, *The Decline of*



means all, of the more significant books on early twentieth-century public policy, their analysis remains rooted in categories and theories fashionable before the Second World War.

The tariff was a persistent source of dissension within Republican ranks after 1900, and the Merrills concentrate much of their text upon party efforts to resolve the question. But an archaic view of the political process prevents them from doing justice to the problems of protection. Apparently disdaining extended use of the *Congressional Record* and close scrutiny of debates and votes, they have depended too heavily on selected primary sources and secondary accounts, and their analysis suffers accordingly. In the case of the Dingley Tariff of 1897, for instance, they charge that Republicans regarded the final product as an embarrassment. Their evidence is a statement attributed to a senator but actually made by his biographer, a single newspaper clipping of dubious relevance, and the erroneous assertion that William McKinley "avoided public discussion" of the measure.<sup>13</sup> They also claim that the Republicans inserted a twenty per cent increase on certain unnamed goods into the Dingley Bill to compensate certain unspecified producers against possible injury from reciprocal trade provisions in the law. Yet their own evidence discloses the partisan origin of this allegation and the vigorous contemporary denials from the Republicans.<sup>14</sup> The analyses of McKinley's reciprocity

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the *Progressive Movement in Wisconsin, 1890-1920* (Madison, 1968); Stanley P. Caine, *The Myth of a Progressive Reform: Railroad Regulation in Wisconsin, 1903-1910* (Madison, 1970); and David P. Thelen, *The New Citizenship: Origins of Progressivism in Wisconsin, 1885-1900* (Columbia, Mo., 1972), all make clear the complexity of the Wisconsin situation.

<sup>13</sup> For the error in attribution, see Merrill and Merrill, *The Republican Command*, 47, and William Dana Orcutt, *Burrows of Michigan and the Republican Party* (New York, 1917), 2: 104-05. The Merrills refer to a clipping from the *New York Sun*, July 30, 1897, in which a railroad president, Melville E. Ingalls, failed to mention the tariff in a general discussion of the economy. Merrill and Merrill, *The Republican Command*, 47-48. A Gold Democrat who supported McKinley in 1896 and 1900 and William Jennings Bryan in 1908, Ingalls was cool toward protection and is a dubious source for Republican tariff attitudes. Melville E. Ingalls, "The Duty of the Democrats," *North American Review*, 171 (1900): 299-300; *Dictionary of American Biography*, s.v. "Ingalls, Melville Ezra." McKinley's "avoidance" of the tariff is discussed in Merrill and Merrill, *The Republican Command*, 48, but see also James D. Richardson, ed., *A Compilation of the Messages and Papers of the Presidents, 1789-1907* (Washington, 1908), 10: 26; McKinley's speech to the National Association of Manufacturers, Jan. 27, 1898, in *Speeches and Addresses of William McKinley from March 1, 1897 to May 30, 1900* (New York, 1900), 65, and his speech at Danville, Illinois, Oct. 11, 1899, *ibid.*, 257-58; *Official Proceedings of the Twelfth Republican National Convention, 1900* (Philadelphia, 1900), 148-49, 158.

<sup>14</sup> Merrill and Merrill, *The Republican Command*, 46. The charge about the Dingley Bill seems to have originated with McKinley's reciprocity commissioner, John A. Kasson, in testimony before a Senate committee in 1900. U.S. Senate, Committee on Foreign Relations, *Reciprocity Convention with France*, Senate doc. 225, 56th Cong., 1st sess., 1900, p. 69. It was expanded in John Ball Osborne, "Expansion through Reciprocity," in Robert M. La Follette, ed., *The Making of America* (Washington, 1907), 2: 382. The Merrills cite Osborne but not the Kasson testimony. For an effective refutation of Kasson's charge, see David Arganian, "McKinley and Commercial Reciprocity," (master's thesis, University of Wisconsin, 1958), 83-85. In September 1902 a Philadelphia newspaper carried a story that Nelson Dingley had admitted raising the rates in the bill for the purpose of securing reciprocity agreements. The *Washington Post*, Sept. 22, 1902, commented on the story, and the Merrills cite this editorial. They do not appear to have consulted the congressional debates that grew out of Democratic use of the allegation, or the detailed Republican denials that followed. *Congressional Record*, vol. 36, 57th

campaign, the Payne-Aldrich Tariff, and the struggle over Canadian reciprocity in 1911 demonstrate similar tendencies toward oversimplification and distortion.<sup>15</sup>

*The Republican Command* has the apparatus of scholarship, but it is actually a tract for the times, written out of the Merrills' concern "with the unnecessary suffering, waste, and danger which legislative inadequacy perpetuates in our society."<sup>16</sup> A deep contempt for the professional politician and an impatience with the mixed results of the democratic process pervades the book and shapes the Merrills' jaundiced appraisal of Republican action. In its simplistic view of what constitutes corruption, its readiness to see conspiracy in normal compromise, and its intolerance of explanations less melodramatic than its own, *The Republican Command* looks back to the era of Matthew Josephson and Charles Beard. This intellectual perspective gives it a certain nostalgic charm but makes it an unreliable guide to the Republican party in the period it covers.

After 1901 Republican unity, the product of the McKinley era, faded as the party divided over the proper role of government in regulating an industrial society. Few men participated more directly in the quarrels that rent the GOP than Albert J. Beveridge, and few careers more accurately mirrored the tensions within the party. Entering national politics in 1899 as an expansionist senator of conservative convictions, Beveridge became a proponent of reform causes, an insurgent against the leadership of William Howard Taft, and finally an ally of Theodore Roosevelt in 1912. During the last fifteen years of his life he combined successful excursions into biography with a fruitless quest for a political comeback as an opponent of the reforms he had earlier endorsed. John Braeman's careful biography goes directly to the problem of Beveridge's Progressivism and, in the process, clarifies the central question of Republican history in this period, the trauma of 1912.

The transition from regular to reformer owed something to Beveridge's well-known presidential ambitions but more to his belief that the party should advocate policies to alleviate popular discontent and avert more drastic social change. Unlike the Republicans of Hoar's stamp who legislated from a faith in social cohesion, Beveridge and his fellow Progressives wanted to preserve the stability of society through moderate reform. In the era of Theodore Roosevelt this position made him an innovator in comparison with conservatives who clung to the view that government might encourage enterprise but should not regulate it. But

Cong., 2d sess., Jan. 6, 1903, p. 517; Jan. 8, 1903, pp. 598-97; Jan. 12, 1903, pp. 668-71; Jan. 13, 1903, pp. 711-15. The U. S. Tariff Commission, *Reciprocity and Commercial Treaties* (Washington, 1919), 202-03, summarized the highpoints of this controversy, and the Merrills rely heavily on this source. They did not, however, trace down the footnote on page 203 that would have led them to the *Congressional Record*, vol. 44, 61st Cong., 1st sess., July 3, 1909, pp. 4081-85, and a recapitulation of the 1903 debates.

<sup>15</sup> Merrill and Merrill, *The Republican Command*, 87-90, 279-98, 317, 320-24.

<sup>16</sup> *Ibid.*, ix.

Beveridge's reform impulse was fragile. When he confronted the demands of interest groups, like organized labor, more concerned with concrete goals than social harmony, he retreated toward the conservative stance of his former enemies. This process paralleled the experience of other Progressives and suggests why the Republicans forfeited their place as the party of change to the Democrats after 1912.

Since tariff reform played a large part in Beveridge's evolution as an insurgent, Braeman closely scrutinizes such crucial episodes as the Payne-Aldrich Tariff fight of 1909. A revival of interest in this central problem of Republican policy is overdue. Protection stood at the heart of party doctrine, but it has been passed over in favor of more glamorous topics like conservation or the railroads. Detailed studies of the various tariff bills, and of the reciprocity movement generally, are necessary before the exact course of the Republican decline can be traced. Braeman shows, for example, how Beveridge's opposition to high tariffs reinforced his suspicion of partisan politics and implicitly hints that the tariff was a key element in shaping the antiparty sentiment of the Progressive years. The references to the tariff and the high cost of living indicate that an investigation of the relationship between inflation and Republican misfortune after 1908 is in order. There is also further evidence of Theodore Roosevelt's timidity on the tariff, a subject that needs more attention.<sup>17</sup>

After Beveridge's election to the Senate at the age of thirty-six, friends forecast further successes for the young Hoosier statesman. Somehow the bright hopes were never fulfilled, and he remained a "promising" young man for the next two decades. Traces of the anticlimactic quality of Beveridge's life appear in this biography. The fault is probably Beveridge's, but Braeman's fondness for clichés and tendency toward repetition slow the narrative. Just as his subject was happiest working on biographies of John Marshall and Abraham Lincoln so Braeman appears more comfortable in the concluding chapters when Beveridge leaves politics for the delights of history.

Several years ago Richard Abrams urged his colleagues to "get on with 'The History of Progressive Era Politics.'"<sup>18</sup> Taken together these four books reveal how much remains to be done in that regard for the Republicans. Until there are studies of the tariff that go beyond the work of Frank W. Taussig and Edward Stanwood, the essentials of Republican theory will remain unclear. Investigations of the presidential administrations of William McKinley and William Howard Taft would enable

<sup>17</sup> Braeman, *Beveridge*, 122-67. George E. Mowry, *Theodore Roosevelt and the Progressive Movement* (Madison, 1946), 45, Wiebe, *Businessmen and Reform*, 95, and Abrams, *Conservatism in a Progressive Era*, 90-91, 222-23, 233, among others, have noted the impact of the high cost of living, but the subject needs more examination. Albrow Martin, *Enterprise Denied: Origins of the Decline of American Railroads, 1897-1917* (New York, 1971), 11-17, among its many other merits, stresses the effect of rising prices. Richard C. Baker, *The Tariff Under Roosevelt and Taft* (Hastings, Nebr., 1941), is still the best study of Roosevelt and protection.

<sup>18</sup> *AHR*, 74 (1968-69): 314.

historians to place the contributions of Theodore Roosevelt in a more balanced perspective. Roosevelt's relationship with his party needs dispassionate examination; one fruitful approach would be to look at his dealings with Republicans in states like New York, Iowa, and Ohio. The presidential elections that followed the contest of 1896 also require systematic scrutiny along the lines of Jensen's work. In particular the election of 1912 demands a full-scale exploration of the huge mass of available sources on the Republican disaster. Finally, important party leaders like Henry Cabot Lodge, Albert Baird Cummins, and Robert La Follette should receive fresh biographical treatment of the caliber of Welch on Hoar and Braeman on Beveridge.<sup>19</sup>

Intense partisan debate characterized American politics between 1877 and 1913, and echoes of these battles have persisted in historical scholarship. The survival of analytic premises left over from contemporary controversies remains an important obstacle to a proper understanding of the Republican party in this period. As the Merrills' book demonstrates, the sterile categories of "reformer" and "conservative" no longer answer the important questions about the rise and fall of the GOP. Welch, Jensen, and to a lesser extent Braeman avoid facile moralistic generalizations and go about the serious business of writing party history. They accept politics as a legitimate occupation in a democratic society, recognize the internal rules of that profession, and mete out judgment only after a careful evaluation of context and circumstance. As a result their work contributes to a more balanced appraisal of the Republicans and enriches the literature of political history.

<sup>19</sup> David W. Detzer, "The Politics of the Payne-Aldrich Tariff of 1909" (Ph.D. dissertation, University of Connecticut, 1970), and James O. Wheaton, "The Genius and the Jurist: A Study of the Presidential Campaign of 1904" (Ph.D. dissertation, Stanford University, 1964), show what still can be done with worked-over or seemingly uninteresting topics. Joel A. Tarr, "President Theodore Roosevelt and Illinois Politics, 1901-1904," *Journal of the Illinois State Historical Society*, 58 (1965): 245-64, is a model for an analysis of Roosevelt's dealings with state parties, and Tarr's *A Study in Boss Politics: William Lorimer of Chicago* (Urbana, 1971), makes an even more substantial contribution. Ralph Mills Sayre, "Albert Baird Cummins and the Progressive Movement in Iowa" (Ph.D. dissertation, Columbia University, 1958), is sound, but Cummins's tariff and railroad views could stand more investigation. James E. Hewes, Jr., "Henry Cabot Lodge and the League of Nations," *Proceedings of the American Philosophical Society*, 114 (1970): 245-55, suggests the possibilities in a new study of Lodge. Professor David P. Thelen is at work on a biography of La Follette that should provide a balanced perspective on the Wisconsin senator.

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## Reviews of Books

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### GENERAL

MATTHEW MELKO. *The Nature of Civilizations*. Introduction by CRANE BRINTON. (Extending Horizons Books.) [Boston:] Porter Sargent Publisher. 1969. Pp. xvii, 204. \$4.95.

BERNARD NORLING. *Timeless Problems in History*. Notre Dame, Ind.: University of Notre Dame Press. 1970. Pp. x, 301. Cloth \$8.00, paper \$3.25.

Professor Melko's work deals principally with periods; Professor Norling's book deals principally with problems of history. The first work tries to grapple with the knowledgability of all history, the second book proceeds from the historicity of our knowledge. They should thus complement each other. Unfortunately they do not. There is a profound difference between the quality of these works. Much of this difference results from the divergent scope of the two authors. Professor Melko has tried to do (or perhaps outdo) a Spengler or a Toynbee. He tries to establish certain basic laws, or at least certain basic characteristics, of what must necessarily constitute an entire civilization. He feels compelled to use heavy sociological language, at times skidding into formulations where vocabulary comes close to serving as a kind of substitute for thought. Elsewhere the style is jagged, a curious mix of popular cheeriness and academic jargon. There are also certain oddities, not always endearing, as when Professor Melko chooses to utilize the crankish lucubrations of a New England colleague, called "The Bowler Continuum," which "can also be used to describe fluctuations in attitudes. Thus Sam Jones, sitting in church, may concentrate alternately on the sublime love of God and the temporal beauty of Susie Smith, sitting in a summer frock, two rows down and

to the left" (p. 77). There follows an imbecilic chart in which God and Susie Smith float suspended between "Total Material Concern" and "Total Spiritual Concern," Bowler's sublime formulas. This kind of silliness is not untypical of Professor Melko's work.

Professor Norling is more modest; and, consequently, also more serious. It is not easy to sum up his book either, but for the opposite reason from Professor Melko's. Norling deals with many serious matters. These include illusions in history, a discussion of inevitability and of determinism, the legacies of revolutions and of national character, and certain conditions of cultural achievements. This list does little justice to this book, which is both an important corrective and a useful introduction for the purposes of undergraduates (and also of first-year graduate students) who ought to know something about what historical problems are. Professor Norling's thinking is in the tradition of Herbert Butterfield's to the effect that the principal purpose of historical study should deal with problems rather than with periods. While Melko speculates about the nature of periods Norling is deeply interested in the nature of problems. His brief footnotes reveal an extraordinary breadth of reading in many different fields. *Timeless Problems in History* may be too modest a title. The book fulfills a potential service that is comparable to the late Crane Brinton's *Anatomy of Revolution*. (By a curious, and perhaps paradoxical, coincidence Professor Melko's book is furnished with a brief introduction by Crane Brinton.)

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MORTON A. KAPLAN. *On Historical and Political Knowing: An Inquiry into Some Problems of Universal Law & Human Freedom*. Chicago: University of Chicago Press. 1971. Pp. xvi, 159. \$6.50.

Books like this one in which a learned scholar seriously "reflects" on significant critical and philosophical problems associated with rich conceptual subject matter are very much needed. Yet they seldom achieve a clear-cut success. Their brevity prohibits a sufficiently detailed analysis of the issues to satisfy the experts, while the inherent difficulties of the topics treated will often discourage interested "by-standers" working in other scholarly fields. Professor Kaplan has done about as well here as can be expected.

Two lengthy essays, "Knowing and Explaining" (pp. 3-71) and "Historical Consciousness and Truth" (pp. 73-137) account for most of the book. In the first essay Professor Kaplan clearly sides with the "covering-law" theorists and criticizes Professor Michael Scriven, a prominent philosopher, for arguing that historical explanations are "nontrivial truisms." Seven pages are quoted from a relevant article by Scriven (pp. 24-31), followed by the author's pointed criticisms in rebuttal (pp. 31-37). Yet Professor Kaplan later admits that historians normally achieve only "explanation sketches" (rather than laws), though these are stronger than mere truisms. Summarizing this valuable essay he writes: "Even though we do not have real theories or laws in social science—at least yet—we do have some theory sketches and explanations of a looser sort" (p. 69). His claim is that in practice social scientists find difficulty in formulating scientifically convincing laws.

In "Historical Consciousness and Truth" the argument again claims that "most social science theorizing will likely remain qualitative and comparative" after a careful five-page analysis (pp. 121-25) of why quantitative success is improbable. A valuable part of this challenging essay (pp. 130-37) is concerned with the tormented "question" whether and to what extent social science is and can be value free.

A briefer concluding essay, "Freedom in History" (pp. 139-59), argues in a lively way that freedom is a relational as well as a contextual concept. Unlike the earlier two, this essay seldom refers to important arguments and theo-

ries in other books by Professor Kaplan; hence it is more self-contained and easier to understand. Probably his *Macropolitics: Essays on the Philosophy and Science of Politics* (1969) and *Dissent in the State in Peace and War* (1970) are important to a proper grasp of the reasoning in this volume.

In this stimulating but difficult book Professor Kaplan contributes assorted logical, methodological, and evaluative observations of the best kind to the wisdom literature of modern social science.

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MARTIN BALLARD, editor. *New Movements in the Study and Teaching of History*. Bloomington: Indiana University Press. 1970. Pp. vi, 234. \$6.95.

HAROLD PERKIN, editor. *History: An Introduction for the Intending Student*. London: Routledge and Kegan Paul; distrib. by Humanities Press, New York. 1970. Pp. vii, 214. Cloth \$5.50, paper \$2.50.

These books are collections of essays on the study and teaching of history written chiefly by English practitioners. The more significant of the two has been edited by Martin Ballard, who is director of the Educational Publishers Council of England and has taught history at both Burlington Comprehensive School in Bristol and Clare College, Cambridge. In it is assembled a stimulating and highly useful group of essays by a wide range of authors, including noted historians such as William S. McNeill (one of two Americans represented), Arnold Toynbee, G. R. Elton, and Peter Mathias, as well as teachers in English schools of education and secondary schools. Notable among the contributions are McNeill's call for new patterns for comprehending the past to accommodate new needs, including an intelligible picture of world history; Toynbee's plea that historians see themselves as both generalists and, in some particular part of their work, specialists and that they aim both their teaching and their writing at a cultivated and intelligent but nonspecialized public; a discussion of the implications of Piaget for history teaching at different age levels by Roy Hallam; and a highly suggestive essay on the use of sources

in the classroom by Peter Bamford of Richard Taunton Sixth Form College, Southampton, which includes practical suggestions for preparing students and for using such immediately available sources as the local church. Equally notable and suggestive are discussions by E. A. Wrigley and Mathias of the implications for both study and teaching of new methodology, particularly in connection with family, demographic, and economic history; an essay by Marcus Cunliffe suggesting the particular resonance of United States history for the asking of modern questions, with useful suggestions of particular themes as examples; and a challenging essay on the relationship between the study of history and biology by C. D. Darlington, professor in botany at Oxford.

As Ballard points out in a thoughtful introductory essay, the collection has no common point of view except an assumption that history teachers must face up to change. By inference the collection suggests several crucial and related ideas. One is that in considering change, questions of how and why one teaches in the field are inextricably connected with questions of how and why one studies in it. One regrets that missing in the essays, except the one on Piaget, is any serious consideration of the still more fundamental question of how and why people learn. A second is that pressures for change are springing up not only outside the discipline, in demands for "relevance" and competition from other disciplines, but also within, where they emanate from new questions historians are asking and from new methodology. A third is that the problems of history study and teaching are much the same at all educational levels, even though the answers may necessarily be different, and that practitioners at all levels have much to learn from one another if only they would make the effort to communicate.

Harold Perkin, professor of social history at the University of Lancaster, has compiled a book less committed to change. Designed "for the intending student" at the university level, the book asks where the study of history stands in various traditional subject matter fields: what themes are being emphasized, what research methods used, what questions asked. Answers are offered in brief but thoughtful essays, all by present or former faculty at the Univer-

sity of Lancaster. All are "content" oriented. None is much concerned with the methodology either of teaching or of learning. And yet all take seriously the teaching-learning task, as do the essays in the Ballard book. Both books are examples of a genre of historical literature and of professional concern that have too few equivalents in the United States.

RICHARD H. BROWN  
Newberry Library

PAUL F. KRESS. *Social Science and the Idea of Process: The Ambiguous Legacy of Arthur F. Bentley*. Urbana: University of Illinois Press. 1970. Pp. xiv, 260. \$8.95.

The stature of Arthur F. Bentley (1870-1957) may be indicated by political scientist Peter Odegard's remark that Bentley's *The Process of Government* (1908) stands as "one of the very few indispensable books" in the literature of the behavioral sciences. Paul Kress's monograph on Bentley and the idea of process should also prove indispensable, especially to those historians, and their number seems to be growing, concerned about the methodological bases of their projects.

Kress goes beyond previous commentators to show how Bentley's work contains implications still largely ignored by American social scientists. Bentley has generally been studied as the father of "the group theory of politics," because of the influence of the second part of his 1908 work entitled "Analysis of Governmental Pressures." Scholars working in this tradition, in Kress's words, "seem to have made the 'group' their central concept and to have allowed 'process' to become the general milieu in which the group 'functions.'"

Kress focuses on "process" rather than on "group," and he succeeds in demonstrating that Bentley was far more interested in the former concept than he was in the latter. The heart of Kress's analysis is a close reading of Bentley's major theoretical works combined with an assessment of the extent to which American sociologists and political scientists have met the challenge posed by Bentley's image of a universe of process. To paraphrase Kress, if Bentley and subsequent analysts conceived of the social realm as "homogeneous, continuous action" then they were faced with the task of creating "differentiating principles,

ways to re-introduce discreteness—in short, new units of investigation.” Kress contends that Bentley and his successors at the task have by and large failed. The chapters on “Process in American Sociology,” “Process as Transaction,” and “Process as System,” though at times unnecessarily opaque, are superb essays in their own right. They describe both the evolution of Bentley’s intellectual constructions and the socio-intellectual context of later writing pertinent to the issue. Bentley’s attempts to confront the philosophical issues central to the process concept culminated in a transactional analysis replete with arcane terms such as “communicane,” “dicaudane,” and “scriptillect.” Bentley hoped with such a vocabulary to express the “relative stresses and emphases” in a process social realm, as well as to avoid the dualisms inherent in everyday language.

While Kress regards Bentley’s attempt as heroic, he by no means sees Bentley as his hero. In company with other political scientists skeptical of “pluralism” and influenced by phenomenology and existentialism, Kress argues that political theory is “seriously threatened by the abolition of units [called for by the process concept] whether particulars, individuals, or persons. . . . Bentley’s mistake was to seize upon the explanatory weakness of common sense thought and then find himself forced to repudiate the integrity of human experience itself.” This conclusion, like the entire monograph, raises critical questions for historical studies as well as for social science. Any historian interested in “the behavioral approach,” in “the new political history,” or in the relationships between history and the social sciences should put this book on his list of required reading.

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EDWARD WHITING FOX. *History in Geographic Perspective: The Other France*. New York: W. W. Norton and Company. 1971. Pp. 190. \$6.95.

In a slim book full of things Professor Fox reconsiders the history of France as a test case for “the proper reintroduction of the geographical dimension” into historical study. To this timely invitation one would like to add a plea

for geography in its own right. More often than not in American universities geography is not even recognized as a unified subject for doctoral dissertations but split between the departments of geology, anthropology, the natural and the social sciences; history gets no part of it. Is this not the best method to turn out geologists who are reckless of ecology, anthropologists who are unaware of environment, planners who disregard cultural and climatic variables, and historians who cannot tell Washington from Moscow? “Area studies,” carried no further than the introductory level, can hardly make up for the murder of geography or its expulsion from history.

The editor of the Oxford atlases of European and American history, Professor Fox has been once more attracted to the geographical dimension by his search for a method in the apparent madness of contemporary French political behavior. None of the learned, ingenious, or witty explanations offered by French historians explains every turn and contradiction of the latest two hundred years; Herbert Lüthy, a Swiss historian, sums them up under the title *France Against Itself*. Professor Fox plunges far back into historical precedents, broadens his exploration into European and Mediterranean events of the ancient and early medieval periods, and concludes that there are two Frances acting at cross-purposes. The inner core is fundamentally agrarian, self-supporting, inward-looking, and devoted to a strong government (whether monarchic or republican it does not matter greatly) that will give the country law, order, and a thorough administrative guidance. The outer fringe, mainly consisting of seaports and other cities accessible to water-borne commerce, is open to foreign influences, change, nonconformist thought and practices, and unafraid of radical tendencies and self-governing institutions. This bipolarity is neither abnormal nor peculiarly French: it can be observed again and again in history, ever since maritime Greece faced land-based Persia.

These considerations are less novel for the ancient and early medieval world than for modern France; but even where he runs over thoroughly explored ground, Professor Fox offers a few incidental remarks that strike the specialist of the field as a graphic observation



of a perceptive foreign correspondent that may impress a native (one example among many: "The Franks had not been deprived of important revenues by the rise of Islam, simply because the trade from which this income would have had to derive played no significant role in the economy of Gaul").

I am not fully convinced that his explanation of modern French politics takes care of all objections; it is well documented and sounds plausible, but every effect has many causes, and there usually is no single key to any complicated safe. Here and there, one senses a certain effort in correlating every quirk to the same leitmotif. Still the leitmotif rings clearly, and it was important to attract attention to it. What is more, the geographical dimension has been properly introduced in a pattern of behavior that is French because human, and human because French.

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JOHN HICKS. *A Theory of Economic History*. New York: Oxford University Press. 1969. Pp. vii, 181. Cloth \$5.00, paper \$1.95.

JONATHAN HUGHES. *Industrialization and Economic History: Theses and Conjectures*. New York: McGraw-Hill Book Company. 1970. Pp. xii, 336. \$6.95.

The English Industrial Revolution is the focal point, the burning glass of modern history. Into its decades streams a diffuse light—the flickerings of capitalism from Greece through the Dutch Republic. From it pours out the intense heat of modern industrial civilization. This at least is the form imposed on history by a study of economics. It is wholly congruent to the form imposed by a faith in the divinity of Christ. The decades of the Industrial Revolution are the decades from Christmas to the Passion in the material history of the modern world.

The two books here noticed fit together almost exactly at the point of this revolution. Both are extended essays in the grand synoptic tradition of economic history. Both betray that subject's origins in the Enlightenment and the urge to provide a rational alternative to Christian myth. In ten compelling—though hardly conclusive—chapters, J. R. Hicks turns the mind of a great economic theorist to the mate-

rials on the rise of capitalism. His theme is an old one—how the growth of trade and the culture of the trader undermined and transformed the economic organization of agrarian societies, producing at last the thoroughgoing capitalism out of which nineteenth-century industrialism could grow. J. R. T. Hughes begins with that growth and follows the nineteenth-century diaspora of elements of income growth among the nations from England to the Continent and overseas and the progressive wreckage of economic forms and ideologies, both "native" and capitalistic, that has littered the world since Marx, Mill, Marshall, and Queen Victoria.

To note their complementarity in coverage, theme, and viewpoint, however, is to give little of the flavor of the two books. Both are highly personal artifacts and derive from persons of markedly different temperaments and culture. Hicks, casting his web over the West, weaves arguments like a spider, one strand at a time. His concern at all points is to be not exhaustive but plausible and, above all, clear. One catches him on occasion in that tone of faint condescension, reminiscent of the language of Alice in Wonderland, that theorists must adopt in order to communicate with the academic multitude. Unfortunately, the materials of history do not readily give themselves to the devices of so cool a rhetoric. Nor can an American review suppress a provincial irritation at the casualness with which an Oxonian scholar surveys the world. Of sixty-three references to other works, the frequency distribution of places of publication is as follows: Oxford 13; London 13; Cambridge 11; New York 3; Paris 2; all others, 1 each. The result of Hicks's mind working on history is not the perfect and satisfying magic of *Value and Capital* (1946), his great work in economic theory. He has produced not a theory of economic history, but a theorist's economic history—a different, but a more human and interesting thing.

Hughes's personal style is quite different—and thoroughly American. Superficially, he appears to have written a book highly usable as an undergraduate text. Written with clarity and vigor, with ample bibliography, forthright judgments, and immense coverage, the book must interest students who, however, may fail to appreciate the breadth of its learn-

ing and the subtlety of its judgments. The essays on imperialism, economic doctrines, and the interwar period are particularly original and valuable.

If economic history, then, is a religion whose theology has a certain chronological form, Hicks expounds the Old Testament in the style of an Anglican bishop and Hughes proclaims the New a bit in the manner of the Methodist minister of a large city church. Yet the message is the same—it is a message of redemption through modern capitalistic forms and industry. What is missing from it is a sensitivity to the inner logic by which economic and social life may be organized outside the market economies and directed to other ends, less individualistic and less materialistic. Yet what is interesting in the whole span covered by the two books together is the cyclical character of the evolution, from nonmarket forms through the market back to nonmarket organization again. Splitting economic evolution at the Industrial Revolution tends to obscure this huge cyclical form that the history exhibits.

And oddly enough, one other omission in both books appears: the omission of technology as a historical force. Hicks explicitly excludes it in his definition of things economic (p. 71). And Hughes, having given the topic full credit in the initial English developments, loses it in a discussion of entrepreneurs, laws, thought, policies, and capital movements of the nineteenth century and thereafter. Yet Hicks presumably would not deny that it was the presence of this catalyst that distinguished the late eighteenth-century revolution from its predecessors in Greece or in early modern times. Nor could Hughes deny that the spread of modern industry occurred most easily where resource and transport conditions were best suited to the developing technology. Hicks and Hughes then must be supplemented by David Landes, who supplied if not Christ, then at least Prometheus, as the superstar to the story. The three authors' books taken together supply an interested historian with a useful and intriguing introduction to modern history as modern economists view it. If all three get lost in their subjects to some degree, that should not be taken to show their hopeless indifference to modern economics, as some recent critics of Landes in this journal have averred (*AHR*, 76

[1971]: 467–74). It is but a sign of their baptism in the subject matter of economic history—by a sprinkling of holy water in the case of Hicks and, for Hughes and for Landes, by total immersion.

WILLIAM N. PARKER  
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JACQUELINE MURRAY. *The First European Agriculture: A Study of the Osteological and Botanical Evidence until 2000 B.C.* Edinburgh: University Press; distrib. by Aldine Publishing Company, Chicago. 1970. Pp. vii, 380. \$10.75.

Derived from a doctoral thesis at Edinburgh University this book represents an ambitious attempt at summarizing the botanical and osteological data produced by more than one thousand excavations in the Near East and Europe. The author has not undertaken new identifications but instead has assembled the published evidence and drawn conclusions that she hopes will "mark the culmination of one stage of research, and provide the basis for the beginning of another."

The conclusions so reached show that agriculture began in the Near East ca. 9000 B.C. with the domestication of sheep and goats. Some time before 7000 B.C. the dog was also domesticated and the cultivation of plants begun. Included were two species of wheat, einkorn and emmer (both of which have wild relatives extant), and barley of the two-rowed type that is quite similar in its characteristics to the present-day wild barley of the Near East.

During the late Neolithic cattle and pigs were added to the domestic stock, and six-rowed barley and bread wheats made their first appearance. The author erroneously attributes the origin of these wheats to a chromosome aberration in the tetraploid emmer. Since geneticists regard the discovery and cultivation of the hexaploid bread and club wheats as one of the most important developments in the evolution of Old World agriculture, this error is not minor. It has been known for many years that these wheats are allopolyploid hybrids—the product of adding a third genome to the tetraploid complex. Since 1956 it has been well established that this third genome is derived from the wild grass *Aegilops speltoides*.

From the Near East the practice of agriculture spread first to Greece, ca. 5000 B.C., and later across Europe. The author treats the spread as a form of colonization; she recognizes no evidence of an independent origin of agriculture in Europe. This is perhaps the book's most important conclusion.

In pursuing her prodigious labors the author has reviewed the literature up until February 1968 to produce a bibliography that includes about 450 entries of works embracing at least ten different languages. Although this will prove useful as a reference work it is already incomplete because of a number of significant contributions published in 1968 or later. Because of the enormous amount of detail involved in the text, the 210 tables and the 137 histograms, the lack of an index is an especially unfortunate omission.

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G. J. CUMING and DEREK BAKER, editors, *Councils and Assemblies: Papers Read at the Eighth Summer Meeting and the Ninth Winter Meeting of the Ecclesiastical History Society*. (Studies in Church History, Number 7.) New York: Cambridge University Press. 1971. Pp. xiv, 359. \$16.00.

Twenty-two essays presented at the eighth summer meeting and the ninth winter meeting of the Ecclesiastical History Society are brought together here. All deal with some piece of legislation or aspect of life connected with a Church synod. The temporal span covers the Early Middle Ages to the twentieth century. The geographic area is Western Europe, with the exception of D. M. Nicol's paper on the Byzantine reaction to II Lyons (1274), Peter Hinchliff's remarks on Bishop Gray's work in South Africa at the middle of the last century, and S. P. Mews's survey of Anglo-Catholic criticism of the Kikuyu Conference (Kenya, 1913). There are fifteen studies of Roman Catholic assemblies and seven of Anglican or Protestant gatherings. The councils of Christian antiquity do not appear nor, among the Latin ecumenical convocations, do III, IV, V Lateran, I Lyons, Vienne, and Trent. Of recent interfaith movements—Life and Work, Faith and Order, the World Coun-

cil of Churches—none provides basis for an essay. Several studies probe substantial questions, with wide-ranging implications; others are more limited. As a collection of independent essays, both quality and scholarship are commendable.

The following gives a brief sense of the contributions: Walter Ullmann's "Public Welfare and Social Legislation in the Early Medieval Councils" shows that Frankish and Visigothic synods of the sixth to ninth centuries continued to concern themselves with alleviating the condition of the poor, the ill, the slaves, etc. as had their predecessors in fifth-century France. J. L. Nelson's "National Synods, Kingship as Office, and Royal Anointing: An Early Medieval Syndrome" traces royal anointing in Spain to 672, in France to 848, in Germany to 911, and in England to 973, and interprets it as an episcopal monopoly. In "The Case of Berengar of Tours" Margaret Gibson discusses the eucharistic statements accepted by Berengar at councils in 1059 and 1079, which foreshadow later teaching on transubstantiation. M. J. Wilks's "*Ecclesiastica* and *Regalia*: Papal Investiture Policy from the Council of Guastalla to the First Lateran Council, 1106-23" indicates that Pope Paschal II's policy at Sutri-Rome (1111) was in line with a developing distinction between episcopal possessions and jurisdiction to be regulated by canon law and a bishop's regalian functions legitimately subject to the law of the land. In "*Viri religiosi* and the York Election Dispute" Derek Baker argues that the twenty-eighth canon of II Lateran Council (1139) giving religious orders a consultative voice in episcopal elections had little influence upon the choice of a successor to Archbishop Thurstan of York in 1140. Peter Linehan's "Councils and Synods in 13th-century Castile and Aragon" gives a capsule description of the efforts of the Aragonese archbishop Pedro de Albalat of Tarragona (d. 1251) to eliminate concubinage and to improve the education of the clergy in contrast to the lack of concern in these matters evidenced by the Castilian prelate Rodrigo Ximénez de Rada of Toledo (d. 1247). In "The Byzantine Reaction to the II Council of Lyons, 1274" D. M. Nicol chronicles in depth eleven years of rejection at Constantinople of the union with the papacy accepted at Lyons

on July 6, 1274. Brenda Bolton ("The Council of London of 1342") describes the twenty-nine enactments of a provincial assembly under Archbishop Stratford of Canterbury. R. M. Haines's "Education in English Ecclesiastical Legislation of the Later Middle Ages" is a recounting of the measures taken by thirteenth- to sixteenth-century English bishops to insure a minimal doctrinal formation for parish priests and laity. Joseph Gill's "The Representation of the *universitas fidelium* in the Councils of the Conciliar Period" deals with the nonmitred participants at Pisa (1409), Constance (1414), Basel (1431), and Florence (1438), and it questions whether they were representative of any but themselves. Margaret Harvey's "Nicholas Ryssheton and the Council of Pisa, 1409" describes four short *quaestiones* presented at or after the 1409 synod by an English canonist. In "The Condemnation of John Wyclif at the Council of Constance," E. C. Tatnall extricates from the 1414-15 proceedings of the synod against Jan Hus its measures in condemnation of Wyclif (d. 1384). A. N. E. D. Schofield's "Some Aspects of English Representation at the Council of Basle" lists what is known of the two English delegations sent to the council in 1433 and 1434-35. A. J. Black ("The Council of Basle and the II Vatican Council") sees Basel's themes on the sovereignty of the whole Church, episcopal collegiality, and the ministry as service in relationship to the deliberations of the recent ecumenical council. Basil Hall's "The Colloquies between Catholics and Protestants, 1539-41" views the discussions at Hagenau, Worms, and Regensburg as reflective of a spirit of understanding that reappears in twentieth-century ecumenical dialogue. W. B. Patterson's "King James I's Call for an Ecumenical Council" depicts royal proposals in 1603 and 1604 for an interfaith gathering, which Pope Clement VIII rejected. In "John Hales and the Synod of Dort" Robert Peters describes the reactions of a sympathetic English observer at the assembly (1618-19) who grew disillusioned with its rigid Calvinism. "Assembly and Association in Dissent, 1689-1831" by G. F. Nuttall is an account of the Nonconformist ministerial Exeter Assembly in Devon from its founding to the establishment in 1831 of national Congregational and Baptist Unions. G. V. Bennett's

"The Convocation of 1710: An Anglican Attempt at Counter-Revolution" relates the unsuccessful efforts of the High Churchman Francis Atterbury to forge a working partnership between the lower houses both of Convocation and Parliament toward the renewal of Church of England discipline. Peter Hinchliff's "Laymen in Synod: An Aspect of the Beginnings of Synodical Government in South Africa" treats the steps from 1855 to 1870 whereby Bishop Gray of Capetown succeeded in bringing the laity into diocesan and provincial synods. In "The First Vatican Council," E. E. Y. Hales suggests that the handling of papal primacy and infallibility at the synod of 1869-70 was both balanced and the outcome of genuine debate. S. P. Mews contends in "Kikuyu and Edinburgh: The Interaction of Attitudes to Two Conferences" that contemporary Anglo-Catholics were happy with the approach to Christian unity at the Edinburgh Missionary Conference (1910) but fearful that an open communion service used at Kikuyu (Kenya, 1913) would lead to a jerrybuilt Christianity.

HENRY G. J. BECK

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MONTE D. WRIGHT and LAWRENCE J. PASZEK, editors. *Science, Technology, and Warfare: The Proceedings of the Third Military History Symposium, United States Air Force Academy, 8-9 May 1969*. [Washington:] Office of Air Force History, Headquarters USAF; [USAF Academy, Colo.:] United States Air Force Academy. [1971.] Pp. xvi, 221. \$1.25.

In 1969 the third in a series of conferences on the history of war sponsored by the Air Force Academy addressed itself to a theme of great contemporary importance, much of whose earlier history remains uncertain: the interaction of science and technology with military organization and performance. The papers and commentaries have now been published, together with the Eleventh Harmon Memorial Lecture, delivered during the conference by Elting E. Morison.

In an admirably lucid survey A. Rupert Hall demonstrates that until the eighteenth century warfare owed little to science. He is effectively supported by his commentators—J. R. Hale, who identifies organizational, social, and intellectual elements in the military that

insulated them from science, and John B. Wolf, whose suggestion that the two realms could not draw closer until sophisticated civil-military bureaucracies developed after the Thirty Years' War introduced a secondary theme of the conference—the role of management. In the second major paper David Bien disputes the view that the founding of the *École militaire* in 1751 was a response to the French army's new technical needs. Indeed such needs scarcely existed. Rather the new stress on mathematics in the training of subalterns was intended to guard them against possibly corrupting, antiprofessional influences of the standard classical education. Not surprisingly Bien's elegant reinterpretation troubled some members of the symposium; nevertheless the thesis that scientific subjects rose to prominence in military curricula for psychological rather than technical reasons is buttressed by two informative comments: Gunther Rothenberg's review of conditions in the Austrian army and John Shy's comparative analysis of European military academies during the revolution and the Napoleonic Age.

In contrast to the imaginative approach taken to the early modern period the paper by I. B. Holley, Jr. on the evolution of operations research in the Air Force is largely narrative. The fact that some protagonists are still living obviously presents problems, as does the unavailability of important documents; yet Robert L. Perry in his critique shows how much scholars can do already to identify and interpret the central elements in the failure of the Air Force to establish a research office competent to deal with the major technological and strategic issues of the 1950s and 1960s. Finally Melvin Kranzberg suggests models for the interaction between science, technology, and warfare in America since 1945.

While the symposium could not hope to explore its theme evenly over six centuries, it is too bad that the interests of the main speakers caused a particularly relevant period to be neglected—the years from 1815 to 1945, during which the separation of science and war gradually and decisively came to an end. Professor Morison's lecture, "The War of Ideas; The United States Navy, 1870-1890," might have gone far to fill the gap; but as he was address-

ing the entire cadet wing rather than the conference proper he was precluded from fully developing his thesis that the navy was incapable of innovation because it lacked a realistic conception of its duties. Nevertheless this is a stimulating volume, of more than specialist appeal, which, incidentally, bears out an impression that American historians who today write on war fall, broadly speaking, into two groups: those who recognize that topics in the history of organized violence are always defined by the interaction of civil and military elements, which must be analyzed before the course of military events can be traced, and others who derive whatever analytic unity their themes might possess exclusively from the beginnings and ends of a campaign or a career.

PETER PARET  
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CAMILLE LIMOGES. *La sélection naturelle: Étude sur la première constitution d'un concept (1837-1859)*. ("Galien": Histoire et philosophie de la biologie et de la médecine.) Paris: Presses Universitaires de France. 1970. Pp. 184. 25 fr.

Camille Limoges has written a short but excellent account of the development of the theory of natural selection. Mr. Limoges's book supersedes most other work done on the topic in two respects. First, having studied French science he is better acquainted with the great French naturalists of the eighteenth and early nineteenth centuries than are most English-speaking commentators on the history of evolutionary thought. He has used that familiarity to especially good effect in his discussion of the views held by P. A. Latreille and A.-P. de Candolle on the geographical distribution of species. It is interesting to note, however, that on the central question of why there was an English Darwin rather than a French one, Limoges follows the by now standard interpretation put forward most forcefully by W. F. Cannon and Charles Gillispie that it was the uniquely English version of natural theology that fostered Darwinian thought.

The second recommendation for Limoges's book is that it is the first work to be published on the formative years of Darwin's development since Darwin's notebooks on transmutation were published by the British Museum.

From these notebooks Limoges has drawn several important conclusions as to the order of Darwin's insights. First, contrary to his word, Darwin was not looking for an analogue to artificial selection when he read T. R. Malthus's *An Essay on the Principle of Population* in late September 1838. Second, the reading of Malthus was in itself the catalyst of his theory. Both points are correct, though I find Limoges's interpretation of them more dogmatic than present knowledge of the early Darwin manuscripts allows. On occasion Limoges also blurs the distinction between the historical development of the components of the idea of natural selection and the route Darwin himself took. For example, he discusses the work of Edward Blyth on points where Darwin very clearly learned much more from Charles Lyell (pp. 66-68). Limoges has nonetheless reconstructed a significant portion of Darwin's early thought in such a way that it illuminates the change involved in the passage from a traditional notion of natural economy to one involving transmutation.

SANDRA HERBERT

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KARL MARX. *Critique of Hegel's 'Philosophy of Right.'* Translated from the German by ANNETTE JOLIN and JOSEPH O'MALLEY. Edited with an introduction and notes by JOSEPH O'MALLEY. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1970. Pp. lxxvii, 151. \$9.50.

The relationship between the young Marx and the mature Marx is one of the major controversies among contemporary Marx scholars; and the position taken in this controversy determines one's attitudes toward various works of Marx. If his life's entire work is seen to be of one piece, then the early books, articles, and fragments in which he formulated his disagreements with Hegel and the Hegelian Left assume central importance.

Professor O'Malley, who takes this view, has made available to the English-language reader the 150-page manuscript in which Marx criticizes Hegel's political philosophy. Although there are minor errors in the introduction the translation itself is careful and faultless. Still the manuscript presupposes so intimate an acquaintance with Hegelian, Feuerbachian, and

Marxian thought that it would not be easily understood even by many scholarly readers.

For this reason the most important part of this book is the brilliant and learned introduction, in which Professor O'Malley carefully explains the circumstances in which the manuscript was written and places it within the course of Marx's intellectual development. He then describes the methodology of his critique of Hegel, dissecting it carefully and convincingly into three distinct devices; and finally he guides the reader painstakingly through every single step.

The argument is convincing. The total work of Marx appears to have coherence and unity. And yet the nagging feeling remains that the opposite case could be made. One regrets that Professor O'Malley argued his case without taking up the points made by those who make a strict separation between the young Marx and the mature Marx. One key to this separation is the contribution to "Marxism" made by Friedrich Engels. O'Malley is perfectly justified in hardly mentioning him; for, after all, when the manuscript on Hegel was being written, Marx and Engels had not yet met. Yet for an understanding of the total work a discussion of their relation to each other's ideas would have been pertinent.

This reservation apart, the work being reviewed is a fine contribution to scholarship.

ALFRED G. MEYER

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HEINZ K. MEIER. *Friendship under Stress: U.S.-Swiss Relations 1900-1950.* Bern: Herbert Lang & Co. 1970. Pp. 423. \$10.60.

Professor Meier has given us a well-researched study of American-Swiss relations in the first half of the twentieth century. And his definition of "relations" is commendably broad. In addition to the strictly intergovernmental business Meier's study covers major aspects of public opinion, cultural affairs, and other people-to-people involvements (particularly tourism), with the result that the book produces a picture more colorful and meaningful than the one that generally emerges from more conventional studies in diplomatic history. There

is warmth in the book, too, and this comes very definitely from the fact that Heinz Meier was born Swiss but lives in America, and he therefore "relates" personally to both of his subjects. Their friendships (quiet but durable) are his pleasure; their arguments (occasional but sharp) obviously distress him. It is refreshing to read history that has been lived.

While much of the book deals with routine bilateral affairs, the chapters involving Swiss neutrality raise questions of great significance. Although Meier is objective enough to see to it that the contentions of both sides receive a full hearing, it is nevertheless pervasively true that he is a great believer in Swiss neutrality, with all that has meant in terms of trading with the enemy. The fact that the Swiss arguments during World War II struck most American officials as particularly exasperating serves as an interesting reminder of how far the United States had moved since its own neutrality days of 1914-16. Also significant is the fact that Switzerland's neutrality problems carried on into the postwar world and resulted in Swiss abstention from the United Nations. Meier tries valiantly to reconcile these divergent philosophies by asserting that "neutrality and collective security have the same goals, namely the maintenance of peace and the status quo and the prevention of revolutionary change." Somehow one gets the impression that he is reluctant to face the fact that it took two ferocious global wars to make the world safe for neutrality—even temporarily.

The chapter of most general interest is probably the one with the unwieldy title "Wilson, Rappard, the Paris Peace Conference, and the League of Nations." Although much of this ground was covered years ago in the writings of that remarkably able and distinguished Professor Rappard, it is good to have the whole story brought together, as Meier has done, from all pertinent sources.

The least satisfactory chapter is the one dealing with Switzerland's activities as a protecting power in World War II. The author is aware of this shortcoming and indicates, by way of extenuation, that he could not possibly cover this huge story in one chapter. This is true, but then his study is not so large that he could not have devoted more space to a sub-

ject so significant and so little studied. Now that the American, British, Canadian, and German documentation is open for the war period, perhaps the Swiss will follow suit, and Professor Meier can give us a fuller treatment of this neglected topic.

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BRITON COOPER BUSCH. *Britain, India, and the Arabs, 1914-1921*. Berkeley and Los Angeles: University of California Press. 1971. Pp. ix, 522. \$14.50.

Amidst the complexities of the period of World War I, the question of British policy in the Middle East during those years is surely one of the most difficult to unravel. Scholars have tended to skip over this area with a few well-worn generalities, none of them major distortions, but none sufficiently examining or explaining the reasons behind British actions. It is only in recent years that the immense body of documentation necessary for such a study has become available, and in this book Professor Busch has given us the first full-scale attempt to present a detailed picture of the evolution of British policy. He has consulted not only British governmental records in England but in India as well, and he has also used a number of collections of private papers that are of extreme importance.

Professor Busch has restricted his examination to the conflict between British authorities in London and in India. It was a peculiarity of Britain's governmental structure that the Middle East fell into two separate spheres of control, the western half under the Foreign Office in London and the eastern half under the Government of India. Busch argues that Indian intervention and influence hampered the creation of a unified British policy toward the Middle East, and he demonstrates his thesis by illustrating the conflict between India and London at a number of crucial points during the war.

On the outbreak of war, the Government of India dispatched an expeditionary force to Mesopotamia, which lay within its area of responsibility. Almost immediately Indian political officials in Mesopotamia came into conflict with their opposite numbers in Cairo who held an altogether different view of what Brit-

ish policy should be. In London the views of Cairo and Mesopotamia were expressed respectively by the Foreign Office and the India Office (which served as the spokesman for the Government of India); thus local opinion was transferred to a much broader scale. Professor Busch has wisely not devoted a great deal of space to the well-known aspects of the question such as the Husain-McMahon correspondence or the Sykes-Picot Agreement, but has concentrated instead on the day-to-day developments in Cairo and Mesopotamia and the increasing divergence in their views. These views emerge clearly with Cairo supporting Arab nationalism and independence (but not necessarily, as Busch notes, unity), and the Indian officials opposing this policy and preferring to rely on traditional methods taken from their experience in India. The play of personalities was of major importance since decisions were often made by those on the spot, and Busch has provided excellent vignettes of British officialdom.

One's only regret is that, in emphasizing the British side, Professor Busch has given little attention to Arab questions: the nature and content of Arab nationalism or Arab objectives in terms of independence and unity, for example. These problems are perhaps outside the scope of the present work, but they might have provided a useful counterpoint. Nevertheless Professor Busch has given us an extremely valuable study, well written and researched with admirable thoroughness. His book will go far toward illuminating a hitherto dark corner of Middle Eastern history.

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NICHOLAS BALABKINS. *West German Reparations to Israel*. New Brunswick: Rutgers University Press. 1971. Pp. 384. \$12.50.

The Luxemburg Treaty by which the Federal German Republic paid 3 billion DM to Israel and 450 million DM for the rehabilitation of Jews throughout the world is an accord without precedent. Massacres and genocide took place in the past (if not on such a vast scale), but no compensation has ever been paid to the survivors by a government that was not it-

self a party to such brutality. Mr. Balabkins tells his story well: he analyzes not only the painful negotiations themselves, but also the economic background against which they took place and the effect that the payments had on the German and Israeli economies. At the time of the treaty in 1952 the Federal Republic had not yet experienced its economic revival, and German financiers feared that additional foreign commitments might make it impossible to pay outstanding obligations and restore German credit abroad. German resistance to the negotiations was based upon such fears. Israel, on the other hand, was in need of the money in order to solve a deepening economic crisis due largely to the rapid absorption of some 700,000 immigrants. Nominally, at least, the payment to Israel was supposed to help defray the expenses of their settlement: eventually, the expenditure had little effect on Germany itself, for it only constituted seven per cent of the billions paid out for the lost war. Israel, however, was able to build an economic infrastructure, though military considerations and some ill-considered planning produced considerable economic waste. It is a pity that Mr. Balabkins excludes the effect of individual restitution payments from his analysis, for they seem to be a part of the general economic picture.

The book is less convincing when it assesses the continuing popular reaction to the treaty in Germany and Israel. He finds hostility or indifference, but this is based on rather narrow evidence, mostly selections from the public debates on the issue. His introduction, which attempts to give the history of German anti-Semitism and the holocaust, suffers from compression. At the present stage of research these issues cannot be profitably discussed in some twenty pages without undue dogmatism and some factual errors.

Konrad Adenauer's predominant role in the conclusion of the treaty is rightly stressed, but the praise bestowed upon him seems more deserved in the subsequent generous interpretation of the treaty at a time when, thanks to the cold war, the moral rehabilitation of the Federal Republic was no longer a political issue. Ben Gurion's realism is given its due, while one of the most important motivations for his actions is omitted. Through lead-



ing the negotiations from the start Israel could validate its claim to represent all Jews throughout the world. The book assumes the correctness of a claim that was much disputed at the time. The Germans also accepted it, to the point of showing reluctance in compensating Jews living outside Israel. Here, for reasons that remain to be explained, the German government made nearly as great a contribution to Israel's status in the world as in the treaty itself.

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### ANCIENT

SINCLAIR HOOD. *The Minoans: The Story of Bronze Age Crete*. (Ancient Peoples and Places, Volume 75.) New York: Praeger Publishers. 1971. Pp. 239. \$9.50.

This excellent book expresses compactly and in an extremely readable manner the growth, maturity, and final collapse of the Minoan civilization of Crete, that "first high civilization on European soil" (p. 11). Although the text of this "popular" book is relatively short (140 pages) the author, with an ability shown earlier in his *Home of the Heroes: The Aegean Before the Greeks* (1967), produces a rich, detailed impression of the somewhat elusive character of the Minoans through a study of their material remains and environment. The variety of in-text line drawings (numbering 131), many made expressly for the book, and the 120 clear and attractively arranged plates in the back of the book display a good cross-section of Minoan architecture, sculpture, wall painting, pottery, and seals. The captions to the plates and figures (see pp. 217-32 for the former) are especially informative, and the notes (pp. 151-63) will prove useful to students and scholars.

The author is the former director of the British School of Archaeology in Athens and is the excavator at Knossos who recently verified in large part the much debated sequence of pottery styles established previously by Sir Arthur Evans. His approach to writing combines a praiseworthy tendency to stay as close as he can to archeological fact with an intimate knowledge of recent archeological dis-

coveries. For instance, worthy of note is his analysis of the effect that the volcanic eruption of Thera may have had on Minoan Crete, an analysis partly based on what he learned at the Thera Conference (p. 55). He proposes that the effects of a Late Minoan (LM) IA destruction, caused by the eruption, can actually be found on Crete and that the later destruction in LM IB was caused by foreign invaders, the latter a theory originally proposed by early excavators. We can place this in direct contrast to the theory of a second eruption in 1450 B.C. (the end of LM IB), most recently reiterated by Nicolas Platon, the excavator of Kato Zakros (*Zakros* [1971], pp. 265-320). As additional bonuses in his book Hood includes descriptions of Early Minoan II Fournou Korifi (p. 61), excavated by Peter Warren, and the palace of Kato Zakros, the latter being given the courtesy of being considered before the other palaces (p. 65). There are also up-to-date discussions of the evolution of Minoan pottery styles (pp. 35-48), of objects of foreign origin found in Minoan contexts (pp. 123-26), as well as a clear restatement of his reasons for remaining skeptical about the generally accepted decipherment of Linear B (pp. 113-15).

For those wishing a congenial, scholarly text that compresses into a short space what we know about Minoan Crete, this book deserves the highest recommendation. In this respect it supersedes R. W. Hutchinson's *Prehistoric Crete* (1962). The editors of Thames and Hudson should be congratulated on the format, the attractive and well-placed illustrations, and a generally flawless job of printing.

JOSEPH W. SHAW  
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A. W. H. ADKINS. *From the Many to the One: A Study of Personality and Views of Human Nature in the Context of Ancient Greek Society, Values, and Beliefs*. (The Society for the Humanities, Cornell University. Studies in the Humanities.) Ithaca: Cornell University Press. 1970. Pp. xv, 311. \$8.50.

To a modern age earnestly asking what is the nature of man and his good life Adkins's subtitle promises much. But this is a book for the specialist, a close concentration on the evolving semantic values of certain Greek words (for example, *phusis*, *psuche*, *agatha*, *arete*, and *eudai-*

monia) that indicated men's values, roles, and beliefs.

Homeric heroes, responsible for small *oikoi*, lived in a "results culture" that judged men's worth by lineage, wealth, and ability to protect their own. Failure in these external responsibilities shamed a man, and no good intentions or personally held moral standards restored his honor. Such external sanctions over which he had no judgment or control caused a disunited personality. Later developments perpetuated this disunity. Hesiod's tale of creation brought some core of personality to men but again stressed a class society and an afterlife brightened only by this world's fame. The emerging *polis* was small enough that each individual's birth and political and military successes were known. Religion only precariously balanced the limitations of human nature and the demands of success. In philosophy the Sophists stressed competitive excellences even against laws and customs. Plato lauded the virtues primarily as the monopoly of the few who controlled and conditioned the passions of lesser men. Aristotle judged that the war between men's desires and intellect could be won only by training the ablest men to desire what was good. Surprisingly Adkins finds integrated personality for the first time in the Hellenistic age. The *polis* had imposed psychological stresses of competition; the larger Hellenistic states, by lessening individual responsibility and prestige, permitted men of all classes to find inner harmony, self-sufficiency, and satisfactions—be they only Stoic *apatheia* and Epicurean *ataraxia*.

This book is a sequel to Adkins's earlier *Merit and Responsibility* (1960), which uses the same word analyses to trace historical development of other concepts. Both books are part of a valuable, growing literature analyzing the meaning and significance of Greek concepts as different from our own (for instance E. R. Dodds, *The Greeks and the Irrational* [1951]; Bruno Snell, *The Discovery of the Mind* [1953]; and Helen North, *Sophrosyne* [1966]).

Weaknesses, however, beset Adkins's book. The stress on only certain words throws much of the period out of proportion; the length of time covered and the variety of approaches used make for superficiality. Sometimes the theme distorts the facts; Aristotle, for example,

says that the state exists to aid the good life of the citizen, not the reverse. Indeed Adkins's central theme remains unproven: that man suffers personality fragmentations when seeking successes for external approval, but that the many become the one when man retreats to his own internal solace.

ELEANOR G. HUZAR

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CHARLES W. FORNARA. *Herodotus: An Interpretative Essay*. New York: Oxford University Press. 1971. Pp. 98. \$5.75.

Fornara's purpose is to show how Herodotus wrote his *Histories*, how he developed intellectually, how the book finally reached its present form. Chapter 1, "Unitarians, Separatists, and Book II," refers to the views of his predecessors, especially Jacoby (Herodotus passed from a series of independent studies to a Persian history to the history of the Persian War), and states his own: that Herodotus began with an ethnographical interest, in book 2 ("written to instruct and entertain"), and, adjusting his intentions as he matured and so constantly revising, gradually became the historian whom we honor. This conception, which I find sensible, is not far from Jacoby's, although less rigid in that it refuses to divide the *Histories* into easily separable parts.

Fornara then illustrates his announced thesis. In chapter 2, "The Persian Empire," Herodotus's ethnographical concern becomes a more sophisticated interest in Persian expansion, with emphasis on motive. Chapter 3, "Herodotus and Athens," brings us to 431, when Athens and Sparta were on the verge of war; Herodotus wrote books 7-9 as he did in response to this tragic crisis. Chapter 4, "Herodotus' Perspective," contrasts the eternal relevance of Thucydides' work with the more restricted public envisioned by Herodotus, who "directed himself exclusively to his own generation" (dated, too closely, 431 B.C.). "Herodotus considered the events of his own time an unmitigated but thoroughly unavoidable disaster. It was the truth of this proposition which his *Histories* intended to convey" (chapter 5, "The Archidamian War"). The epilogue sums up: "Herodotus developed as he wrote and shifted his intentions as he developed." A simple "Index of Names and Subjects" and an "Index

of More Important Passages" complete the book.

This study (my summary is cruelly brief) is worth reading, and Fornara makes a good case. He has read the literature, although he keeps documentation to a minimum. He is at his weakest in his references to the Athenian empire, for he suffers from the contemporary uncritical attitude to imperialism. The editing and proofreading are not perfect, and he indulges in some unfortunate usage, but I am happy to see the correct form of the adjective in the title.

MALCOLM F. MCGREGOR

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JOHN TRAVLOS. *Pictorial Dictionary of Ancient Athens*. (German Archaeological Institute.) New York: Praeger Publishers. 1971. Pp. xvi, 590. \$70.00.

John Travlos has published a superb pictorial guide to ancient Athens consistent in quality and appearance with its distinguished predecessor on Rome by Ernest Nash. The scholarship is impeccable; the photographs and drawings sharply made, beautifully printed, and aptly chosen; and the intelligent entries crisply written and thoroughly up to date. Without question, Travlos has produced a masterful reference work on the topography and monuments of the classical city that will stimulate and inform every historian, classicist, and archeologist concerned with Athens and Greek civilization.

The encomiastic tone of this review applies equally to the individual entries, eighty in number and arranged alphabetically, through which the author has surveyed the active topography of Athens from 3000 B.C. to the late Roman Empire. Every element of textual, literary, historical, and physical evidence has been used comprehensively in the presentation of all major sites (for example, Agora, "Ilissos Area," or Panathenaic Way), monuments (for instance, of the Eponymous Heroes or Lysikrates), buildings (such as Erechtheion or the Library of Hadrian), and even private houses. Each entry begins with a textual introduction and descriptive analysis that sums up the present state of knowledge and frequently contributes to its advancement, since Travlos has participated in so many important excavations and programs of archeological restoration over

the years. The entry is illustrated brilliantly to reveal what survives and to suggest through reconstruction what once existed, often established in context by the incorporation of related sculptures, inscriptions, or other ancillary materials. Each entry then concludes with a complete bibliography of the greatest breadth, usefulness, and precision. Because the entries are so comprehensive and the treatment so perceptive, Travlos's book is more than a categorical compilation of data about Athens, as the author reaches toward the systematic description of the life of the city as revealed in its discrete, constituent elements. The reader who is interested by the author's brief account of the urban history of Athens (pp. 158 ff.) and impressed by the scope of his knowledge and by the sensitivity of his judgments, will be grateful for Travlos's outstanding achievement but asks him now to give us a modern history of the ancient city. The auspices are very favorable.

RICHARD BRILLIANT

*Columbia University*

W. ROBERT CONNOR. *The New Politicians of Fifth-Century Athens*. Princeton: Princeton University Press. 1971. Pp. xii, 218. \$8.50.

In the fifth century B.C. a conservative Athenian leader drew his political strength from many small groups of friends. Pericles, on the contrary, put himself deliberately at a distance from such friends in order to enhance his strength with the undifferentiated Athenian mass. Cleon went a step further for the same reason. He was not really a lower-class type, as Aristophanes makes him seem, but a rich and clever man who, while he could have taken—had he wanted—a place in the world of clubs and powerful, special friends, instead explicitly repudiated the so-called best people and identified himself with the "base" (*poneroi*). Pericles and Cleon were innovators, but the rise of rhetoric also had a part in these changes, as it did in a decline in the power of the generalship and a lowering of the age at which men became political leaders. Friendship, however, continued to be a principal way of getting the city's business done, in a city whose political life was ubiquitously and vigorously polycentric.

This rough summary of a lively and gener-

ally convincing book omits notice of a number of fine observations that enrich it throughout. One matter, however, needs comment. There is an evocation of a successful conservative politician's career, repeated at least once in the course of the book, in which the man is made to seem to float gracefully to power and eminence on washes of luck, good family, and easy benefactions. He is withal prudent. His "heart is captivated by a woman who happens to belong to a wealthy and influential family," and he shows himself "affable, sensible, reliable" at clubby meetings of the right people. But this amiable fantasy ignores certain assumptions that should underlie speculation about Athenian political life. Men were exiled and murdered, both outside the legal system and within, for being on the wrong side. A politically ambitious benefactor had dependent friends and supporters sitting near him at meetings of the ekklesia, and they made proper noises at useful times. Successful politicians have perfect memories for favors given and received and a clear understanding of how to enforce these obligations. One might consequently have felt in a book about Athenian politics more of a sense of their cold-eyed assessments of resources and their literally murderous competitiveness. There is no question, however, that Professor Connor has made a good contribution to our understanding of ways in which power could be acquired at Athens during a time of important change.

ALAN L. BOEGEHOLD  
Brown University

F. E. PETERS. *The Harvest of Hellenism: A History of the Near East from Alexander the Great to the Triumph of Christianity*. New York: Simon and Schuster. 1970. Pp. 800. \$15.00.

The book jacket of *The Harvest of Hellenism* calls it "historical writing at its most ambitious." The claim (with usual allowance for the beloved American superlative) is well justified. It is certainly a most ambitious synthesis. In a single volume the author, who is professor of classics and Near Eastern studies at New York University, has attempted to deal with the whole range of cultural and historical phenomena of "Eastern Hellenism" from the death of Alexander until the late fourth century A.D.

His purview includes not only the Near East strictly speaking but also pertinent developments elsewhere in the Mediterranean area and in adjacent territories as far as India and Turkestan; not only the immediate phenomena but also their background; not only political, economic, and military events but also philosophy, religion, art, science, and literature. One is reminded that the book covers much of the ground of the last seven volumes of the *Cambridge Ancient History* (from which its ten line maps are borrowed). Even the mode of presentation is similar in that chapter headings, like the volume titles of the *History*, indicate only the leading aspect of the contents: thus "Hellenes and Jews" covers, by a natural extension, the Seleucid successor states in the East and, as a kind of digression, the history of Greek-Indian connections down to the Kushan Empire (whose history and character are described in a later chapter entitled "Classic and Baroque"). In the whole work there is not a single exact reference to textual authority, the occasional footnotes giving added bits of information or more frequently cross-reference to another section of the book. An "Informal Guide to Some Further Reading" describes briefly some books and essays grouped by topics, with the odd omissions characteristic of such surveys—von Arnim for the Stoics, for example, but not Usener for Epicureanism, and no up-to-date guide like Lesky's for Greek literature. An eighteen-page chronology, especially useful for political figures and events, is in some respects equally idiosyncratic (for instance, dates of Varro, Luccretius, the poet Gallus but no mention of Virgil, Horace, Ovid, nor the vital dates of Cicero).

Despite all the pitfalls and the criticisms to which so personal, wide-ranging, and undocumented a work is naturally exposed, it is an extraordinarily successful venture—full in its factual coverage, readable in its style, and sane and accurate in most of its interpretations, which range from such matters as the nature of the Aetolian League or literary quarrels in Alexandria to explanations of the class system and its strains in the Empire, Plotinus and his philosophy, the history of Mithraism, and the origins of Christian monasticism. In a confident and continuous narrative of this sort

there are many choices to be made amidst conflicting evidence. Sometimes one would wish for less assurance, as in dating Zoroaster so exactly ("c. B.C. 628-551"), in making Plutarch simply monotheistic, in accepting Cicero's view of Nigidius Figulus as the originator of a revived Pythagoreanism, and in following exclusively Eusebius's account of the events of A.D. 310-13. More often, however, the ambiguities in our present knowledge are met by sensible weighing of the probabilities, as for the life and background of Mani (where the new papyrus evidence was not yet available to the author), or by discreet silence, as for the chronology of the life of Christ. Sometimes wrong choices are made, as in following the *Historia Augusta* and placing figures of Abraham and Jesus in Severus Alexander's *lararium*, in making transcendent *eide* the essential feature of Plato's philosophy, and in following a somewhat antiquated view of Posidonius. Outright blunders, such as saying that Ashoka's edicts were written in Aramaic, are remarkably rare; and there are few mechanical errors like the misprints and inconsistency of dates that dog the account of Apollonius of Perga.

For some people surely the two greatest weaknesses of the book will be reflections of its greatest strengths. First, the extraordinarily wide coverage leads to unusual compression of statement. Sometimes the result is a confusing welter, as in the pages (154-65) given to the varying fortunes of Ptolemaic and Seleucid rivalry; sometimes the exposition jumps about so much in time and place that it appears confused as well as confusing, as in the three pages (526-28) on the classes in the Empire. When one considers that the essentials of Gnosticism, for example, are described quite clearly in a single page and the history of the Greek romance in a page-and-a-half digression in a discussion of Lucian, the usual success is far more remarkable than the occasional failure. Second, the lively style may well offend those whom it does not attract. It is self-consciously "mod"—one chapter is called "Life Styles and Life Sciences"—as well as sophisticated and ironic. At worst it may appear merely puckish, as in the footnote describing Antinoöpolis as "named for the Bithynian youth after whom Hadrian sighed, in the Greek style, for some

years," or in the phrase describing enforced converts to Judaism as "those recently converted at and by the end of a Hasmonaean sword." Vividness and a distinctive tone give the work life and movement, but occasionally at a price.

All in all this book is a fascinating combination of a smooth-flowing narrative and a concise encyclopedia written with concern and judgment. An expert will feel that each page could stand some qualifications or addenda, but he should welcome the high standard of presentation; a layman may well find that too much is taken for granted and feel at sea in places, but he will recognize the intellectual integrity and breadth. One might make the single general criticism that the book dwells too exclusively in the world of intellectual and literary men. Amidst the talk of rulers and philosophers and schools there is almost no use of the materials showing what common men and women felt and thought, what participation in Greek and Roman religion really involved, for example, or how men faced death. In its emphases it reflects strongly the previous work and interests of its author. But where so much has been included in a book that can be warmly recommended for its soundness and readability, it is appropriate rather to congratulate the author for his achievement and the publishers for a very well-produced volume.

ZEPH STEWART

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GEORGES DUMÉZIL. *Archaic Roman Religion*. With an appendix on the religion of the Etruscans. In two volumes. Translated by PHILIP KRAPP. Foreword by MIRCEA ELIADE. Chicago: University of Chicago Press. 1970. Pp. xxx, 394; vi, 397-715. \$25.00 the set.

This is a translation of the author's one-volume work, *La religion romain archaïque, avec un appendice sur la religion des Étrusques* (1966). After some forty years of research on the origins of Roman religion by the comparative method, Georges Dumézil treats Rome and its religion "in themselves, for themselves, as a whole." "Without surrendering the advantages of the comparative method," he here adds "the other traditional ways of knowing."

The principal thesis of Dumézil's long and

distinguished scholarly labors is that early Roman religious ritual and political history are the result of the transference to the human plane of the mythology common to the peoples of the Indo-European language groups, notably the Sanskrit, Vedic, Germanic, and Celtic. He rejects the primitivist *mana-numen* theory of Herbert J. Rose *et al.* and the "pontifical revolution" of Kurt Latte. The archaic triad, Jupiter-Mars-Quirinus, and their counterparts in the three Grabouic deities among the Umbrians at Iguvium find their correspondents in the Vedic Mitra-Varuna-Indra-Nasatya and Scandinavian Odinn-Thorr-Freyr triads. This religious structure divides the domain of life into the functional spheres of sovereignty, physical power, fertility, and prosperity. The origin and significance of the Capitoline Triad, Jupiter-Juno-Minerva "constitute a problem for which, in the present state of our knowledge, it is not possible to provide a final solution" (p. 291).

The basic thesis is outlined in part 1, the preliminary remarks. Part 2 deals with ancient theology and closes volume 1. In volume 2, part 3 discusses the extensions and mutations experienced by Roman religion, notably under the influence of foreign religions. Part 4 on the cults, *sacra publica et privata*, is followed by an appendix, "The Religion of the Etruscans," which has "enriched the structure of Roman religion without dislocating it."

For the historian the principal question that Dumézil's work poses is whether the accounts of the most ancient period are *exclusively* Indo-European myths transformed into history. Accepting the parallelisms—the war of Romulus and Tatius and the conflict of Aesir and Vanir in Scandinavian lore (pp. 66–75), Romulus and Remus and the divine twins of Indian mythology (pp. 252–55), and many others—is it possible to distinguish persons and events that have a certain historical value, as Mommsen does, in the legends that for him are projections into the past of names and events of early history? Does Dumézil leave any place for the historian in the earliest period of Rome when he writes (p. 252) that "the accounts of the earliest period of Rome are *in large part* humanized and historicized mythology"? (*italics mine*).

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JOACHIM SZIDAT. *Caesars diplomatische Tätigkeit im Gallischen Krieg*. (Historia: Einzelschriften Number 14.) Wiesbaden: Franz Steiner Verlag GmbH. 1970. Pp. 162. DM 28.

This book studies a neglected aspect of Caesar's Gallic campaigns, namely his diplomatic contacts with the various tribes and the manner in which these are presented to the reader. Only at the beginning and the end is the wider question of the trustworthiness of Caesar's narrative briefly raised, and here, after presenting a useful summary of views, the author steers a middle course, admitting the likelihood of bias for political ends, but stressing the extent to which actual misrepresentation would be limited by the existence of reports to the senate and those of numerous eyewitnesses.

Szidat systematically describes and analyzes all Caesar's accounts of diplomatic activity. The space allotted to dealings with the Helvetii, the Aedui, and Ariovistus reflects the emphasis Caesar himself gives to his first year in Gaul. Having presented his early diplomacy in extended form to set the tone, Caesar could thereafter be briefer: more important, it was crucial to represent the dubious actions of 58 B.C. in a favorable light. The arguments Caesar gives to Gauls and Germans are not always the same as he gives his readers, but Szidat shows that he was at pains to follow correct protocol, especially with Ariovistus, who, as friend and ally of the Roman people, presented a tricky problem: only when the latter had shown himself an arrogant barbarian who was impossible to deal with diplomatically did Caesar resort to armed force. The rest of Caesar's diplomacy broadly follows the lines initially laid down, and he is careful to show that the feckless legate Sabinus, victim of Ambiorix's ruses, failed to follow the proper practices in parleying with the foe.

Caesar's choice of material and his manner of literary representation are considered next, and the various typical arguments enumerated, the commonest being the duty of Rome to her allies (as interpreted by Caesar). One interesting general feature of Caesar's diplomacy is his close observance of traditional Roman customs, though never to the point of inflexibility: while emulating the eastern conquests of Pompey, Caesar wished to appear more truly in the Roman mold than his rival.

This useful piece of writing is of interest to students both of ancient diplomacy and of Caesar. Its main flaw is the lack of a wider, more imaginative approach: one is left with the suspicion that Caesar has once again outwitted his commentators.

E. M. WIGHTMAN  
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A. H. M. JONES. *Augustus*. (Ancient Culture and Society.) New York: W. W. Norton and Company. 1970. Pp. xi, 196. \$6.00.

This is the fifth in the Ancient Culture and Society series. It is an excellent study for anyone wishing to learn about current scholarly opinion concerning Augustus and his age. The powers of the author in synthesizing vast amounts of information into a small book are positively Thucydidean.

After a very full and satisfactory survey of Augustus's life there are special chapters on the Augustan constitution, magistrates, senate, provinces, armed forces, finance, justice, social and religious policies, and literature and art. In an important final chapter the author carefully analyzes the ancient sources and concludes that the best full-length ancient account of Augustus's reign is found in Cassius Dio's *History of Rome*.

The work abounds in revealing insights. We learn that Augustus's power rested not on the nobles but on the lesser senators and the upper stratum of Equestrians. Velleius is cited to show that Egyptian revenues after Actium went directly into the Aerarium; thus Egypt was not a part of Augustus's household as some have inferred from remarks of Tacitus. More startling perhaps is the author's insistence that the standards of Augustus's provincial administration probably did not improve much on those of the Republic and that businessmen and financiers were always a minority in the Equestrian order: later under Augustus the *equestres* became a class of landed gentry.

Clearly Jones suggests a more favorable view of Augustus than did Syme in his *Roman Revolution* of 1939. But Jones uses the numismatic evidence very sparingly and does not seem to attach much significance to Augustan propaganda and the "selling of the principate." Despite the great Augustan achievement outlined here one is still suspicious that the price paid

in freedom for the Augustan settlement by the Roman educated classes was much too high for Rome's future.

JAMES E. SEAVER  
University of Kansas

SIR RONALD SYME. *Emperors and Biography: Studies in the Historia Augusta*. New York: Oxford University Press. 1971. Pp. vii, 306. \$10.50.

With this volume, and with its recent predecessor, *Ammianus and the Historia Augusta* (1968), Sir Ronald Syme has made himself in just three years the leading authority in a controversy now more than eighty years old. No one but Ernst Hohl, the longtime proprietor of the *Historia Augusta* (HA) debate, has done so much to expose the personality of these round-heeled, winking biographies of second- and third-century emperors. Syme writes not to orient the uninitiated or the neutral, but as a partisan promoting controversy. Yet these two books are the most comprehensive guide to the HA that now exists. He has pronounced himself on virtually every problem—authorship, date, purpose, sources, and authenticity—and in so doing he has expertly arraigned and shaken out the several bibliographies that cluster around the text. At the same time he has investigated a number of more narrowly defined topics to which he is the first or largest contributor.

The eighteen essays of *Emperors and Biography* lead in many directions. Some continue and enlarge upon themes of the earlier book. Syme urges that researchers take a fresh start by recognizing the HA as a production unified by characteristic usages and structure. And with the admiration of one professional for another's knowing satirization of their discipline, he again calls attention to the perverse accomplishments of the writer responsible for the HA. In several chapters he attempts to deduce the character of certain sources employed in the earlier lives, the lost Marius Maximus and two postulated works, one a series of biographies, the other an extensive epitome; his exposition entails a skillful dissection of the biographies of Hadrian, Pius, Verus, and Macrinus. Seven of these studies are spent in an effort to transcend the suave harlotations of the HA. Attending to the heuristic categories that

underlie all his work—strategic roads and lands, the origins and careers of persons, marital alliances and factions—Syme recovers portions, though miserably small portions, of the true history of the emperors from Severus Alexander to Diocletian. There are still other essays that escape this enumeration. Among them are two of the best in the book, an investigation of the patterns by which personal names have been faked and an exposé of the procedures by which the biographer concocted the first five lives of pretenders and heirs apparent.

The book is not so elegantly structured as Syme's earliest works. The essays overlap, though to some extent necessarily; it is a more serious objection that in some cases the component parts of an ideally coherent argument emerge in different connections through the book. These disadvantages, together with the restriction of the index to an *index nominum* and the choice of chapter titles that do little to disclose the breadth of topics treated in each study, make a valuable series of explorations less accessible than they should be.

PETER WHITE

University of Chicago

JOHN FERGUSON. *The Religions of the Roman Empire*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1970. Pp. 296. \$8.50.

No proper understanding of life in the Roman Empire is really possible without some knowledge of the bewildering number of religions and religious philosophies that dominated the Roman scene from the first to the fourth centuries A.D. John Ferguson, whose decade of experience in Nigeria provided him with remarkable first-hand experience not only in exploration of the Roman Empire but also in comparative study of African religions with Roman religions, was motivated to write a new book that would supplement and update T. R. Glover's *The Conflict of Religions in the Early Roman Empire* (1920). Ferguson has gone beyond the literary evidence, which was Glover's principal focus, and made extensive use of the archeological evidence of the last half century in order to show how these various religions arose, conflicted, and sometimes fused with the traditional religions of Greece, Rome, and the northern countries and also how they inter-

acted with Judaism and Christianity. Particularly noteworthy are examples generously drawn from British classical archeology to provide support for some of Professor Ferguson's observations.

The concentration of *The Religions of the Roman Empire* is on the third century, but the author does not limit himself to this century chronologically. He sweeps widely over a great deal of ground that the historian of religions would probably wish to have examined more slowly and more generously. Such topics as the Great Mother, the Sky Father, the Sun-God, divine functionaries, Tyche, the sacred figure of the emperor, personal religion, the afterlife, divination of the future, shamans and shams, philosophers and the gods, and syncretism and confrontation are brought before the reader with dazzling rapidity and forceful erudition and are backed up by a formidable chapter-by-chapter bibliography and notes and appropriately illustrated from the archeological evidence. We do not get in this book a theologically or religiously profound, synthetic view of the great variety of religions that permeated the Roman world but rather a kind of historical catalog or encyclopedic exposure to a great number of religious, semireligious, and even pseudoreligious phenomena that were important and are quite interesting. Almost any section of this book could be read independently and the astute student will perhaps wish the book had some kind of unity. The style is direct enough but this is hardly the kind of book that makes for smooth, uninterrupted reading.

Still, even though *The Religions of the Roman Empire* is not comparable to the work of an A. D. Nock, a Martin P. Nilsson, or an A. J. Festugière, it is competent, necessary, and welcome. Students of ancient history should especially consult it.

JOHN E. REXINE

Colgate University

JAROSLAV PELIKAN. *The Christian Tradition: A History of the Development of Doctrine*. Volume 1, *The Emergence of the Catholic Tradition (100-600)*. Chicago: University of Chicago Press. 1971. Pp. xxiii, 394. \$15.00.

This is the first of a projected five volumes that will present a history of the development of Christian doctrine from about A.D. 100 to the



twentieth century. The author, who is professor of religious studies at Yale University, has undertaken the task with the special intention of providing a comprehensive history of the development of "what the Christian church believed, taught, and confessed." The analysis of the theological factors forms the sole purpose of the study, in contrast to other methods of exposition in which nontheological influences, such as political history, may tend to distract attention from the evolution of essential doctrine. The work is based on the primary sources, cited here in translation, and is addressed both to students of theology and church history and to students of intellectual history who are concerned with the influential ideas contained in Christian doctrine. Two of the author's preceding works, *Development of Christian Doctrine: Some Historical Prolegomena* (1969) and *Historical Theology: Continuity and Change in Christian Doctrine* (1971), provide the historical background for the present study and show the methodological assumptions on which the work is based (p. 361).

This volume will be of value to historians who are concerned to understand the doctrinal results of the activity of the church in the Greco-Roman world; the succeeding volumes may be expected to render like service. The writer's learning and his gift of clear exposition are evident on every page. Beginning with an account of the religious and intellectual background of the period of the origin of the church, Professor Pelikan treats systematically its sources of faith, the mystery of the Trinity, the person of the God-man, and nature and grace; and he closes with a summary of the achievement in the sixth century of what he terms "an orthodox consensus about what was to be regarded as normative" in doctrine (p. 332).

Scholars and students will welcome the present volume and look forward to those that are to follow.

GLANVILLE DOWNEY  
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FRANK M. CLOVER. *Flavius Merobaudes: A Translation and Historical Commentary*. (Transactions of the American Philosophical Society,

New Series, Volume 61, Part 1.) Philadelphia: the Society. 1971. Pp. 78. \$3.00.

Interest in late Roman panegyrists as sources for the history of their times has quickened in recent years. The best of them, Claudian, has been the subject of perceptive study. Now a lesser figure, the fifth-century poet Flavius Merobaudes, receives the attention of a monograph, which presents for the first time an English prose translation of, and a historical commentary on, the extant fragments of his work. An appendix includes the complete Latin text (without apparatus) as established by Vollmer.

Clover is obviously at home in his period, and his familiarity with the ancient sources and modern literature concerning Merobaudes is exemplary. He has produced a study, which, within limits, can be read profitably by historians of late antiquity. His translation conveys the sense of Merobaudes' allusive style and will be a serviceable aid to scholarship. The author has also reinterpreted the significance and dating of Merobaudes' panegyrics, but his new views will result in only minor changes in our conception of the period. Thus, he argues convincingly (pp. 32-41) that *Panegyric I* is not, as commonly supposed, a *laudatio* in honor of the general Aetius and composed in 437, but rather a *gratiarum actio* datable to 443-46. The author is aware of the difficulties inherent in drawing historical conclusions from a genre of literature that combines propaganda and poetic fancy. On this point the reader should consult Alan Cameron's *Claudian: Poetry and Propaganda at the Court of Honorius* (1970), which was published too recently for Clover to cite.

WILLIAM G. SINNIGEN  
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City College of New York

#### MEDIEVAL

IOLE MAZZOLENI. *Paleografia e diplomazia e scienze ausiliarie*. Naples: Libreria Scientifica Editrice. 1970. Pp. 474, 53 plates. L. 10,000.

Full-fledged treatment of all of the medievalist's traditional auxiliary sciences within the covers of a single volume is an idea that has been somewhat out of fashion. The present volume, a revision of the author's *Lezioni di*

*Paleografia e Diplomatica* (1964), partially resurrects the old idea and in doing so shows that there were some good, if not conclusive, reasons for its earlier interment. Studies on each of these sciences have so proliferated that it is very difficult for any one scholar to keep up to date on all of them, and, even if he could, it would be almost impossible to compress his knowledge within a single manageable volume. Professor Mazzoleni has made a valiant and in some respects an admirable effort, but she has not entirely succeeded in overcoming the difficulties, even by having other specialists contribute brief and useful chapters on heraldry (Ferdinando Acton di Leporano), numismatics (Giovanni Bovi), weights and measures (Catello Salvati), and sigillography (Antonio Allocati), as well as a chapter on transcribing documents (Luigi Pescatore). The treatment accorded to paleography and diplomatics touches on all phases of these subjects. With respect to paleography, on which I prefer to concentrate, the generous attention paid to the often-neglected documentary scripts is particularly welcome. The author maintains an independent approach, but for the most part she follows the lead of Luigi Schiaparelli, Giulio Battelli, or Giorgio Cencetti. Although the work of these masters is inevitably aging and although the author makes some new contributions, her work does not replace either Battelli's *Lezioni di Paleografia* (3d ed., 1949) or Cencetti's *Lineamenti di Storia della Scrittura Latina* (1954). For an introductory survey, too much space is devoted to recounting past controversies, while important current views are completely neglected, for example, Bernhard Bischoff's opinions on the development of Caroline minuscule. Mention is made of the theories of Jean Mallon on the scripts of the Roman epoch, but neither his nor Robert Marichal's views are dealt with in adequate fashion. And a disproportionate amount of space is accorded to Italian, particularly south Italian, materials at the expense of other deserving subjects. Punctuation, for example, is dispatched in one page. In the bibliographies appended to each chapter even experts may find materials of whose existence they were unaware, but they will also be surprised by what is overlooked and dismayed by the number of works cited incorrectly.

Somewhat ironically, one of the features that detracts from the book's usefulness as a general introduction to the auxiliary sciences, namely its overemphasis on materials from Italy, turns out to constitute its special value. Any scholar interested in the medieval manuscripts and documents from this region will find here many rewards. The numerous plates in the volume, although many are unfortunately of very poor quality, will be welcomed for the same reason. They reproduce a large number of Italian documentary and humanistic scripts. Also welcome are the transcriptions provided for most of the facsimiles.

JAMES J. JOHN  
Cornell University

JAMES A. BRUNDAGE. *Medieval Canon Law and the Crusader*. Madison: University of Wisconsin Press. 1969. Pp. xx, 244. \$12.50.

Professor Brundage's stated purpose is to provide "a study of the crusades as a canonistic institution." What he has actually written is an important volume on a narrower topic, the crusader's vow and the web of privileges, spiritual and temporal, that it entailed. The most important legal question involving the crusader, the justice of the war in which he engaged—an important question in the canonistic literature—is not discussed in any detail.

Following Michel Villey, Brundage argues that "the mechanism of the vow made it possible for the Church to turn this enthusiasm [for the crusade] into a binding commitment" so that after the initial fervor evaporated a system of legally binding sanctions could be applied to the laggard. Brundage's major contribution is to describe in detail the development of canonistic thinking on the vow and what it entailed. Where Villey argued that crusade legislation was a new development within canon law to meet a new situation, Brundage is able to show clearly that in fact the canonists were adapting old regulations concerning pilgrims to meet the special needs of a special kind of pilgrim, a pilgrim bound also to engage in a holy war. Furthermore, he has benefited from a generation of interest in medieval canon law so that where Villey spoke of the legal status of the crusader in general terms, Brundage is able to cite a number of specific legal points concerning the vow and the opinions of the lawyers on these

points. Where Villey had a small amount of canonistic material available to him, Brundage's bibliography contains six pages of printed and manuscript canonistic materials.

Finally, Brundage raises an interesting question about the canonists that deserves further study. Why, so far as we know, did no canonist write a full-length treatise on the crusade? So important a subject should have elicited a thorough treatment and, as he notes, Hostiensis, among others, mentioned the lack, but no one seems to have remedied it. Although Professor Brundage has not considered all the aspects of the crusader's status in canon law, he has paved the way for future studies in this model study.

JAMES MULDOON  
Rutgers University,  
Camden

CHARLES T. WOOD. *The Age of Chivalry: Manners and Morals, 1000-1450*. New York: Universe Books. 1970. Pp. 175. \$10.00.

RICHARD BARBER. *The Knight & Chivalry*. New York: Charles Scribner's Sons. 1970. Pp. x, 383. \$12.50.

Despite the similarity of titles, these two books have little in common in their subject matter. They are both, however, written for the general reader or beginning student and share a certain amateurishness of approach—this in spite of the fact that Dr. Wood is the author of other highly professional books. While almost entirely lacking in originality, they will serve a highly useful purpose. Dr. Wood's book is an excellent introduction to social life in the high and later Middle Ages, while Mr. Barber has brought together material on chivalry hitherto only available in various monographs.

Dr. Wood's study of life and manners in the period 900-1450 is very readable. Economic and social relationships are his chief concern as he sets forth his views on the peasantry, the nobles, and the Church in three principal periods. These views tend to be traditional (that is, in the scholarly world) and at times would be considered outmoded by many. Curiously, although his specialized work has been on thirteenth-century themes, his opening pages on "The World of the Millennium" are perhaps the best. Of course he disposes of the notion

that Europe awaited the year 1000 A.D. in terrified anticipation. His section on the "High Middle Ages" is adequate if uninspired. Most readers will be very hesitant to accept his gloomy view of the fourteenth and fifteenth centuries as set forth in his epilogue. There are signs of haste here, and the overall picture of dire calamity and collapse is unacceptable—at least to me. Apt if conventional quotations and illustrations help the book along. Altogether a book well worth putting in the hands of a beginning student or an interested friend.

Mr. Barber's work is also a piece of generalized popularization. Here chivalry is the core of the book. By taking a more restricted topic than Dr. Wood he is able to assemble a mass of information that is simply unavailable in any other single volume, at least in English. A much bigger, more scholarly work is needed in this field. Yet in trying to bring together the literary tradition of chivalry and the practice of knighthood in a single volume, Mr. Barber has done yeoman service. There is a total absence of the gushing sentimentality and the acid skepticism that has marred so many writings on similar themes. Yet one is brought up short by such assertions as that knights draw their ideas of love from the romances. Such failures of insight could be cited many times over. But no one else has hitherto brought together the themes of military knighthood, knightly tenure, romance, chivalric orders, and the idea of the crusade. Mr. Barber may blur the lines between fact and fantasy at times, but it will take a formidable scholar to get the whole thing right. In the meantime his book is far the best yet written for the student who is interested in the codes, conduct, and imaginings of medieval knights.

MICHAEL R. POWICKE  
University of Toronto

HILARY L. TURNER. *Town Defences in England and Wales: An Architectural and Documentary Study, AD 900-1500*. [Hamden, Conn.: Archon Books. 1971. Pp. 246. \$9.00.

Miss Turner has aimed her book at dating surviving defenses, analyzing why they were built, how they were paid for, and what uses they served. Dividing her treatment of the subject into six all-too-short chapters, she uses some

eighty pages to examine the historical context in which town wall building arose. The major part of the book is devoted to a gazetteer listing walled towns from seven sections of England and Wales.

The author must have had great fun visiting her sites, although envious beginners should know of dangers as well as joys—nettles flourish around ancient walls. A major nettle in architectural history seems to be linearity, the description of points along a wall. Towers of so-many feet (meters) in diameter are connected by curtain walls so-many feet (meters) long with mural bastions at intervals of so-many feet (meters). Miss Turner rounds her linearity with historical chapters in which she attempts to answer the questions that arise out of centuries of town wall building. But over a period of six hundred years, 900–1500, the treatment is necessarily thin. Conway and Caernarvon, for example, were military units in the Welsh wars of Edward I, as well as towns. Their form, siting, construction, and original planting reflected the needs for security, advance base depots, and supply that Edward met in his conquest of Wales. Berwick, unlike Conway and Caernarvon, has not been treated in detail. Its fortifications were indeed begun before 1313 (p. 78), as the documents describe. In fact Edward started on them immediately after his forces stormed Berwick's earthworks in 1296. These new fortifications protected a reformed town that became the major port and advance base depot for Edward's Scottish wars. Similar considerations could be made for Carlisle and perhaps nearly as much for other towns.

This book raises the question of whether the history of building, to be made fully useful, requires structures to be placed as parts of a wider framework than themselves. Miss Turner has range and sweep to her book, certainly; what is wanting are the circumstances underlying the construction and maintenance of those walls. She applies her historical imagination admirably in analyzing details of construction. Some parts of the book provide information not to be found elsewhere, and her long gazetteer sections on such towns as Newcastle, Yarmouth, and Tenby are invaluable. Overall, it is a useful survey.

A. Z. FREEMAN

*College of William and Mary*

ELISABETH G. KIMBALL, editor. *Records of Some Sessions of the Peace in the City of Lincoln, 1351–1354, and the Borough of Stamford, 1351.* (Publications of the Lincoln Record Society, Volume 65.) Lincoln: J. W. Ruddock and Sons, for the Society. 1971. Pp. xxx, 67. £4.00.

This Peace Roll concerns history, law, and genealogy, and it is the sixth such volume that Elisabeth Kimball has edited. Her professional skill makes it a model of economy, for instead of translating each Latin entry verbatim, she gives a neat, compact précis of its main points. An illuminating introduction describes the way the courts did their business, how the clerks put the rolls together from notes taken in court on scraps of parchment (as were the early Year Books), the qualifications of the justices, and the dates (outside the law terms) of the sessions. The cases are common felonies and a variety of trespasses by wrongdoers called "common malefactors," "disturbers of the peace," "nightwalkers," and "night peepers through doors and windows." The penalties imposed, from fines to hanging, seem disproportionate to the offenses and to one another but perhaps not to the convict's status or repute. For the genealogist, there are many names and next of kin (well indexed), but his clients may not like to learn about their Lincoln progenitors' crimes and punishments. Clearly, Elisabeth Kimball has succeeded to Bertha Putnam's position as scholarly authority on medieval crime and justice in shire and city.

WILLIAM HUSE DUNHAM, JR.  
*Yale University*

YVES RENOUD. *The Avignon Papacy, 1305–1403.* Translated by DENIS BETHELL. [Hamden, Conn.:] Archon Books. 1970. Pp. 157. \$7.50.

With the English translation in 1963 of G. Mollat's standard work *The Popes at Avignon 1305–1378* and with the appearance in the last decade or so of a number of major studies (e.g., by Zacour, Guillemin, Favier, and Esch) that both demand and permit the construction of a new synthesis, Renouard's compendium seems rather out of date. Originally a modest and cheap little paperback in the Que sais-je? series (1954), it now appears in English as an expensive hardback, aimed at "pupils who do not read French" and who, presumably, do not have enough time to read Mollat. In any case

the book exists, and we must now consider its merits. First, Renouard, unlike Mollat, covered the Avignon popes of the Schism (at least until 1403), and rightly—even though the translator has imposed his personal belief that Clement VII and Benedict XIII were antipopes by putting their names in single quotes. Second, since Renouard had no interest in Mollat's apologetic intention to refute the idea that France dominated the Avignon papacy, he could give due emphasis to more important matters—like the geopolitical significance of Avignon in relation to papal concern with France, England, and the North generally, the remarkable effect of the papal stay in making Avignon a major economic center of the South, the cultural importance of the papal court, and the alleged relationship between the institutionalization of papal government and the insensitivity of the Church to the vital spiritual currents of the time. Everything is too brief, and sometimes what seems to be novelty is merely a manipulation of stereotyped categories of late-medieval history (such as the nation-state, secularization, or humanism), but often enough Renouard's socioeconomic orientation suggests the lines along which the new synthesis will probably emerge. If, as suggested above, his book is out of date, it is also prophetic. It has the quota of errors and bad generalizations usual in such works, and the translator has noted two or three of them; he has also, more conscientiously, provided an up-to-date bibliography.

HOWARD KAMINSKY  
University of Washington

MICHAEL JONES. *Ducal Brittany, 1364-1399: Relations with England and France during the Reign of Duke John IV.* (Oxford Historical Monographs.) New York: Oxford University Press. 1970. Pp. xxi, 250. \$11.25.

In the middle of the fourteenth century John de Montfort, nephew of the late Duke John III of Brittany, and Joan de Penthievre, niece of the same duke, engaged in civil war over succession to the duchy. With the defeat and death of Joan's husband in 1364 Charles V of France recognized Montfort, who became Duke John IV. Brought up at the court of Edward III who gave him indispensable aid in the civil war, John was caught for the remain-

der of his life between the French king, who expected him to be either a faithful vassal or a neutral magnate, and the English king, who required grateful compensation for past assistance and cooperation in his dynastic struggle against France. The Hundred Years' War contributed much significance and complexity to this topic.

This is not, therefore, a social and economic history of a French province, as a first glance at the title may lead some to infer. The subtitle suggests the content of the book, for it is a study in medieval diplomacy and political relationships, a closely argued thesis that John IV of Brittany did not wish to side with either his English patron or his French lord but to establish and maintain an autonomous duchy. In the process, Mr. Jones disputes and seeks to revise the opinions of La Borderie, Pocquet du Haut-Jussé, and Planiol, who insisted that John's household was an English fifth column in France and that the duke himself was politically inept and an Anglophile for most of his life. So persistently does the author defend his thesis that a chapter on ducal government is not a study of bureaucracy and administration but a demonstration that John's council was not dominated by Englishmen and that his governmental organization was not inspired by English procedures but was modeled on, or similar to, that of other French duchies.

The English honor of Richmond and the Breton nobility were important factors in John's maneuvering with the two monarchs. Perhaps if John could have divested himself of the desire or need for the former, his search for independence would have been easier. While the author recognizes the Breton seigneurs as an influence in the duke's policy, they may well have been the most important, and the duke's inability to secure and retain their support may have been his greatest failure. One wishes that Mr. Jones had given more detailed treatment to this group. And genealogical tables would have been helpful in a study that concerns ducal dynasties.

FRANKLIN J. PEGUES  
Ohio State University

SALVATORE TRAMONTANA. *I Normanni in Italia: Linee di ricerca sui primi insediamenti.* Volume 1, *Aspetti politici e militari.* (Università degli

Studi di Messina, Facoltà di Magistero. Pubblicazioni dell'Istituto di Storia "Vittorio de Caprariis," Number 2.) Messina: Peloritana Editrice. 1970. Pp. 215. L. 4,000.

This study represents the first of a series of volumes that the author is projecting dealing with the establishment of a Norman Kingdom of Two Sicilies in southern Italy. It covers in four chapters the period down to 1046 when the three Norman principalities had appeared there. The first chapter deals with southern Italy on the eve of the Norman arrival in the late tenth century. The second summarizes the Normans' Scandinavian past and their establishment in Normandy itself. The third tells of Viking incursions that reached Italy and the first contacts of Normans with the region. The fourth is a detailed political study of their establishment in this part of Italy down to 1046. Extensive use is made throughout of Italian, Western European, and Byzantine sources that mention Scandinavian Viking and later Norman contacts and activities in southern Italy during the period covered. These sources are critically examined and the information they contain correlated to produce a coherent story.

Unfortunately this book suffers from two major deficiencies. In the first place the author wrongly assumes more relationship between the Normans and a Scandinavian past than he has any right to do. By the time the Normans arrived in southern Italy they were Norman French in language, culture, and military and political organization. Except for vague memories the Normans had no links with their Scandinavian ancestral home or with earlier Viking expeditions into the Mediterranean.

Second, the narrative the author presents is lacking in any understanding of the military realities of the time. We find nothing about fortresses and no sense of how the Byzantines organized their southern Italian lands with which Helene Ahrweiler and others have dealt extensively. We learn nothing about how Lombard dukes prepared themselves and their realms for war and no understanding of the way Norman military forces were able to operate. One misses the insights of David Douglas's *Norman Achievement* (1969) or what Whaley has told us of their military practices on the southern Italian scene after 1060. In

short the historian is advised to look elsewhere for a coherent explanation of the political and military side of the first Norman establishment in this part of Europe.

ARCHIBALD R. LEWIS  
University of Massachusetts,  
Amherst

GIOACCHINO VOLPE. *Studi sulle istituzioni comunali a Pisa: Città e contado, consoli e podestà, secoli XII-XIII*. New edition with an introduction by CINZIO VIOLANTE. (Biblioteca storica Sansoni, New Series, Number 48.) [Florence:] G. C. Sansoni Editore. 1970. Pp. lxvii, 523. L. 6,000.

Late in 1971, having long passed his ninetieth birthday, Gioacchino Volpe died. His influence on medieval Italian historiography has remained remarkably strong, particularly if one considers that his last monograph dealing with medieval history was published in 1913. In the course of the past decade many of his works have been reprinted. First published in 1902, his *Studi sulle istituzioni comunali a Pisa*, one of the earliest and weightier of his monographs, presented Volpe's long-debated theories regarding the origins of the Italian communes and the social composition of the Italian urban elites during the communal and late medieval periods. In the course of the past thirty or more years many of Volpe's ideas—those dealing with the general process by which urban centers grew in Italy, as well as those concerned more directly with Pisan history—have been subjected to a series of often damaging criticisms. The work of Ottokar, Sestan, Cristiani, and Herlihy make it possible for us to examine the historical questions that Volpe addressed from a more interesting perspective and to reach subtler and more satisfactory conclusions about the course of Italian history.

But one of the real gifts of Gioacchino Volpe, an attribute that Cinzio Violante rightly stresses in his sympathetic and judicious introduction to the reprint of this work, was Volpe's concern with capturing the dynamism of history, his attempt to penetrate behind the façade of things, and by drawing together a variety of disparate factors to write what today we describe as *histoire totale*. For this reason, even seventy years after its first publication, a

rereading of this work can still prove instructive to students of medieval history.

ANTHONY MOLHO  
Brown University

ARMANDO SAPORI, editor. *Libro Giallo della Compagnia dei Covoni*. With a study by GIULIO MANDICH. (Pubblicazioni della Direzione degli "Studi Medievali," Number 4.) Milan: Istituto Editoriale Cisalpino. 1970. Pp. ccxxiii, 381. L. 35,000.

Among the mercantile families of Florence in the *trecento*, the Covoni were of middling importance. The *Libro Giallo* is an account book that records their operations, chiefly transfers of funds between Florence and Venice or Padua, over the years 1336 to 1340. Armando Saponi, to whom we owe the publication of several similar accounts, has given the *Libro Giallo* a truly magnificent edition. Saponi himself provides a historical introduction to the Covoni family, and Giulio Mandich contributes a long and technical analysis of the varied transactions recorded in the book. Brief biographies of all Covoni who appear in the accounts, family trees, and a thorough analytical index add to the utility of the edition. This type of source will probably most fruitfully be used in conjunction with the other extant account books of the period, as together they provide a unique insight into the commerce and commercial practices of Florentine banks in the fourteenth century.

DAVID HERLIHY  
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Madison

*Dumbarton Oaks Papers*. Numbers 23 and 24. Washington: Dumbarton Oaks Center for Byzantine Studies, Trustees for Harvard University; distrib. by J. J. Augustin, Publisher, Locust Valley, N. Y. 1969-1970. Pp. xiii, 393. \$15.00.

The latest *Dumbarton Oaks Papers*, a double number for the price of one, is of excellent value. From the 1967 symposium on Justinian and Eastern Christendom (largely published in *Dumbarton Oaks Papers*, 22 [1968]) comes an intriguing paper by Professor Antoine Guillaumont on imperial dealings with the Persian Nestorians, who have been somewhat overlooked in considerations of Justinian's often ham-handed

religious policies. Guillaumont publishes a supposed dialogue between the emperor and Paul, the Nestorian bishop of Nisibis (in BM Add. MS 14535), from a Syrian Monophysite source, apparently dating from after 561. Information on the actual beliefs of the Nestorians is rare and the document is therefore of great interest, but it should surely not be regarded as an actual *procès-verbal*, any more than are the published discussions between Manuel II Palaiologos and a Muslim teacher. Dr. Averil Cameron's paper on Agathias's two excursuses on the Sasanians also falls into the scope of the 1967 symposium. Virtually no Sasanian literature survives, but the royal annals can be partly reconstructed from later Persian and Arab sources. The most direct and contemporary source for the tradition, however, is in Agathias, and Dr. Cameron's substantial contribution skillfully demonstrates how far the Byzantine chronicler can be relied upon for it.

The 1968 symposium, from which three papers are included, was on after the fall of Constantinople. Professor Halil Inalcik brings much recently available Ottoman material to bear on his study of Mehmed II's treatment of the Greeks of Istanbul. The traditional view of a policy to favor the Greeks and repopulate the city is largely substantiated, although in 1477, Istanbul and Galata numbered only 3,743 Greek and 3,095 "other" families against 9,486 Muslim. There are notable examples of continuity—for example, part of the endowment of Hagia Sophia passed to the *vaqf* of the new mosque (there are parallels in the Pontus). Professor Speros Vryonis's contribution, "The Byzantine Legacy and Ottoman Forms," is a fascinating and wide-ranging preview of his forthcoming book. This field, although of the highest importance, has not been attempted since the more limited research of F. Hasluck and M. Köprülü; now substantially more Ottoman archives can be used, and the real nature and stages of the de-Hellenization of Anatolia are becoming a little clearer. The role of the Turkomans is, surely correctly, emphasized, and the swiftness of the process before the tide turned, demographically, in favor of the Greeks again in the late eighteenth century is revealed. Even so, it is hard to believe Ömer Barkan's figure of only eight per cent for the

Christian element of Anatolia as early as ca. 1500. Professor Vryonis concludes that by decapitating Byzantine *Hochkultur*, the Turks drove the Greeks to defend a folk culture from which they themselves inherited much. Professor Vryonis has a keen eye for significant detail, but it is perhaps unnecessary to suggest that the Turks are indebted to the Byzantines alone for such Middle Eastern commonplaces as the threshing sledge.

The third paper, on Theophanes the Cretan (fl. 1527–59), the leading artist of what was near to being a Byzantine *Hochkultur* that survived, is by Dr. Manolis Chatzidakis, the *doyen* of Byzantine art historians in Greece. This monograph includes biographical documents on the painter of the Meteora and Athos and what amounts to a corpus of his work in 132 black-and-white plates. In the stance of Adam naming the animals in St. Nicholas, Meteora, one wonders if there is not a final echo of the classical statuary in Constantinople, which earlier artists had used in mimesis when depicting Adam.

In other articles Professor Herbert Hunger examines the Byzantine imitation (mimesis) of antique literature in their own. The author is expository and necessarily sympathetic to the discipline—even to the dreadful assemblage of the Christ story in a patchwork of Homer-centos: “the naive and moving attempt to clothe the history of salvation, . . . in that linguistic garb which was most venerable to every Greek. . . .” Professor Ihor Ševčenko publishes and comments, with characteristic skill and thoroughness, on three poems on the death of Leo VI (probably dating before 913) and one on the death of Constantine VII (probably of 959) in the margins of the Madrid Skylitzes. The poems tell the historian little, but, although perhaps derived from formal epitaphs, they are comparatively early examples of the political verse that was to link the two cultures of Byzantium.

Mr. Rowland Mainstone’s investigation into the tympanums of Hagia Sophia could be disconcerting. Three carbon-14 dates have a mean of ca. 700 A.D. ( $\pm 120-30$ ), which, together with examination of the masonry, suggests that the tympanums were reconstructed after the earthquake of 869. This fits in with the probable date of the mosaics, but it makes one wonder

about the condition of the dome, if the reconstruction was as extensive as the author presumes. Professors James Morganstern and Richard Stone have a carbon-14 date of 794 A.D. ( $\pm 44$ ) associated with the mosaics in the diakonikon of Dereagzi in their latest report on the church. This suggests that the mosaics and the church must probably be dated soon after 843—that is, if the iconoclast ban really was complete in the provinces. Three tiny fragments of mosaic, which the unwary visitor to the Antalya museum will trip over in a darker corner, are described exhaustively; they seem to add up to a theophany. Professor Cyril Mango provides most useful details on the hitherto botched publication of the graffiti in the dome of Hagia Sophia, Trebizond, on the Palaiologan Infant Cycle in the Chalkoprateia, and on the Stone of Deposition in the Pantokrator, whose iambic inscription he resurrects.

The double volume begins with obituaries of two Dumbarton Oaks professors: Paul Underwood, whose monument is the work in the Kariye Camii and elsewhere in Istanbul, and Romilly Jenkins, its former director of studies and the author of a number of brilliant papers in *DOP*. All Byzantinists and members of Dumbarton Oaks will feel their loss.

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*Travaux et mémoires*. Volume 4. (Centre de Recherches d'Histoire et Civilisation Byzantines.) Paris: Éditions E. de Boccard. 1970. Pp. 526.

Following the pattern set by its three predecessors volume 4 of *Travaux et mémoires*, published by the Centre de Recherches d'Histoire et Civilisation Byzantines under the direction of Professor Paul Lemerle, continues to be subdivided into several main sections.

The initial *mémoire* presents a critical edition and translation, by the group of scholars associated with Professor Lemerle, of the major part, albeit not all, of the Greek sources pertaining to the Paulician heresy. The subsequent dossier is entirely devoted to Gilbert Dagron’s investigation of “Les moines et la ville. Le monachisme à Constantinople jusqu’au concile de Chalcédoine (451)” in which he traces the urban, social, and often marginally orthodox character of the beginnings of Con-



stantinopolitan monasticism, its opposition to the ecclesiastical hierarchy of the capital, its political role in cooperation with the Alexandrian patriarchs in the Christological controversies of the fifth century, and the ultimate victory of the patriarch of Constantinople over this dissident element through its institutionalization by the middle of the century.

Considerably expanded, the *études et documents* include a larger number of relatively shorter articles on a variety of subjects than did the earlier volumes: J. Grosdidier de Matons's extensive study of a pseudohagiographic moral treatise, "Les thèmes d'édification dans la Vie d'André Salos"; the prosopographic clarifications of W. Wolska-Conus, "De quibusdam Ignatiis," and D. Papachryssanthou, "Hiérissos, métropole éphémère au XIV<sup>e</sup> s."; the systematic analysis by Ch. Astruc of the *Codex Parisinus Graecus 180*, "Le livre III retrouvé du Commentaire de Théodore Méliénienotes sur les Évangiles," which he identifies as a compilation of earlier patristic material rather than the original exegetical treatise it purports to be; and P. Schreiner's "Note sur la fondation de Monembasie en 582-583," confirming the early existence of the city. As in the past, considerable space has been devoted to the publication, translation, and study of hitherto unfamiliar sources: J. Lefort's "Prooimion de Michel, neveu de l'archevêque de Thessalonique, didascale de l'Évangile," one of a group of *Prooimia* of which a general study is planned; J. Gouillard's valuable identification of the *Letter* of the patriarch of Constantinople to the bishop of Larissa, "Une source grecque du Sinodik de Boril: la lettre inédite du patriarche Cosmas," as both a new link between the Byzantine and Bulgarian worlds and a source on Neo-Paulician-Bogomil theology; and finally, M. Balard's detailed account of the disappointing Genoese campaign of 1351-52, "À propos de la bataille du Bosphore: l'expédition génoise de Paganino Doria à Constantinople," to which he appends the log of one of the Genoese galleys, as well as other records, and in which he stresses the support received by the Genoese from the Turkish emirs and the incommensurate expense of the expedition in lost ships and lives, as well as in extraordinary taxation at home.

The usual bibliographical survey, I. Sorlin's useful "Bulletin des publications en langues

slaves: recherches soviétiques sur l'histoire byzantine," begun in volume 2 of this series, has now been extended to 1968. To these familiar divisions, the present volume adds the new rubrics: *epigraphica*, containing J.-P. Soldini's brief "Notes sur quelques inscriptions de Chypre," and *Informations*, with G. Dagron and J. Lefort's still briefer but valuable "Missions au Mont-Athos" concerning the communities of Esphigmenou and Russikon and attesting once again the alarming state of the latter foundation.

The excellence of the specialists responsible for *Travaux et mémoires*, under the leadership of Professor Lemerle, is beyond enhancement, and the present volume continues the now firmly established tradition of scrupulous and original scholarship characterizing this series. The critical edition of the Greek Paulician sources with their multiple problems fills a long-standing lacuna, as does their translation into a Western language, albeit Russian versions of the so-called *Histories* of Peter of Sicily and the Patriarch Photius had been made by H. Bartikyan in his *Istochniki dlia izucheniiia istorii pavlikianskogo dvizheniia* (Erevan, 1961). Of particular interest are the various versions of Paulician *Abjuration Formulae* given by Jean Gouillard, and, in general, future studies must unquestionably base themselves on all of these texts.

Until the announced publication of Professor Lemerle's more extensive commentary on these sources in his forthcoming history of Paulicianism in Asia Minor (see page 2), the present work must be considered in some sense preliminary. Certain questions remain still: for example, the reason for the acceptance of the much attacked *Histories* attributed to Peter of Sicily and the Patriarch Photius, despite their contradictions and inaccuracies, as authentic sources rather than as compilations (see page 94). Despite the editor's statement that only the "fundamental" Greek sources on the Paulicians are included (p. 1), the selective nature of the present edition might have been made clearer, and some discussion of the basis for the selection would be welcome. Are the *Logoi* that follow the *Histories* to be eliminated as irrelevant? What of the notices in the *Chronicles*, not only the debated passages in the alternate versions of George the Monk (which

are discussed though not published) but the complementary historical notices found in the *Basileia* of Genesius and the Continuator of Theophanes? Should there not be some mention of the existence of the *Letter* of the Patriarch Theophylakt to Tsar Peter of Bulgaria even though the excellent edition of I. Dujčev, "L'epistole sui Bogomili del Patriarca Constantinopolitano Teofilatto," in volume 2 of *Mélanges Eugène Tisserant* (Vatican City, 1964) is available? It is to be hoped also that a parallel edition of the Armenian sources without which any study of Paulicianism must remain dangerously distorted will complete the present publication. Even at this preliminary stage, however, a major contribution to Paulician history has been made through the final establishment of the manuscript histories and the texts of a number of crucial sources. This solid beginning makes us await with all the more impatience the complementary appearance of Professor Lemerle's promised synthesis.

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ROBERT BROWNING. *Justinian and Theodora*. New York: Praeger Publishers. 1971. Pp. 272. \$15.00.

Although there are already many books for the general reader on the reign of Justinian and Theodora, Robert Browning's biography is a valuable addition to the list. The general outlines of this portrait of Justinian and his consort are not new, but here the sources for the reign are analyzed thoughtfully to provide a detailed narrative written in a compelling style. The author has attempted "to depict the events as they may have appeared to the emperor himself," and it is the personalities of Justinian, Theodora, and the men who served them that interest Browning rather than the institutions of the sixth century.

Any student of Justinian must deal with the fact that the fullest contemporary description of the emperor's reign comes from the vitriolic *Secret History* of Procopius. Like other modern scholars, Browning has used the information given by Procopius without adopting his viewpoint. In this biography Justinian emerges as a flexible and effective autocrat, and Theodora as an intelligent and loyal partner to the em-

peror. In one of the most interesting sections of the book the various officials appointed by Justinian are vividly portrayed, and the choice of men as unexpected and talented as Narses and John the Cappadocian is shown as one indication of Justinian's skill as a ruler. Browning's final assessment of Justinian's reign is a favorable one. As other historians have concluded, the emperor's program of reconquest and his attempts to bring about Church unity must be viewed as a single plan for restoration that grew out of concepts shared by Justinian's contemporaries. These ideas made the continuity of the Christian empire a central part of the pattern of history, in which its divine mission could hardly be accomplished if the empire itself no longer existed or if Christianity remained divided. Thus Justinian's wars were not grandiose schemes, but part of a theory that was always central to the empire. Nor could Justinian have foreseen the new invasions that were to destroy his restored empire. The greatest weakness in the book is its exclusion of the study of the institutions with which Justinian had to work. The difficulties of the Italian campaign, as recent scholars have shown, cannot be understood without a consideration of Justinian's armies themselves. Similarly, can financial and domestic policy be explained without a more thorough analysis of the economic and social structure of the time? In spite of these limitations, this is a fine narrative. It would make a good introduction to the study of Justinian's reign. The many illustrations from sixth-century art are a real addition to the work.

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#### MODERN EUROPE

FRANK D. PRAGER and GUSTINA SCAGLIA. *Brunelleschi: Studies of His Technology and Inventions*. Cambridge, Mass.: MIT Press. 1970. Pp. xiii, 152. \$10.00.

PAOLO ROSSI. *Philosophy, Technology, and the Arts in the Early Modern Era*. Translated by SALVATOR ATTANASIO. Edited by BENJAMIN NELSON. (Torchbook Library Edition.) New York: Harper and Row. 1970. Pp. xii, 194. Cloth \$6.00, paper \$2.95.

The two books under consideration have as their common theme the role of technology in early modern Europe. Aside from this similarity of theme they are quite different in focus and content. Prager and Scaglia write about Filippo Brunelleschi, a Florentine who contributed to the development of technology in the *quattrocento* through his machines and his triumphs in structural engineering achieved in the Duomo. Rossi's book, translated into English by Salvatore Attanasio, approaches the question of the changing relationship between science as contemplation and science as action in the sixteenth and seventeenth centuries. He concludes that men who combined thought and action, from Bernard Palissy to Galileo Galilei, helped to elevate the workshop to an important and dignified position in scientific advancement.

The first book contains some useful and illuminating material on the quarrel between the Gothic architects and the classical artists in Florence concerning the style of the cathedral, which was moving ponderously toward completion in the late *trecento*. There is also a chapter on Brunelleschi as patentee and contractor that discloses an early attempt to protect an invention for personal profit. One wishes that these carefully researched and well-written insights had been presented in the context of a more objective evaluation of the subject. Brunelleschi's position in Renaissance art and technology is probably secure enough without gratuitous assertions claiming him to be "one of the great developers of Gothic building, the principal founder of the Renaissance, and an important forerunner of modern structural design and analysis." The authors, at their best in relating Brunelleschi's technical contributions, are less convincing when they move into analysis of the merits and authenticity of their subject's achievements.

Rossi's book is a thoughtful appraisal of one aspect of the quarrel of the ancients and moderns that exercised so many minds in early modern times. He is successful in suggesting that sixteenth-century figures such as Robert Norman, Luis Vives, Rabelais, and Vesalius brought observation and practical knowledge to an elevation that allowed for a fruitful conjunction of the theoretical and empirical in the advancement of science. The author also does

creditable work in a chapter on the seventeenth century in which he suggests that the battle had by then been won by the spiritual descendants of the great sixteenth-century intellectual warriors.

In a chapter devoted to "The Idea of Scientific Progress" Rossi makes a significant contribution when he asserts that theories of science acquired the concepts of accumulation of knowledge and collaboration in the sixteenth and seventeenth centuries. He is not entirely successful, however, in his attempt to establish a logical connection between these two ideas and the notion of the inevitability of progress. The author should not be blamed for the fact that a generally adequate translation falters at certain points in this chapter and in a related appendix. Thus passages that seemed to be insightful in the 1962 Italian edition appear confusing and even misleading in the English version. This is particularly true where Rossi is dealing with ideas that cannot properly be rendered as "progress," "pragmatism," or "utility."

Both of these books make a contribution to knowledge—the one because it elaborates the inventiveness of a Renaissance artist, the other because it reminds us again that reflection without observation and action is sterile.

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NICOLE PEREMANS. *Érasme et Bucer d'après leur correspondance*. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège, Number 194.) Paris: Société d'Éditions "Les Belles Lettres." 1970. Pp. 162.

There has been, as Nicole Peremans observes, a need for study of the relations between Erasmus and the Strasbourg reformer Martin Bucer. As often happens in such cases, her study of Erasmus's conflicts with Bucer and the Strasbourg Protestants appeared in the same year as Friedhelm Kruger's *Bucer and Erasmus* (1970), a study of Erasmus's influence on Bucer's intellectual development. Peremans's conclusions are sound if not novel; readers will not be surprised to find that the old Erasmus was irritable and that personal grievances intruded in his controversial writings. On points of detail she has fresh observations. That Bucer was the author of the *Epistola Apologetica* (1530), a reply to Erasmus's *Epistola contra*

*Pseudoevangelicos* (1529), has long been suspected but can now be affirmed with some confidence. She notes, as no one else has, that Bucer virtually admits that the reformer Konrad Pellican had sought to have pressure exerted on Erasmus by the city council of Basel. This circumstance might shed new light on Erasmus's important letter to that body during the Eucharistic controversy. But the principal defect of Peremans's work is that she does not pause for analysis of the issues raised in her narration. One wonders, for example, whether Bucer was correct in maintaining that Erasmus had at one time argued against the death penalty for heresy. Elsewhere Bucer replies to Erasmus's charge that Protestants quoted his writings out of context: did not St. Paul quote an inscription out of context for his sermon on the Areopagus? The interesting thing about this passage, which Peremans fails to observe, is that Erasmus himself frequently used the same argument to justify "dissimulation" for a good purpose. What Bucer learned from Erasmus is perhaps a more fruitful topic than their manifest disagreement.

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KARL JOSEF SEIDEL, *Frankreich und die deutschen Protestanten: Die Bemühungen um eine religiöse Konkordie und die französische Bündnispolitik in den Jahren 1534/35*. (Reformationsgeschichtliche Studien und Texte, Number 102.) Münster: Aschendorffsche Verlagsbuchhandlung. 1970. Pp. 191. DM 34.

Karl Josef Seidel has produced a valuable study of relations between German Protestants and France in 1534-35. The specific focus is announced in the subtitle, *Die Bemühungen um eine religiöse Konkordie und die französische Bündnispolitik in den Jahren 1534/35*.

The stage is set by a description of France's international position between 1529 and 1534 and the growing receptivity to the possibility of improved relations with the German Protestant states. If a colloquy could lead to some *modus vivendi* on the religious question, political and military alliance against the emperor seemed at least a possibility. Various position papers were exchanged, most important of which was Melancthon's *Consilium ad Gallos*,

moderate in tone and optimistic in its hopes for an irenic agreement on essentials.

In letters and memoranda from Melancthon, Bucer, Bullinger, Hedios, Chelius, Myconius, Zwick, Blarer, Luther, and the Du Bellay brothers we discover not only different opinions but also the importance of differing emphases, of how a statement is understood and summarized, and of the havoc created by distortion. Taking full advantage of the printed sources, Seidel has provided an instructive account of a complex interchange of ideas during a brief period of "thaw."

Repeatedly he illustrates the interplay between the discussion of theological questions and the surrounding political realities. The "affaire des placards" in 1534 had regrettable effects on the mind of Francis I. And John Frederick of Saxony's refusal to allow Melancthon to go to Paris for face-to-face discussions well-nigh destroyed all possibility of successful negotiations.

Earlier treatments of the story tended to treat the matter very briefly or to discuss only one aspect of it. What Seidel has now done is to re-examine the topic, pull the relevant material together, and set the whole complex series of exchanges within the necessary political and diplomatic context. In many respects it is a model study—manageable yet significant. It will be of interest not only to modern ecumenists but also to anyone wishing to understand the international currents and crosscurrents of the Reformation.

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JACOB KATZ, *Jews and Freemasons in Europe, 1723-1939*. Translated from the Hebrew by LEONARD OSCHRY. Cambridge, Mass.: Harvard University Press. 1970. Pp. viii, 293. \$11.00.

Jacob Katz's study of Freemasons and European Jews is important in its own right. It is important as well, however, for the knowledge it brings to our understanding of the development of religious toleration, anti-Semitism, and Western European assimilation of Jews, who enjoyed formal equality in the eyes of the law after the French Revolution.

Almost from their start in England in the early eighteenth century, the Freemasons incorporated elements of natural religion and reli-

gious toleration into their constitution. Influenced by deism they held that "a Mason is obliged [only] to obey the moral Law . . . , that religion in which all Men agree" (quoted in Katz, p. 14). In 1736, however, only four years after the first Jew was initiated into the London lodge, the Freemasons qualified their mandate of universal religion. Members, they now maintained, must henceforth conform to "the Christian usages of each Country." For all that—or possibly because of that—Masonic lodges spread to the Continent, first to Holland, then to France.

To participate in Christian lodges Jews often had to compromise their religious traditions. Nonetheless, the number of Jewish members increased, Katz suggests, because membership in the lodges brought tangible advantages and because many Jews felt that they should emulate their Christian neighbors. "Wie es Christelt sich, asoy yidelt sich," they argued. ("As things go among the Christians, so they also must go among the Jews.") It should be noted, however, that some Jews sought to retain their own religious principles within the framework of the lodges. (They were least successful in England, ironically enough, where religious toleration for Masons was first affirmed.) Others, unable to reconcile participation in Christian lodges, formed lodges of their own. In any case the increase of Jewish members suggests that in Western Europe barriers between Jews and Christians were softening before 1789.

But only up to a point. The readiness to accept Jews into Western European society was not universal. Even Freemasons imposed restrictions upon Jews and, at times, were hostile to them—especially in Germany, where Jews were systematically excluded, perhaps because of *völkisch* ideas. In general, as legal barriers to assimilation broke down, social barriers hardened.

In an important way, then, *Jews and Freemasons* suggests the limitations of assimilation in Germany and France, that is, it suggests the persistence of anti-Semitism. Paradoxically the myth that Jews and Freemasons were plotting to control the world continued to persist, especially in the incredible *Protocols of the Elders of Zion*.

*Freemasons and Jews*, I have already suggested, is an important study. It suffers, how-

ever, from an apparent lack of understanding of the relations between Jews and Freemasons in nineteenth-century France. For it is here, as Norman Cohn reminds us, that the myth of Jewish conspiracy gained credibility and was first popularized.

LEON APT

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FEDERICO CURATO, editor. *Gran Bretagna e Italia nei documenti della missione Minto*. Second Series: 1830-1848. Volume 1 (21 agosto 1847-4 febbraio 1848); Volume 2 (5 febbraio-12 maggio 1848). (Fonti per la storia d'Italia. Documenti per la storia delle relazioni diplomatiche fra le grandi potenze europee e gli Stati italiani, 1814-1860. Part 3, Rapporti tra Stati europei.) Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1970. Pp. xvii, 374; 336. L. 5,500 each.

These two volumes, well edited by Federico Curato, have the unique distinction of covering the diplomatic roving mission of one man and of giving the historian a detailed description of persons and situations throughout Italy on the eve of its revolutions of 1848 and 1849. The man with the mission was Gilbert Elliot, second earl of Minto, Lord Privy Seal in the Russell cabinet. In 1847 and early 1848 he was sent to Switzerland, Austria, and then through the entire length of Italy down to Palermo in Sicily and was instructed to send back his observations on the Italian political scene. He was likewise to discourage an impending Swiss civil war, an Austrian intervention in Italy, and the achievement of Italian unity. On the positive side he was to encourage in Italy moderate monarchical parliamentary reforms and civil liberties, obtain the pope's help in quieting the Irish clergy, and mediate a conflict between Sicily and Naples. His observations were of inestimable value to his government and are now to historians of 1848; but nearly all the other purposes of his mission failed by the end of the year. Count de Bresson had had a similar French mission throughout all Italy, and he committed suicide in Naples. Minto, however, was made of sterner stuff and returned home a sadder but a wiser man.

This two-volume collection contains 365 pieces of official and private correspondence, in the English original text, to and from Lord Minto and ninety pages of his diary, which

links together the events in the correspondence. There are letters to and/or from Victoria, Prince Albert, Russell, Palmerston, Charles Albert, and other British diplomats in Italy. The selection was made from the Minto papers and diary in the National Library of Scotland, from the official Foreign Office dispatches and the Russell private papers in the Public Record Office, from the Palmerston incoming letters in the National Register of Archives, and from the Royal Archives in Windsor. However, the Palmerston outgoing letter copies, in his letter-books in the British Museum, were not used. An indication of Professor Curato's careful preparation is the very small number of typographical errors in the English texts. As usual, there are excellent inventories and summaries at the beginning, helpful explanatory footnotes throughout, and an identifying index of persons at the end.

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KARL KAUTSKY, JR. *August Bebels Briefwechsel mit Karl Kautsky*. (Quellen und Untersuchungen zur Geschichte der deutschen und österreichischen Arbeiterbewegung, New Series, Number 2.) Assen: Van Gorcum & Comp. N.V. 1971. Pp. lx, 394. 60.50 gls.

ROSA LUXEMBURG. *Lettres à Karl et Luise Kautsky*. Translated from the German by NADINE STCHOUPAK and A. M. BRACKE-DESROUSSEAUX. Preceded by *L'œuvre et la vie de Rosa Luxemburg*, by DOMINIQUE DESANTI. ("Collection Hier.") Paris: Presses Universitaires de France. 1970. Pp. lxiii, 145. 24 fr.

The correspondence between German Social Democracy's leading political practitioner, August Bebel, and its most prolific Marxist theoretician, Karl Kautsky, covering the exciting and decisive period from 1879 to 1913, could hardly fail to be an intriguing and valuable source for the history of European socialism. How Bebel acted and what Kautsky wrote in those years often proved to be of crucial significance. Their published correspondence, of which more than eighty per cent comes from the years following the death of Friedrich Engels in 1895, complements chronologically the prior publication in individual volumes of the intense exchange of letters between Engels and the inner group of German Social Demo-

crats—Bebel, Kautsky, Wilhelm Liebknecht, and Eduard Bernstein. Unfortunately, while Kautsky faithfully preserved nearly every piece of paper he received from Bebel, the latter was apparently much less careful, and so of the 306 items included in this volume only 26 come from the pen of Karl Kautsky, to which have been added a few written by his wife Luise. As a result of the imbalance many nuances of the relationship between the skillful political tactician and the earnest editor of the *Neue Zeit* remain partially hidden. But we can discover many details about Bebel—his adept guidance of the campaign against Bernstein's revisionism, the care he took in setting the stage for the party congresses, the frequency and the nature of his advice on the policies of the *Neue Zeit*, and his frank and sometimes piercing evaluations of fellow socialists. (How deeply he and Kautsky distrusted the brilliant and irascible Franz Mehring is documented in some piquant phrases.) The letters of the last years turn to personal matters as Bebel faced tragedy in his own family—the death of his wife in 1910 and then the severe and prolonged nervous collapse of his daughter following the unexpected death of her husband early in 1912. "I had imagined that the end would be different," Bebel confided sadly to his Marxist comrade in October 1912, though subsequent letters show that he soon regained the optimistic spirit so characteristic of his active life.

The edition itself meets the high standards set by the previous source publications of the International Institute for Social History in Amsterdam. Although not an academic scholar, Karl Kautsky, Jr. has an intimate knowledge of the Social Democratic movement, and he has compensated to some degree for the missing letters of Karl Kautsky through informative notes and the use of direct quotations from other sources. In a few instances, noticeably in the introduction, Kautsky's solicitude for his father's reputation is evident but not to the detriment of accuracy or balance. For other reasons, however, the introduction is disappointing. The editor has chosen to remain close to the texts published in the volume and passes over an excellent opportunity to present a comparative analysis of Bebel and Kautsky.

The volume of Rosa Luxemburg's *Lettres à*

*Karl et Luise Kautsky* is a French translation from the original German edition of 1923. Except for the competent introductory essay on Luxemburg's life and work by Dominique Desanti, the publication adds little of scholarly value to the original edition, an English translation of which also appeared in 1923. Although some new annotations have been included, others from the original have been omitted, and neither a complete list of the letters nor an index has been supplied.

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GÉRARD CHOLVY. *Géographie religieuse de l'Hérault contemporain*. Preface by GABRIEL LE BRAS. (Publications de la Faculté des Lettres et Sciences humaines de l'Université de Montpellier, Number 32.) Paris: Presses Universitaires de France. 1968. Pp. 513. 39 fr.

It would be hard to disagree with Gabriel Le Bras, the founder of "sociologie religieuse," that of all the works in the genre Cholvy's is one of the finest. Using hard quantitative data he maps out the contemporary patterns of religious practice in the department of Hérault and then places them convincingly in their changing environment—geographic, demographic, economic, social, political, and ecclesiastical. While concerned primarily with the twentieth century he also ranges over the nineteenth century and even into the *ancien régime*, thereby giving the book an unusually deep historical perspective.

The department of Hérault, comprising Mediterranean coastal plains and upland fragments of the Massif Central, belongs mostly to the "pays indifférents à traditions chrétiennes" on Boulard's map (though not to the "pays de mission"). The centerpiece of the book is an analysis of church attendance based on data from over 100,000 questionnaires returned by those attending Catholic churches on Sunday, December 2, 1962. They amounted to only a quarter of the eligible Catholic population, but the proportion varied according to age, sex, marital status, occupation, education, milieu (rural or urban), and region. Many of these findings are not surprising but are valuable for their precision and comprehensiveness. Other data—from baptisms, ordinations, communi-

cants, voting patterns—enrich and qualify the picture. In explaining the generally low level of religious practice Cholvy underlines economic and social factors in this largely rural area, minimizing the adverse effects of the anticlerical press, state schools, and Protestantism; he also blames a reactionary, slow-moving ecclesiastical establishment. In his discussion of regional patterns he is especially instructive on the western coastal plains, relating religious practice to the evolution of large-scale capitalist viticulture, its class conflicts and successive waves of immigrants (including many from Spain). But with the advance of urbanization in recent years, regional influences on religious practice appear to be yielding to national ones, while religion itself withdraws from the public to the private sphere.

Altogether this is one of the most impressive studies of a modern French diocese. Its limitations, which Cholvy acknowledges but does not overcome, are chiefly those of the genre. His book therefore suffers from a certain lack of inwardness, a neglect of the psychological and nonquantifiable dimensions of religion, including superstition and other phenomena of popular devotion. Both the merits and the limitations of the book reflect the present state of the social history of religion as it advances from the sociographic stage to the truly sociological and psychological.

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DAVID MITCHELL. 1919: *Red Mirage*. [New York:] Macmillan Company. 1970. Pp. 385. \$7.50.

This volume demonstrates both the strengths and weaknesses of popular history. It is superbly written, successfully capturing the hysteria and emotions of a highly emotional time. As Mr. Mitchell quite aptly demonstrates, the panorama of rebellion and counterrebellion that spread across Europe in 1919 literally boggles the imagination. The task in describing this panorama, in relating events across Europe—and briefly in the United States and Canada—is awesome, and the author is indeed to be complimented on his ability to organize the incredible diversity of activity into an intelligible account. The extensive bibliography is

annotated with insight and clarity, often giving as much of a clue to the author's interpretations as does the text itself. The illustrations are quite well chosen and serve to illuminate further the passions of revolutionary activity.

Then what is wrong? For one thing Mr. Mitchell's publisher has apparently served him badly. The total omission of documentation in a work full of personal judgments, quotations, and many debatable conclusions is unacceptable. Perhaps it is the publisher rather than the author who is also to blame for such cliché-ridden chapter titles as "Don't Count your Soviets." More seriously, Mr. Mitchell's research is almost entirely limited to English-language sources. While his work in this language has been reasonably exhaustive, especially as regards memoirs, he has nevertheless accepted a limitation that forces him to deal with the tip of the iceberg rather than the entire movement. Thus in dealing with the founding of the Communist party of Germany, for example, he must draw his conclusions on excerpts from English-language works, and these are not necessarily the conclusions that might be drawn had he access to all of the extant materials. The same holds for the founding of the Comintern and other critical events of 1919.

This is, in a sum, an extraordinarily well-written and intelligently presented book. It is, however, by no means definitive history.

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JAMES BARROS. *The League of Nations and the Great Powers: The Greek-Bulgarian Incident, 1925*. New York: Oxford University Press. 1970. Pp. 143. \$6.50.

This work by Mr. Barros on the Greek-Bulgarian incident of 1925 is based mainly on research in British, Greek, Italian, American, and League of Nations archives. Most of the book deals with a description of the day-by-day development of the crisis and its solution. The concluding chapter discusses not only why the intervention of the League was "successful" in this case, but it also generally and rather quickly considers how the 1925 incident shows the limitations of using international organizations like the League and the United Nations for the peaceful settlement of disputes.

Mr. Barros is at his very best when he blends

together materials from the various archives to indicate how the powers, particularly Britain and France, became involved in settling the crisis. His treatment of the roles of the Greek and Bulgarian governments is a little less successful, primarily because he really fails to give enough background information on the internal issues that were helping determine the foreign policies of the two countries. Thus, while we are told that Greece under the rule of the dictator Pangalos may have needed some outside diversion to turn attention away from internal matters, we never fully understand why Pangalos persisted so long in following a course that obviously alienated foreign statesmen who could have been friendly to Greece. Nor are we adequately informed about Bulgaria. We learn correctly that the Macedonian Revolutionary Organization (IMRO) had great power over the Bulgarian government; but, we do not find out sufficiently how IMRO may have helped influence Bulgaria's foreign policy. There is one particularly intriguing question raised about Bulgarian reaction to the clash with Greece. Mr. Barros mentions that in October 1925, as the crisis was in its very first stages, the Bulgarian government proposed that Yugoslavia join in "an immediate military alliance, with a view to combined action against the Greeks, the joint spoils to be the port cities of Salonika, Kavala, and Alexandroupolis" (p. 17). The reader may well ask how the Bulgarian leaders, whose country was heavily demilitarized by terms of the Treaty of Neuilly, could feel able to make any offer with a view toward wresting from Greece three of its most important northern centers.

The above observations are not meant to detract from the general usefulness of this study; they are indications of areas that still need elucidation. Mr. Barros has in the main presented a very competent account of an event in history that heretofore had not received enough attention from scholars.

GEORGE J. MARCOPOULOS  
Tufts University

RICHARD GLEN EAVES. *Henry VIII's Scottish Diplomacy, 1513-1524: England's Relations with the Regency Government of James V.* (Exposition-University Book.) New York: Exposition Press. 1971. Pp. 197. \$6.50.



In spite of its title the focus of this book is on Scotland and its international relationships during the period when John Stuart, duke of Albany, a Frenchman in all but descent, was regent for the young James V. Dr. Eaves is especially interested in describing "the complicated diplomacy by which Scotland preserved her independence," and he has presented his gleanings from the available printed sources with great thoroughness. This is undoubtedly the definitive account of the subject, yet it is flawed by several of the deficiencies frequently attending dissertations. While the style is straightforward, it is nevertheless dull, often repetitious, heavily footnoted, and very detailed, and it contains a minimum of interpretation. One wishes for more elucidation of the basic motives and goals, as opposed to the moves, of the principals; one looks in vain for some discussion of Scotland's real position in international affairs or an evaluation of the extent to which the Scots themselves actually controlled their situation. Perhaps the most fundamental unexamined issue is whether Scotland's continuing independence at this time was the result of Scottish (or French) diplomacy at all. Was Henry VIII ever prepared to pay the price in men, money, and attention necessary for a successful "annexation" of Scotland? It seems to be at least an open question. One can agree that the documents tell "an ugly story of English aggression and meddling in Scotland's domestic affairs," but was the French record substantially better? Dr. Eaves's findings do not significantly alter the picture of the period given in such standard general accounts as Gordon Donaldson's *Scotland: James V to James VII* (1965).

J. WILSON FERGUSON  
Russell Sage College

ROY STRONG. *Tudor & Jacobean Portraits*. Volume 1, *Text*; Volume 2, *Plates*. (National Portrait Gallery.) London: H.M. Stationery Office; distrib. by Pendragon House, Redwood City, Calif. 1969. Pp. xiv, 390; 693 plates. \$48.00 the set, postpaid.

KATHERINE S. VAN EERDE. *Wenceslaus Hollar: Delineator of His Time*. Charlottesville: University Press of Virginia for the Folger Shakespeare Library. 1970. Pp. ix, 122. \$15.00.

Both of these works should encourage histori-

ans of Tudor and Stuart England to use art and the evidence associated with it in their research and lectures. Unlike treatises on attribution and other technical problems and unlike many of the tributes to great country houses and their contents, these books help make the artistic evidence of these reigns accessible and pertinent to general history.

*Tudor & Jacobean Portraits* catalogs all the portraits in the National Portrait Gallery down to 1625, where Piper's catalog of 1963 takes up (the Gallery's portraits of pre-Tudor subjects are included; for example, a silver penny ca. 887 depicting Ælfred). Designing the work "primarily as a working tool for students of the period regardless of the field in which they are active," Strong has made it "an index of faces," with the portraits arranged alphabetically by names of the sitters. Strong refers those concerned with developments in painting during the period to his own work, *This Time's Pencil* (apparently this is a projected but abandoned title for his *The English Icon: Elizabethan and Jacobean Portraiture* [London, 1969]).

Volume 1, the text, contains more than the usual data about the portraits in the Gallery. Biographies of the sitters are reprinted from the *Concise Dictionary of National Biography*; up-to-date biographies might have been worth the extensive space these reprints take. Contemporary descriptions of the appearance of the sitters are given, too. Strong also discusses the portraits in relation to the total iconography of the sitter, and he considers not only the portraits in the Gallery but those elsewhere. A section on groups appears after the last main entry and then two appendixes on recent acquisitions. All of this data (with the bibliography), arranged by sitter, make this volume, in conjunction with volume 2, the excellent "working tool" that Strong planned.

Volume 2 contains black-and-white plates, at least one for each of the Gallery's portraits entered in volume 1, and also reproductions of the principal portraits in other collections discussed in the iconography paragraphs mentioned above. There are, in addition, eight attractive plates in color, seven of them in volume 1 and one as frontispiece to volume 2; not referred to in the text, they duplicate the black-and-white reproductions. Although this "index" shows primarily faces, the background

often depicts much else of importance in Tudor and Stuart history.

Hollar (1607-77), a Bohemian topographical etcher and draftsman, traveled extensively on the Continent, joined the suite of the earl of Arundel, the great collector, in 1636, and thereafter lived most of his life in England. Working for noblemen and gentlemen, the royal family, and publishers, Hollar produced a wide range of attractive work, much of it still surviving. Those wanting information about buildings, landscapes, important men, costume of persons of high and low degree, ships, and famous events are likely to find at least part of what they want in Hollar. Van Eerde, weaving together "the scanty facts concerning the artist and the information given us by his works," has not notably advanced our knowledge of him, but she has produced an attractive guide to his works. By placing reproductions alongside her narrative of his life and of the great events of his time, she makes evident to teachers and students the resources that Hollar brings to us for study of seventeenth-century England and the Continent and especially how such an artist was patronized. Except for the inconvenient placing of the notes at the end of each chapter, the book is very handsomely produced.

Historians of Tudor and Stuart England can get great pleasure and much help in their teaching, perhaps also in their research, from these volumes.

WALTER L. WOODFILL  
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*Davis*

DAVID UNDERDOWN. *Pride's Purge: Politics in the Puritan Revolution*. New York: Oxford University Press. 1971. Pp. viii, 424. \$15.25.

This study of Pride's Purge contains an original interpretation of the manner in which that historic episode took place and its meaning in relationship to the Puritan revolution as a whole. Underdown has sought to demonstrate that it was less a deliberate act of policy on the part of Puritan idealists than the result of a shift in party alignment in the Long Parliament that permitted the radicalism in the New Model Army, the Levellers, and the sects to find vent in December 1648 and January 1649. Working from J. H. Hexter's and Valerie

Pearl's findings he insists that there existed in parliament after 1645 a middle group whose members cooperated with a small number of radicals to form the Independent party. The role of the middle group was pivotal. Its decision to go with the Presbyterians at the time of the Treaty of Newport broke up the Independent party, and with its collapse went the last hope of parliamentary control of the New Model Army. The sequel was revolution in the form of Pride's Purge and the death of Charles I.

If the purge is to be explained in these terms it is no surprise that the Rump government that followed is described as lacking the revolutionary impetus to carry out a thoroughgoing revolution. An essentially conservative body, so Underdown believes, its members had little interest in further change after the King's death and the destruction of the House of Lords. In four main areas of policy—religion, law, social reform, and parliamentary reform—little was accomplished unless the army applied pressure. In these years Cromwell was thinking in terms reminiscent of the middle group in the Long Parliament that foreshadowed the main lines of the Protectorate. "In the end," Underdown concludes, "the typical Rumper was not the determined Commonwealthsman, not Edmund Ludlow or Henry Ireton. It was not even Oliver Cromwell. It was Bulstrode Whitelocke."

This estimate of Pride's Purge is likely to dominate historical scholarship during the foreseeable future. But some questions are bound to arise about Underdown's treatment of political ideology, which in turn has influenced his description of the political complexion of the Long Parliament. It has shaped in key places the tone of his commentary. What he has failed to do is to consider the importance of the theory of mixed monarchy, which was prevalent in the late years of the Puritan revolution. It had grown out of Charles I's highly influential answer to the Nineteen Propositions, and among its advocates were Nathaniel Fiennes, William Prynne, and Whitelocke—all of them conspicuous in Underdown's account. Since the theory subverted the idea of kingship that prevailed before the Puritan revolution, it is difficult to view its advocates as conservative in the way that Underdown does. This is not to deny that Leveller doctrine was

more radical if a greater degree of democracy is the test but rather to insist that the advocates of mixed monarchy were themselves the product of a far-reaching ideological revolution actually more dangerous to the Stuart kingship than Leveller teachings. It had penetrated the establishment in a fashion simply not true of Leveller ideology. The failure to recognize that this was the case lessens the value of this important book.

CORINNE COMSTOCK WESTON  
Herbert H. Lehman College,  
City University of New York

SHEILA LAMBERT, *Bills and Acts: Legislative Procedure in Eighteenth-Century England*. New York: Cambridge University Press. 1971. Pp. ix, 246. \$13.00.

Historians of eighteenth-century England have good reason to be grateful to Miss Sheila Lambert. Several years ago she compiled a valuable list of House of Commons sessional papers for the first half of the century. And in this book, which complements the recent fine study of the House of Commons by P. D. G. Thomas, she provides further expert help with another tangled problem: how legislation was prepared and passed in the eighteenth-century parliament. The book springs from and centers on the work of Robert Harper, an attorney who for thirty years after 1732 was a leading draftsman of and agent for private legislation. Miss Lambert has discovered (the kind of discovery that only comes with long familiarity with the source material) that his working papers formed the basis of the British Museum's collection of private bills before 1767; her further discovery of Harper's own manuscript indexes to these papers in Lincoln's Inn makes the Museum's collection much more accessible than hitherto. Miss Lambert uses Harper's career as a springboard from which to discuss changes in parliamentary procedure over the course of the century, the role of private draftsmen and agents in getting legislation through parliament, and the work of the clerks of the House of Commons and of government departments like the Treasury. Though Robert Harper specialized in estate bills—the unsettling of settled estates—Miss Lambert ranges far outside this area and describes the procedures involved in the preparation and passage of other

legislation, including enclosure and local bills of all kinds. Her careful discussion of the stages of procedure, the changes in the printing of legislation over the century, the dangers of using printed bills as though they invariably represent the legislation as passed, and many other cognate matters will be of the very greatest benefit—not least to those whom the book encourages to exploit the vast amount of social and economic material in parliamentary sources.

J. M. BEATTIE  
University of Toronto

J. P. P. HIGGINS and SIDNEY POLLARD, editors, with the assistance of J. E. GINARLIS. *Aspects of Capital Investment in Great Britain, 1750-1850: A Preliminary Survey*. (Report of a conference held at the University of Sheffield, 5-7 January 1969.) London: Methuen and Company; distrib. by Barnes and Noble, New York. 1971. Pp. 200. \$9.50.

With support from the Social Science Research Council, Professor Pollard has recruited a team of assistants to sift through public accounts and business records relating to every sector of the economy in order to measure capital formation in Britain during the Industrial Revolution. To achieve "better co-ordination among all the participants" and to ensure by "consensus rather than by fiat" that the results of the separate studies can ultimately be combined into a single series, a conference of work in progress was held at the University of Sheffield in January 1969. The six papers presented on that occasion together with edited summaries of the discussions form the contents of this volume.

In their introduction the editors conservatively assess the benefits derived from such workshop sessions as a form of quality control. The first two contributors discuss appropriate theoretical guidelines for the project; Mr. J. Hibbert reviews modern national accounting practices, and Dr. C. H. Feinstein describes his recently published estimates of gross capital formation for the period 1856-1913, to which Pollard's series will be related. The remaining papers contain accounts of research actually done or planned for different sectors of the economy. Most of Dr. S. D. Chapman's paper on investment in the cotton industry has already appeared in the *Economic History Re-*

view ("Fixed Capital Formation in the British Cotton Industry, 1770-1815," 23 [1970]: 235-66). Using insurance company records and the concept of the representative firm he concludes that "the cost of fixed capital investment in the initial period of 'take-off' in cotton was very modest." Because their research is less advanced the contributions of Mr. J. E. Ginarris on roads and canals, Mr. Robert Craig on shipping, and Dr. B. A. Holderness on agriculture center on the pitfalls of interpreting sources, the scarcity of evidence, and the consequent problems of constructing long-run time series. Most of the discussion concentrates on the difficulties of sampling, using proxy figures, allowing for obsolescence, constructing index numbers, and defining capital assets. And since the estimates of the dominant agricultural sector are expected to be "among the weakest and most doubtful series in the project," additional complications are bound to occur at a later stage in moving to larger aggregates. Yet despite such obstacles and notwithstanding the fact that housing and industries other than cotton receive only passing mention, these progress reports offer grounds for cautious optimism about the outcome, providing the team can agree on accounting categories and procedures appropriate to the evidence.

GORDON RIMMER

*University of New South Wales*

W. S. LEWIS *et al.*, editors. *Horace Walpole's Correspondence with Sir Horace Mann and Sir Horace Mann the Younger*. Volumes 9-11. (The Yale Edition of Horace Walpole's Correspondence, Volumes 25-27.) New Haven: Yale University Press. 1971. Pp. 685; lxii, 546; 547-1113. \$17.50 each.

With these three volumes the Yale edition of the correspondence between Horace Walpole and Sir Horace Mann is now complete. For good measure there are a few letters to Walpole from Mann's nephew, several appendixes, and finally the index to all their letters and the appendixes.

The availability of both sides of a correspondence, always desirable, is especially advantageous in this case, for the two principal correspondents were, geographically and in their immediate concerns, far removed from each other. Until his death in 1786 Mann re-

mained as diplomatic representative at the court of the grand duke of Tuscany. His view encompassed the affairs of Europe. Walpole observed the world from London or Strawberry Hill. Yet their mutual understanding and sympathy and their interest in each other's letters never failed.

The subjects of the later correspondence are essentially the same as earlier: politics, diplomacy, culture, friends, and society in the narrow sense. But times had changed. New topics absorbed the correspondents' attention. These were the closing years of the American war and the conflicts stemming from it. There was unrest at home and in other parts of the Empire. Walpole had long since abandoned his seat in Parliament and he went about less often, but his home in Berkeley Square was, in his words, "a little coffee-house" where his visitors brought the latest news, which he relayed to his friend in Florence (9:371).

With the war came additional responsibilities for Mann. Both correspondents longed for peace. Walpole frankly deplored the policies that had plunged England into war and ever heavier debt. He often recalled his father's motto, *Quieta non movere*. He distrusted the influence of the Crown and in 1782 hoped that changes in the administration would result in "the recovery of the constitution" (9:262). He saw no need for electoral or civil service reform.

The style of the correspondence changed somewhat. Both men grew philosophical. Walpole's letters are marked by an even greater wealth of anecdotes, historical and literary allusions, and a charming ability to poke fun at himself as well as others. Whom is he mocking when he writes: "At first I thought myself grown deaf when with young people, but perceived that I understood my contemporaries though they whispered" (9: 22)?

This edition permits posterity, whom Walpole never forgot, to appreciate more fully the historical value of the letters and to savor their unique flavor. Anyone interested in the eighteenth century must be grateful to Mr. W. S. Lewis and his associates who, as these three volumes with their wealth of notes and detailed index testify, have maintained unflaggingly the highest editorial standards.

DORA MAE CLARK  
*Wilson College*

RICHARD W. DAVIS. *Dissent in Politics, 1780-1830: The Political Life of William Smith, MP*. London: Epworth Press. 1971. Pp. xv, 268. £5.00.

William Smith (1736-1835) was member of Parliament, with some intervals, between 1784 and 1830 for Sudbury, Camelford, and Norwich; and from 1805 to 1832 he was chairman of the Protestant Dissenting Deputies. A wholesale grocer in the City of London, he lived in the days of his prosperity as an opulent gentleman, given to good causes. He was a friend and for a time a neighbor of the Clapham sect and worked with them against the slave trade and slavery. His education had been under Dissenting auspices, but he had sons at Cambridge. He became a theological liberal and joined the fashionable congregation started by Theophilus Lindsey in Essex Street. Dr. Davis says that he had the "catholicity of eighteenth-century dissent," rather than the sect spirit of nineteenth-century Nonconformity. From being a follower of Pitt, he became a disciple of Fox, as Dr. Davis shows, not because of difference about religious tests for public office but because of the French Revolution and the war. Like Fox, Smith was one of the friends of the people, not one of the people. His view of instructions by constituents to members of Parliament was anything but radical. Dr. Davis says that his attitude to the electors of Norwich was like that of a headmaster to unruly schoolboys. Smith, moreover, always preferred behind-the-scenes negotiation and management in the House to outside demagoguery and piecemeal progress—by accepting half-loaves—to losing one great campaign for a whole loaf unlikely of attainment. In this again Smith was at variance with some of his coreligionists. In the nineteenth century Aspland and Southwood Smith disliked Smith's eighteenth-century Whiggism. Smith entered into prominence in the agitations of the 1780s for the repeal of the test and corporation acts, and as chairman of the Dissenting Deputies had an important part in the repeal of 1828. Dr. Davis also gives interesting accounts of Smith's part in the opposition to Sidmouth's bill of 1811, in the repeal of the five mile and conventicle acts in 1812, and in the Unitarians' toleration act of 1813.

The Epworth Press is to be praised for putting the footnotes at the foot of each page and for permitting an ordered and useful bibliogra-

phy, easy to use. The index could be better. Dr. Davis would have found some useful material in the William Smith Papers in the Spencer Research Library of the University of Kansas.

This book, well produced, well documented, and readably written, supplements most usefully, and occasionally corrects, the work already done in this field by Barlow, Machin, Manning, and others.

R. W. GREAVES

University of Kansas

A. S. WOHL *et al.* *The History of Working-Class Housing: A Symposium*. Edited by STANLEY D. CHAPMAN. [Totowa, N. J.:] Rowman and Littlefield. 1971. Pp. 307. \$16.50.

This is a difficult book to review. It is in no sense a history of working-class housing in Great Britain. Rather it is a collection of essays of varying length, coverage, and focus, dealing with some aspects of working-class housing in London, Glasgow, Leeds, Nottingham, Liverpool, Birmingham, southeast Lancashire, and Ebbw Vale. These essays reflect the authors' interests and research pursuits, and with the exception of perhaps the last, on Ebbw Vale, they are of uniformly high quality; but, put together, they do not represent a balanced description of housing in Britain at any period, much less a history. In reality they are a collection of journal articles.

Certainly it is unfair to criticize a scholarly work for not being something else, but one could wish to have found rather more vigorous editorial supervision of this book. A reader, hoping to find an answer to some question of his own, may be disappointed. Even within the general topic of housing the choice of subjects is haphazard: some deal with the economic circumstances of housebuilding, some deal with working-class architecture, and one, Anthony Wohl's, traces the history of the growing public interest in lower-class living accommodations in London.

As a piece of historical scholarship, Wohl's article on the housing of the working classes in London between 1815 and 1914 has perhaps the greatest general interest. Although he deals almost exclusively with the last half of this period and is expanding upon his previous work concerning the impact of Andrew Mearns's pamphlet of 1883, *The Bitter Cry of Outcast*

London, he shows, with careful statistical detail, the exploding public interest in the domestic conditions of the laboring classes and makes clear again that, despite repeated expressions of concern and interest from politicians, government activity in the field before the First World War in fact amounted to nothing.

The other articles, although useful, are of such narrow scope that one would have expected to find them in county historical journals. The economics of lodginghouse keeping in Glasgow or the origins of back-to-back house-building in Leeds—deriving not from the search for economy but from the configuration of fields and footpaths—are of important but parochial interest. Perhaps the most unfortunate result of this excessive concentration, besides the breakdown in parallel coverage in the essays, is the practical absence of any reference to the more general national movements concerned with working-class housing. Octavia Hill is scarcely mentioned, nor are the Health of Towns Commission, the General Board of Health, the Medical Officers of Health, or the establishment of the Local Government Board. The entire topic of the growing interest in sanitary legislation, which affected housing more than any other field, is hardly noticed. Only Wohl, dealing with London, has much to say about urban transportation.

These remarks are criticisms only in the sense that a book of this sort, reflecting a large amount of careful work, might have become much more than it is. The impact of the Industrial Revolution on the ordinary people of England has grown in the last few years to be a major preoccupation of many historians and, within this, housing is a central topic. A medium-sized general history of nineteenth-century housing would be of great use. In such a volume the monographic work contained in the present book would be of significant value, but these studies themselves are not such a history. Indeed, they demonstrate how much remains to be done.

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University of Illinois,  
Chicago

JOHN HURT. *Education in Evolution: Church, State, Society and Popular Education, 1800-1870*. London: Rupert Hart-Davis. 1971. Pp. 286. £2.50.

Historians accept the importance of general elementary education in the formation of national states but neglect its history, while those who write on the subject too often are untrained in historical research. John Hurt is a political scientist, and he has credentials in education as well, which may explain why he has produced one of the few studies of elementary education in Britain before the Forster Act of 1870 that both historians and educators will approve.

The basic assumptions are uncontroversial. The parliamentary decision to support a voluntary system of elementary education for working-class children reflected a growing belief that a minimum of schooling under religious auspices was a better assurance of social stability than ignorance. The ultimate compromise of small matching grants to church schools, including nonconformist and Roman Catholic, which accepted inspection, reflected political realities. Leaving control with local school managers while London officials pressed for improvement along uniform lines through grants that might be reduced by adverse reports of inspectors was in the Victorian administrative tradition.

The special value of this study grows out of research in little-used official papers and minutes, including the reports of inspectors and examiners. These civil servants appear as an exceptionally well-educated and hard-working group, even though appointed under the old patronage system. R. R. W. Lingen, who succeeded Sir James Kay-Shuttleworth as secretary of the Education Department, is revealed as a significant influence even though he did not appear in the public eye. The arrangements by which schoolmasters for working-class children were drawn from the working class while inspectors and examiners came from Oxford and Cambridge are considered. The effort to raise standards through training colleges and the certification of teachers is described as well as the effect of "pupil-teachers"—in effect apprentices—who provided much cheap instruction. The circumstances under which women came to outnumber men as teachers are examined. Finally, much evidence is produced to support the author's belief that it was the development of the voluntary system before 1870 that made the Forster Act workable and fas-

tened on Britain the system of class education that lasted into the twentieth century.

FRANCIS H. HERRICK  
Mills College

NICHOLAS C. EDSALL. *The Anti-Poor Law Movement, 1834-44*. Totowa, N. J.: Rowman and Littlefield, 1971. Pp. viii, 285. \$12.00.

Mr. Edsall has a sound grasp of the complexities of political society in the North of England in his chosen period and a shrewd eye for the realities beneath the rhetoric of political conflict. He is an able and perceptive historian and writes clearly and succinctly; nevertheless his book is less satisfying in its conception than in its generally admirable execution.

The anti-Poor Law "movement" was scarcely an entity worthy of extended study in isolation. For a couple of years, in 1837-38, resistance to the introduction of the New Poor Law became the chief focus of radical discontent in the North, but it was an issue chosen by those already discontented. In the kaleidoscopic history of Northern radicalism its place between the campaign for factory regulation and for the charter is modest enough. Edsall's careful account shows that the agitation against the New Poor Law in 1837 gained its apparent strength through the coincidence of inept haste on the part of the commissioners (lulled in part by their equally accidental easy success in the south), of economic recession, and of a temporary confluence of various Northern suspicions of "London" government and its alleged machinations against traditional local rights. The propaganda against the New Poor Law—which Edsall rightly finds "very mediocre stuff"—had scarcely more relevance to the real problems of poor relief than had the inanities of Cobbett in 1834. It is entirely understandable that the 1834 act should provide fuel for the fire in radical bellies, but even in the North no one died to preserve the Old Poor Law. Edsall's analysis of local reactions to the commissioners and their aggressive assistants reveals the ultimate weakness of a movement that might paralyze Poor-Law affairs in Huddersfield for a few months but could muster only seventeen votes for Fielden's repeal motion at Westminster. It remains astonishing that so revolutionary a piece of legislation should meet so little active resistance.

In later chapters Edsall gives a brief account

of affairs in the Northern unions after 1838, when active resistance became passive, and old and new systems were alike found wanting in the face of industrial unemployment. He rightly remarks that the opposition "had no more idea than the Poor-Law commissioners of how to cope with really severe distress." Unfortunately his own assessment of the dilemmas inherent in the problem of providing a viable system of relief in England at this time is scarcely adequate either: his opening chapter on "The Debate on the New Poor Law" is—by his standards—superficial, and it is surely anachronistic and pointless to nominate a system of insurance as the "obvious alternative" to the New Poor Law that nobody had the wit or will to adopt. Even if so, why not? His useful account of the Northern opponents of the New Poor Law must remain only partially satisfying without a sharper sense of the significance of the innovations against which they were reacting. What were the commissioners in fact about, if not to poison paupers and destroy the social compact? Edsall shows signs of finding the champions of the new law rather more interesting than his ostensible objects of study, and it is a pity he did not widen his canvas further. Reality, not radicalism, defeated the ideologues of 1834 and taught the commission to compromise. This book is a good critical study of the critics, but what of the work itself?

J. R. POYNTER  
University of Melbourne

ROBERT STEWART. *The Politics of Protection: Lord Derby and the Protectionist Party, 1841-1852*. New York: Cambridge University Press, 1971. Pp. viii, 240. \$12.50.

W. D. Jones published his biography of the fourteenth earl of Derby in 1956, without access to Derby's papers. In the early 1960s, however, the papers came into Robert Blake's possession, where they still remain on loan from the present earl. Blake's *Disraeli* (1966) used only the Disraeli letters in the collection, and his *Ford Lectures* used a few of Derby's copied letters. In the meantime Blake permitted Robert Stewart to consult the papers for the years 1841-52—having himself begun to prepare the definitive biography of Derby.

Stewart's subtitle, *Lord Derby and the Protectionist Party, 1841-52*, is misleading: the book is less a biography of Derby in these years

than it is a detailed, chronological account of top-level party politics. Accordingly Peel tends to eclipse Derby, and later Derby shares the stage with Bentinck and Disraeli: at the end we still know Derby less well than we do the others. This is disappointing. At some point in the book, a thorough examination of Derby's personality and ideas would have been welcome. He was a puzzling, many-sided, fascinating man. As for his ideas Mr. Stewart at one point (p. 102) describes him as "intellectually a protectionist," at another (p. 220) remarks on his "deep attachment to the landed class." But nowhere is Derby's case for protection carefully analyzed, nor is his devotion to the countryside—beyond a partiality for bowling over rabbits—specifically demonstrated. Some account of the great Stanley estates in Lancashire where they bestrode both the worlds of corn and cotton would have been helpful: how much of the Stanley gross income by 1845 was nonagricultural? how much of it went back into agricultural improvements? what were the relations between the Stanleys and their tenants?

Generally Mr. Stewart does less well with the social and economic context of his political narrative than he does with the narrative itself. We are told, for example, that "there was a deep-seated repugnance to improvement among large landowners" (p. 39), which presumably helped to shape their protectionist outlook. But how many estate accounts have been examined in support of this statement? The unhappy answer is: very few. One might guess that further research will reveal that from 1830 onward large landowners were increasingly ready to pay the cost of estate improvements, keeping the nominal rental fairly stable but turning more and more of it back into the farming of the estate. After all, English farming was the best in the world. It is not likely to have got that way through neglect. Even a protectionist like Derby, far from being a slovenly farmer, was a conspicuous improver on a large scale before 1845.

It would be unfitting, however, to end this review on a querulous note. Mr. Stewart's chief object is to tell the story of the breakdown of Peel's party and of the ditherings and fumbings of what was in fact a new party, that of Disraeli, Bentinck, and Derby. The relations

among these three remarkable men are now clearly worked out once and for all. No one can now mistake the oddities of Derby's politics—nor the acute discomfort he suffered in restraining Bentinck so as not to frighten the Peelites whom he eventually decided were Radicals. Last but not least, Mr. Stewart has produced an attractively written book, surprisingly so, in view of its origins as a doctoral dissertation.

DAVID SPRING

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ROY DOUGLAS. *The History of the Liberal Party, 1895-1970*. With a foreword by JEREMY THORPE. Madison, N. J.: Fairleigh Dickinson University Press. 1971. Pp. xx, 331. \$15.00.

Dr. Roy Douglas has written a book that is as much a symptom as a chronicle of the Liberal party's distress. Like its subject, it staggers onward from general election to general election, losing steam and becoming more and more peripheral to the times. Replete with a foreword by Jeremy Thorpe, who leads a half dozen Liberal M.P.'s in the present House of Commons, the work is very much an exhortation to the party faithful to keep the faith. Perhaps not surprisingly, when it is most eloquent as a political tract it proves least satisfactory in its historical judgments.

Without making altogether clear the significance of the dates that frame his study the author attributes his party's repeated misfortunes not to Liberalism, but to Liberals. One may recall that G. K. Chesterton reached precisely the same conclusion early in the century. Curiously enough, those whom Douglas deems responsible for the "avoidable mistakes" and other inadvertencies that, time and again, nearly extinguished the party were not the official leaders—who get off very lightly indeed—but usually their parliamentary whips: T. E. Ellis died before he could reconcile the Liberal Imperialists to the leadership of Sir Henry Campbell-Bannerman; Herbert Gladstone, guilty of a "squeamish refusal to strangle the Labour Party in its cradle," made possible the emergence of a rival to the left; Percy Illingworth ate an oyster that proved fatal, thereby bringing to the whipship John Gulland, whose failure to mend the ensuing breach between H. H. Asquith and David Lloyd George was fatal in its



own right. In the years that followed, a succession of whips further embittered relations between feuding party leaders.

By emphasizing the strategic importance of the men who ran the party machine Douglas helps to restore a balance in Liberal historiography. But, at the same time, he manages the dubious feat of reducing Asquith and Lloyd George, among others, to two-dimensional figures. Lloyd George is blamed essentially for conveying an impression of instability and shiftiness: there is little attempt to evaluate the extent to which this reputation was justified. Asquith, more seriously, is faulted for his tenderheartedness toward Labour: in 1913 he piloted through the Trade Union Act, which allegedly filled Labour coffers; eleven years later he stepped aside ("Why he did so must remain a mystery") and allowed Ramsay MacDonald to form the first Labour government. This, "the most disastrous single action ever performed by a Liberal towards his Party," accelerated the drift from Liberalism, which, for all its intellectual dynamism, had forfeited the chance to be taken seriously as a political force.

Having once taken Lady Violet Bonham Carter to task (wrongly, he now concedes) for coquetting with the Tory enemy, Douglas criticizes Jo Grimond, Thorpe's predecessor, for attempting to come to terms with the Socialists. There can be no accommodation with other parties. Liberals of previous generations, by their indulgence toward Labour, served not the British worker, certainly not the cause of socialism, but only the Conservative party. It is an interesting thesis, but one that would require quite a different book to prove.

STEPHEN E. KOSS  
Barnard College

NEVILLE THOMPSON. *The Anti-Appeasers: Conservative Opposition to Appeasement in the 1930s*. New York: Oxford University Press, 1971. Pp. 256. \$7.25.

In most conventional histories of Great Britain in the 1930s, a special place is given those few members of the Conservative party who were alive to the dangers of nazism at an early date and who sought to alert the nation to the dangers represented by Hitler. The Conservative "opposition" is generally thought to have been led by men like Churchill, Eden, Amery, and

Macmillan. The band, while never large, is usually represented as having been rather deliberate in its efforts to correct the mistaken policies of Stanley Baldwin and Neville Chamberlain.

Not the least of the accomplishments of Neville Thompson's book is to cast doubt on this traditional portrayal of the period. Thompson demonstrates conclusively that antiappeasement developed late in the decade and that few of the so-called antiappeasers were either consistent or bold in their opposition. Neville Chamberlain, with the passage of time, may appear to have been a rather ineffectual leader. While he was in office there were very few in his party who were prepared to tangle with him. The insufficiencies of Stanley Baldwin are now amply known; this did not mean that Conservative party backbenchers saw the Rhineland issue with much greater clarity than he did. Churchill is supposed to have had a sleepless night when Eden resigned from the Foreign Office; Neville Thompson suggests that Eden was never the strong man some of his friends thought him to be and that even after his resignation he was most deliberate in his efforts to dissociate himself from any effort that included Churchill.

In short what Thompson has done is to remind us how difficult it was in the 1930s to be a member of the Conservative party with ambitions for office and a readiness to risk one's career by overtly opposing the prime minister. Moreover, the issues were sufficiently complex that any simple policy of antifascism or antiappeasement seemed excluded. There was massive confusion at the time of the Manchurian and Ethiopian crises. What certain political men were prepared to say in 1938 and 1939 they were not saying at an earlier date. It was only the fact that they were right in the end that made their friends believe that they had been right all along. This is an admirable book. No new textbook on the period can afford to ignore its findings.

STEPHEN R. GRAUBARD  
Brown University

F. S. L. LYONS. *Ireland since the Famine*. New York: Charles Scribner's Sons, 1971. Pp. xiii, 852. \$17.50.

Let us say at the outset that this new book by

Professor Lyons covering the period 1850 to the present should become an indispensable work of reference. Whether it is the last word as a general history of Ireland for the period it covers is a different question. Comparing Irish historians who attempt surveys of their country's history to the ancient Israelites condemned to "make bricks without straw," Professor Lyons indicates the many gaps that remain in Irish history despite the monographic work of the last thirty years. For the nineteenth century scholarly preoccupation with the making and breaking of the Union has meant that other themes, quite as important, have been neglected. Economic, social, and cultural, as well as administrative and constitutional history are waiting for deeper attention. To fill the gaps in the narrative Professor Lyons has had to do a considerable stretch of fresh work himself. Commenting on the paucity of materials for twentieth-century Irish history, he suggests that the last third of his book, called "The Partitioned Island," be regarded as a pioneering enterprise that will have served its purpose if it stimulates others to study more deeply the contemporary history of Ireland. It is, I think, fair to say that the result is a scholar's book, where the questions and problems are argued with unwavering impartiality. But some quality—the sense of a nation's life—does not emerge with the clarity one would wish for in a general history. Perhaps the book's very impartiality deprives it of focus. In the face of Professor Lyons's modesty and thoroughness one hesitates to criticize, but books, whatever their merits, are also useful and significant if they suggest questions that they do not themselves answer. Irish history has suffered from parochialism, and one remedy for this is more comparative history. To raise a few questions: Is Pearse's cult of blood and violence to be seen solely in an Irish context? *The New Cambridge Modern History*, which covers the period 1898–1945, is called *The Age of Violence*. Where and how does Ireland fit in? Also, and here the new psychoanalytic history might suggest lines of enquiry, what kinds of people become patriots of violence, beating their heads against the walls of empire everywhere? What of the problems of religious and ethnic majorities or minorities in European states—the "little Ulsters" of Central and Eastern Europe, to use a phrase of Sir Lewis Namier's? Both Eng-

lish and Irish history would benefit from these comparisons. As for Ireland itself, is the nature of its constitutional tradition sufficiently explored? Beginning as he does in 1850 Lyons deals only with the latter part of O'Connell's career, but O'Connell's searchings for the right relations between Britain and Ireland as well as his philosophy of nonviolence are worthy of more attention by way of introduction. As for Ulster, why after 1886, when opposition to even a limited Home Rule had become so clear, was so little attention given to conciliating the North and to structures that might have overcome problems rather than complicating them? Lyons's account of the six counties from 1920 is admirably thorough, but its very thoroughness suggests the need for a separate history that will focus more directly on the North from the inside as well as on the problem it is for both Britain and the republic.

But comments on general history and on perspectives must not detract from the work Lyons has done. All scholars will be grateful for the superb bibliography, the thorough and informative footnotes, and for the chapters that, in some instances, bring scattered knowledge together for the first time, filling gaps in cultural, social, and economic history. His chapter, "The Battle of Two Civilizations" is especially valuable—pulling together as it does so much of the thinking about Ireland from Thomas Davis to Patrick Pearse. Finally, for those who come fresh to the field, Lyons's work should be read in conjunction with Oliver MacDonagh's brief *Ireland* (1966), which because of its very brevity focuses sharply on so many significant questions. Another recent helpful work brings to the Northern Ireland question the perspectives of political science: Richard Rose, *Governing Without Consensus: An Irish Perspective* (1971). It opens up comparisons both with European and non-European states that have faced the intractable problems of different historical developments within confined boundaries and to which unitary nationalism seems to be no final answer.

HELEN F. MULVEY  
Connecticut College

C. THELLIEZ. *Marie de Luxembourg, duchesse douairière de Vendôme, comtesse douairière de St.-Pol, comtesse douairière d'Enghien, dame de La Fère, et son temps.* (Anciens pays et assem-

blées d'états, Number 52.) Louvain: Éditions Nauwelaerts. 1970. Pp. xx, 150. 360 fr. B.

Marie de Luxembourg (d. 1546) was called Mother of the Poor by some of her contemporaries and is named Mother of Kings by Cyrille Thelliez (Marie's great-grandchildren included Mary Stuart and Henry IV of France), but she remains an obscure figure. To give substance to this small book, Canon Thelliez has had to devote most of the text first to tracing Marie's ancestry and then to following through three or four generations the many progeny of her marriage to François de Bourbon-Vendôme (no genealogical table is provided). Even so, half of the pages are given to plates. His very informal bibliography and documentation do not inspire confidence, but it does seem likely that Thelliez, the diocesan archivist of Cambrai, simply had too few sources to construct a satisfying account of the early life and long widowhood of Marie de Luxembourg.

This is volume 52 in the series *Anciens pays et assemblées d'états* published by the Belgian section of the International Commission for the History of Assemblies of States. The series was launched in 1950 in order to reclaim the history of *pays* or provinces and their institutions, subjects generally neglected for the history of cities and nations. The series has been of mixed quality, with no apparent attempt even to maintain uniform editorial standards. Its purpose is modestly served in this volume, especially in the few pages on Marie's administration of her estates and in the plates showing scenes of Cambrai and La Fère.

JOHN C. MOORE  
Hofstra University

MYRIAM YARDENI, *La conscience nationale en France pendant les guerres de religion (1559-1598)*. (Publications de la Faculté des Lettres et Sciences humaines de Paris-Sorbonne. Series "Recherches," Number 59. Travaux du Centre de Recherches sur la Civilisation de l'Europe moderne, Number 8.) Louvain: Éditions Nauwelaerts. 1971. Pp. 392. 735 fr.

This ambitious study, which presents a conceptual synthesis whose elements are derived from a wide range of opinions, confirms the critical position of the sixteenth century in French history. The author first delineates the essentials of national consciousness at the time: (1) the kingdom is equal to the king in the *mystique*

of French unity (p. 92); (2) the French conceive of themselves as God's chosen people and have "extreme confidence in their own powers... to surmount all obstacles" (pp. 35, 42); (3) Gallicanism is central to the conception the French have of themselves (p. 39); (4) the linguistic criterion is important ("it is the language and not the fact of being the king's subject that makes a Frenchman," (pp. 47-48); (5) the determining factor is the leadership of the *parlementaires*, whose juridical conceptions dominate the national history they created, and to be familiar with this history is a patriotic duty (pp. 62-67).

The case is argued in twelve chapters whose titles show national consciousness developing through a virtually dialectical process involving the entire spectrum of thought from absolutism to constitutionalism. Chapter 1 ("Etat et tolérance, 1559-1572") interprets the policy of Catherine de' Medici and Hôpital as the search for a *modus vivendi sur le plan civique* between the rival factions (p. 92). Chapter 2 ("Positions catholiques, 1559-1584") shows how the difficulty for Catholics of separating the notion of "France" from Roman Catholicism led to the claim by some that "heretics are not Frenchmen" (p. 109). It is doubtful, however, that the statement "the new reality of France as a political entity independent of religion had not yet achieved even the least place in Catholic thinking" (p. 119) is accurate as late as 1580.

In chapter 3 ("Positions protestants, 1559-1572"), we are reminded that the resort to arms was justified as a patriotic duty to rescue the king and the country from the "foreign" Guises (pp. 126-27). The question of whether the nobles' argument that they have the special function of defending the kingdom can be construed as a manifestation of national consciousness is answered affirmatively, given sixteenth-century ideas of the "nation" (p. 128). I would agree, adding that the point needs further development to convince readers unfamiliar with the period.

St. Bartholomew marks a revolution (ch. 4) because the *liaison mystique* between the king and the people was broken for the first time. But the claim that national consciousness was stripped of the centuries-old ties to the Crown and was based "exclusively on concrete and real factors [*données*]" (p. 148), even temporary

ily, seems exaggerated. Chapters 5, 6, and 7 ("Politiques, raison d'état et patriotisme, 1572-1589"; "Le Patriotisme protestant, 1585-1589"; "Catholiques, ligueurs et la chute du Roi, 1585-1589") analyze the growing polarization of the country and the various positions assumed toward the relative priority of the Salic Law and the other "fundamental law," that the king must be Catholic, including nuances of thought within each religious camp. Yardeni's treatment of the League (chs. 8 and 9, "Propagande de la Ligue, 1589-1593" and "Le Débat sur l'ordre social, 1589-1595") follows the traditional *politique* line, but also includes discussion of the class issue, when the nobles ironically lost prestige just as they rallied to a Crown that was resuming its central place in national consciousness.

The accession of Henri IV was the clinching factor in the emergence of a new and strengthened national feeling in all classes and ideological groups (chs. 10-12, "La Lutte pour l'unité national, 1589-1593"; "La Naissance d'une légende, Henri IV et la France, 1589-1595"; and "Le Dénouement de la crise et les redditions, 1593-1595").

One wishes, however, that the author had either made some attempt to distinguish among *sentiment national*, *conscience nationale*, and *patriotisme* or frankly conceded that whatever the earlier divergences they became inextricable with the rising threat to the very existence of France as a nation—whatever that term may connote. Although at moments she seems about to be carried away—the euphoria arising from the documents is contagious—the author realizes that the snowballing *ralliement* tends to smother the few dissident voices and that "history is written by the survivors" (p. 298).

The author's concentration on her thesis sometimes distorts the emphasis of the sixteenth-century writers themselves, as is the case with Bodin and Beza, for instance, but her contention that all groups (some unwittingly) contributed to the refinement and redefinition of national consciousness during the civil wars is clearly demonstrated. The scholarly apparatus is excellent, and there are few typographical errors—for example, the siege of Rouen (p. 61) should be 1562, and *L'Apologie de la Paix*, 1585 (p. 175 n. 35). Probably no single reader will agree with every statement, but everyone inter-

ested in French "national character" should read this useful and provocative book, which makes the history of France in the last four hundred years (in some respects) a seamless web and enlarges our understanding of what General de Gaulle called *une certaine idée de la France*.

NANCY L. ROELKER  
Boston University

MICHAEL PRAWDIN. *Marie de Rohan, Duchesse de Chevreuse*. London: George Allen and Unwin. 1971. Pp. 200. £3.00.

This book reads well. Unfortunately there is not much more that can be said for it. It is better than Victor Cousin's century-old book, excepting the extensive appendix of the latter. But it comes nowhere near surpassing Louis Battifol's sixty-year-old study. Mr. Prawdin has made life easier for the reader by excising many of Battifol's details, but he has substituted a series of general statements about the France of the early seventeenth century that are, to say the least, outmoded.

Beyond doubt there is ample room for books that are sympathetic to the opponents of Richelieu. The great nobility of seventeenth-century France deserve closer study by historians. As has been realized for a long time, the study of the role of women in the early modern period is a rewarding field. But none of these topics, which form the base on which Mr. Prawdin's book is built, is adequately handled by the author.

The bibliography is almost nonexistent. Primary sources are mostly limited to inferior editions of memoirs and letters. Technical errors, such as confusing parlements with provincial estates, are too numerous to mention. Marie de Rohan deserves something more than a pleasant tale.

J. MICHAEL HAYDEN  
University of Saskatchewan,  
Saskatoon

ROGER HAHN. *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666-1803*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xiv, 433. \$12.00.

Throughout the Enlightenment the Royal

Academy of Sciences in Paris dominated the world of science to a degree that no other national institution has since done or had ever done. Professor Hahn has now devoted to it the most sophisticated history of a scientific institution to have been written since the famous *Geschichte der königlich preussischen Akademie der Wissenschaften zu Berlin* (1900). Unlike Harnack, who, in the classic tradition of *Ideengeschichte*, organized his account around the scientific and scholarly work produced in the Berlin Academy, Hahn writes in the current mode of social history and tells of the Paris Academy relative to the society around it rather than to the content of science, of which he says little. In retrospect it seems natural that the academy should have embodied in miniature the structural characteristics of the regime that created it: monarchical, hierarchical, prescriptive, and privileged. Hahn is at his best on the process by which the original project for a Baconian body, collaborating selflessly for the advancement of knowledge, became a forum for the play of individual ambition, where the goal was status in a tribunal ruling the affairs of science through control of publication, patronage, and the technical policies of government. He analyzes the failure of the academy to accommodate civic, educational, literary, and industrial aspirations in the latter years of the Old Regime; traces its suppression at the hands of the Jacobin Convention to its inability to adjust itself to radical political change in the Revolution; and exhibits the combination of more specialized professionalism with bourgeois belief in a meritocracy in the reorganization of science under the Institut de France in 1795.

All these matters are admirably worked out, and only in one respect might it be thought that the exclusion of science itself deprives Hahn of the opportunity to be fully convincing about a point equally important for the social and for the intellectual history of science. The point is that the pre-eminence of French science in the eighteenth century was almost certainly a function of the selectivity of the Paris Academy and its consequent ability to enforce standards. For lack of that authority the Royal Society degenerated into amateurism. The sociological discourse with which Hahn concludes his book does not seem to me to

work very well. Never mind that, however. Like many another history, this one is better than its theoretical argument, and no historian working in this field can feel anything but gratitude and admiration for the ingenuity and generosity with which the bibliographical apparatus gives access to the literature.

CHARLES C. GILLISPIE  
Princeton University

HENRY J. MERRY. *Montesquieu's System of Natural Government*. West Lafayette, Ind.: Purdue University Studies. 1970. Pp. xvi, 414. \$8.50.

Marx once said that Sir James Steuart had a deeper historical and a generally more sophisticated social outlook than most eighteenth-century economists because he was an aristocrat. The same remark could be made with even greater justice about Montesquieu. A sense of history is, indeed, often grafted upon the psyche of the nobility, and in Montesquieu's case it gave rise to a profound understanding of political psychology and sociology. He has not always received his due, and it is only in recent years that his originality and profundity are gaining the attention they deserve. Mr. Merry's book is a contribution to this task. It shows that the American tendency to concentrate on Montesquieu's constitutional theory, with its liberal doctrine of the separation of powers, slights the many far more interesting aspects of his thought. The central conflict for Montesquieu, according to Mr. Merry, was not the usual one between the individual and the state, but that between civil society and the political apparatus of the modern state. His second great concern was the dangers of religion. Mr. Merry says rather little about that, but he does show how the exclusion of the supernatural led Montesquieu to emphasize natural causes in his analysis of social formations. Wisely, Mr. Merry reminds us also that this did not make Montesquieu forget the unique character and importance of "moral causes." It is these, as they manifest themselves in the civil sphere, that Montesquieu set out to picture in all their complex interrelationships. Law, education, commerce, all received a detailed examination. Nor does Mr. Merry forget the radical cutting edge of Montesquieu's thought, especially in his ideas on criminal punishment.

Though there is little effort made to put

Montesquieu in any sort of setting, social or intellectual, his influence on Hegel is very capably explained. There is some mention of the influence of the Roman historians upon Montesquieu, but not enough; and his deeper debt to Aristotle, a debt he and Hegel shared, is not brought out at all. Nothing much is said about the contemporary climate of ideas. The new psychology, the general spirit of libertinage and skepticism, and the reappraisals of the past are not discussed. Lastly, little is said about Montesquieu's historical theory and its bearing upon Europe's past and present.

This is, in short, an adequate work, but it has no audience in mind at all. It is neither original nor scholarly enough to interest serious students of the Enlightenment or of political theory in general. For the more elementary student it is too incoherent and selective. It has neither a central theme nor a set of worked-out interpretive lines of argument. Mr. Merry has obviously read Montesquieu thoroughly and has enjoyed it. One finds many sound remarks in his commentary, but it does not add up to a complete study of its subject.

JUDITH N. SHKLAR  
Harvard University

THOMAS F. SHEPPARD. *Lourmarin in the Eighteenth Century: A Study of a French Village*. (The Johns Hopkins University Studies in Historical and Political Science, 88th Series (1970), Number 2.) Baltimore: Johns Hopkins Press. 1971. Pp. xiv, 248. \$11.00.

The themes of continuity, stability, and responsible leadership provided by pragmatic and humanitarian notables run throughout this study of eighteenth-century Lourmarin, a village of about 1,500 inhabitants located twenty miles north of Aix-en-Provence. There was little social or religious conflict, despite the fact that eighty per cent of the inhabitants were of Protestant background, and there were only two major crises—the plague of 1720–22 and the Revolution. Professor Sheppard's monograph compares favorably with many of the local and provincial studies undertaken in France. The author draws upon the disciplines of demography, sociology, and economics, and he uses municipal, departmental, and notarial archives. About forty per cent of his citations refer to the minutes of the village council's de-

liberations, which constitute the basis for his highly favorable appraisal of the village leaders. Separate chapters are devoted to the land, the people, government, finance, poor relief, the seigneur, and religion. The concluding chapter discusses the effects of the Revolution on the village. Twenty-six tables and five appendixes supplement the text.

Among the particularly important chapters is the one on "The People." Using techniques of historical demography and family reconstruction, the author analyzes population trends and mortality rates, calculates the frequency of illegitimate births, and argues that birth control, probably in the form of coitus interruptus, was practiced in Lourmarin before the Revolution. This chapter is stronger on statistics than it is on such subjects as the villagers' diet, dress, attitudes, and social life. The chapters on government and finance offer a clear picture of the socioeconomic background of the village officials, and they describe in detail the community's financial responsibilities and sources of revenue. However, the limited attention that the author gives to the importance of intendants and subdelegates in village government weakens his argument about increased centralization during the century.

Sheppard is more convincing about the limited effects of the Revolution on Lourmarin. Although the villagers "quickly adopted the verbiage and forms of the Revolution" (p. 208), they were only slightly affected by revolutionary laws concerning religion and property and by changes of regimes. This book is an important addition to the growing number of local and provincial studies that will provide a more balanced evaluation of the Old Regime and of the effects of the Revolution on France.

THADD E. HALL  
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Binghamton

THADD E. HALL. *France and the Eighteenth-Century Corsican Question*. New York: New York University Press. 1971. Pp. xxx, 255. \$10.00.

The eighteenth-century Corsican question is a likely topic for a monograph, since the topic has not been investigated by many scholars and yet illuminates worthwhile aspects of the period. The grand theme of balance of power

politics in the Mediterranean sphere, the struggle of a faction-torn but determined people against a hated state (Corsica versus Genoa), and the formulation of policy by a major power involved in both themes (France)—these are dramatic focal points vying for the attention of the author and his audience. Professor Hall has the linguistic, archival, and detective skills to uncover these themes, and he alludes to the broader situation throughout. Yet he has missed an opportunity to turn his capable study into a major work by focusing his attention on the chronology of French responses to the Corsican revolt against Genoa, 1729–68. Hall makes his point that there was no long-term, secret French plan to seize Corsica, and in the process he shows how even Genoese mis-handling of the Corsicans and the danger of an English occupation failed to turn the French from a cautious and complex diplomatic-military handling of Genoese-Corsican relations until French sovereignty in the island loomed as the only solution. What I miss in all this is the precise connection with the European and Italian situations. The Wars of the Polish and the Austrian Succession, the Seven Years' War, and the changing nature and titles of important Italian powers like Piedmont-Sardinia are somehow lost in the details of French diplomatic correspondence. Eighteenth-century French court politics is another neglected subject. Since Professor Hall knows all the relevant persons from Choiseul down, as well as all the diplomatic, military, and political agents sent from Versailles to Genoa and Corsica, it does not seem sufficient simply to pass names in review, alluding to an official only when he happened to say something and alternating between such individual pronouncements and the general category "Versailles." Skillful editorial work could have helped immeasurably here by identifying individuals more fully (rather than only at first mention, and then incompletely), just as better editing could have made the wars, the alleged Genoese "misgovernment," and the Corsicans' own squabbles more integrated and fuller parts of the book. The author's fine tangential work on French rule in Corsica after 1768, "Thought and Practice of Enlightenment Government in French Corsica" (*AHR*, 74 [1968–69]: 880–905), that the broad questions attract his attention as well as

those that demand the technical skills demonstrated in this monograph.

A. LLOYD MOOTE

University of Southern California

WALTER MARKOV. *Exkurse zu Jacques Roux*. (Abhandlungen der Deutschen Akademie der Wissenschaften zu Berlin, Jahrgang 1970.) Berlin: Akademie-Verlag. 1970. Pp. xiv, 371. DM 72.

Historians in the German Democratic Republic have for some time been producing able studies on various aspects of the popular movement in France and Germany during the Revolutionary era. Hedwig Voegt's book on the radical publicist Rebmann, the work on the peasants and townsmen of Electoral Saxony by Percy Stulz and Alfred Opitz, and Heinrich Scheel's recent book on the South German Jacobins reflect the growing scholarly interest in those revolutionary militants who tried to voice the demands of the common people.

Long a recognized expert on the popular movement, Professor Walter Markov has already produced three volumes devoted to the life and works of the radical cleric Jacques Roux. The first volume examines the historiography of the *enragé* movement in general and Roux's particular place in it. The second is a biography of Roux covering his formative years and tracing his political career up to the point where he began to challenge Robespierre's leadership and the Jacobin definition of the Revolution. The third book presents an extensive collection of Roux's speeches, letters, and articles from *Le Publiciste de la République française* (1793).

In his fourth volume Professor Markov seeks to expand and explain the wide range of materials available for studies of Roux and his political activity. The book, organized into sixty chapters, covers all aspects of the red abbé's life. Markov devotes sections to Roux's family background, education, early career, role in the early phases of the Revolution, development as a radical spokesman, the growing conflict with the Mountain, and his final arrest, trial, and death. Markov also supplies detailed information on individuals and groups, some of them quite obscure, with whom Roux came into contact at important periods in his life.

In each chapter Professor Markov offers extensive bibliographical information. He em-

plays a wide range of unpublished sources including documents from the National Archives, departmental archives, and Parisian police reports. Printed sources include excerpts from local journals, the Parisian radical press, the *Moniteur*, and the *Archives parlementaires*. He also provides an impressive list of secondary works on the French popular revolutionaries.

Although there seems to be a lack of continuity between the chapters and much seemingly peripheral material unrelated to broader political, social, and economic trends, it must be kept in mind that the author is not seeking to write a full history of the Revolution or the popular movement. Moreover, Markov has succeeded in illuminating many aspects of Roux's life, and the wealth of bibliographical detail will be a valuable aid to those students and scholars who wish to investigate the complex story of the *enragés* and their leaders.

STEVEN T. ROSS  
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Austin

MAURICE AGULHON. *La vie sociale en Provence intérieure au lendemain de la Révolution*. (Bibliothèque d'histoire révolutionnaire, Third Series, Number 12.) Paris: Société des Études Robespierriennes; distrib. by Librairie Clavreuil, Paris. 1970. Pp. viii, 531.

With this book Professor Agulhon completes the publication of his excellent doctoral thesis. Thanks to him Var must be the most elaborately studied department of France between the 1790s and 1851, and this volume, although the last to appear, deals with the earlier time period. Its title is somewhat too restrictive; half of the book concentrates on prerevolutionary society. On the other hand, its geographic limits of study are not those of all the interior of Provence but mainly that part which became the department of Var. What is lacking for this area is the revolutionary decade.

M. Agulhon is mainly concerned with structure rather than process. Postrevolutionary society hardly differed from its previous form, and this was especially true in regard to social structure, communal organization, and economic activity—the three aspects of Var that most attract his interest. In his descriptive

analysis of them he is at his best, and his best is of a very high order. His minute study of social stratification is based on an exhaustive investigation of printed and archival sources. Although he is not as strong when dealing with styles of life among the strata, he provides us with a skillfully drawn picture of Provençal society at two critical periods. There is also an equally elaborate description of social role and power relations among the strata within several towns and villages. It was the village, he feels, with its concentrated population, hierarchical structure, and urban characteristics, which gave a special quality to Provence. Although the local economy was chiefly agricultural with an adjunct of artisan industry, there was not only a society as complex as that of a true town, but there was also a fairly well-defined and intricate class struggle between the commercial and industrial stratum and the large landowning classes, as well as between the nearly impoverished agrarian workers and the rich, whether mercantile or agricultural. Sporadic acts of violence were therefore part of a larger struggle—for more power, on the part of merchants; for more land and economic rights, on the part of village workers.

This argument is persuasive. It would be even more so if M. Agulhon had found a more effective means of presenting his data, especially his quantitative data. There are too few tables to summarize the immense detail, and in the section on landed property the text refers by number to tables that are not numbered. The weakness of graphic presentation is, happily, compensated for by his maps, but even here the value of maps 3 to 7 would have been enhanced by the use of place names.

These are relatively minor faults in a big book that is an important contribution to the growing volume of local history. It is contributions of this depth and merit that will soon make possible some new interpretations of French national history.

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THEODORE ZELDIN, editor. *Conflicts in French Society: Anticlericalism, Education and Morals in the Nineteenth Century. Essays*. (St. Antony's College, Oxford, Publications, Number 1.) Lon-



don: George Allen and Unwin; distrib. by Humanities Press, New York. 1970. Pp. 236. \$6.50.

The great merit of Theodore Zeldin's book is to make concrete the psychological, sociological, political, human, and local tensions that sustained and made inevitable in nineteenth-century France the struggles between curé and *instituteur*, bishop and prefect, municipal council and parish. Zeldin in his examination of "The Conflict of Moralities" finds that both the curé and his secular antagonist alike sought a morality of prudence and pudency. They both desired a world of conventional habit; a place free of sin and deviation. On different paths the confessor and his critic were seeking a society protected from disturbances that challenged the wisdom of their separate sciences.

Robert Anderson's analysis of "The Conflict in Education" during his Second Empire concludes that the Falloux Law had to fail. The supporters of the University and those who trusted in the superior merits of Catholic secondary education fought to monopolize the educational control of the few privileged heirs of an inequitable society. Secular and clerical educators shared a common preference for the classics and similar views of pedagogy and discipline. The heart of their quarrel was their belief that only one of them could best serve a bourgeois world.

The ambivalence of moralists and educators was not experienced by those who possessed power. Austin Gough's discussion of "The Conflict in Politics" as that battle was waged by Monseigneur Louis-Edward Pie, the bishop of Poitiers from 1849 to 1880, makes clear how intransigent and destructive of the interests of those he served a bishop could be. Bishop Pie adamantly and unsuccessfully attempted to crush all who challenged his theology, his pastoral counsel, his politics. Gough convincingly demonstrates that Pie's struggles against the prefect and other secular authorities in his diocese alienated and aroused the repugnance of all prudent men. His conduct assured the contempt of all who desired some harmony and civil peace for France.

Malformed giants capable of the harm Bishop Pie inflicted on his people did not exist at the local level of French society. But Roger Magraw in his consideration of "The Conflict

in the Villages," by concentrating on the communal strife in Isère during the Second Empire, demonstrates that smaller men could injure one another as mortally as did their grand superiors. Magraw's essay is a subtle and beautifully documented account of the legendary struggle between curé and mayor in the communes of France. This human, petty, and daily conflict has nowhere been better described or understood. The rich, original, and extensive archival documentation of the four essays in this slim volume makes it indispensable for all who would understand the social history of France in the nineteenth century. The historian is encouraged by these essays to shift his attention from the philosophical differences between believers and unbelievers to the social encounters and conflicts that touched the unborn, the infant, school children, young lovers, husbands and wives, the dead and the dying.

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ALISTAIR HORNE. *The Terrible Year: The Paris Commune, 1871*. New York: Viking Press. 1971. Pp. 172. \$12.50.

ÉDITH THOMAS. *Louise Michel: Ou la Velléda de l'anarchie*. (Leurs figures.) [Paris:] Gallimard. 1971. Pp. 475. 33 frs.

"At first sight," confides Mr. Horne, "the Commune might seem a somber subject for a coffee table book." He is, of course, correct. Nevertheless, for the centenary he condensed and rewrote chapters from his successful *The Fall of Paris* (1965), to which Richard Garnett then ably matched illustrations drawn principally from English sources. The result is an attractive and appealing volume—*sic transit* the Commune.

It remains to the author's credit that he writes well and respects historical fact. Hence, while the capitalist system exacts its price in the format this professional writer has chosen, *The Terrible Year* tells a more accurate story than many books on the Commune derived from party doctrine. But one should recognize that a certain distortion is part of the price. Strive for relevancy and interest, compress an already popularized text, and the nuances that define reality disappear. Under such pressures an analysis of the Commune's failure to seize

the Bank of France becomes an amusing instance of personal intimidation, and a brief portrait of Charles Delescluze determines the Commune's character in its final period. What detailed appreciation of Parisian republican sentiment on March 18 could survive such a chapter heading as "Red Coup at Montmartre"?

Édith Thomas's biography of Louise Michel admirably demonstrates that, within a different format, the professional writer can contribute to scholarship. A graduate of the Ecole des Chartes, the late Mlle. Thomas held an advantage over Mr. Horne. As a librarian at the Archives Nationales she enjoyed ready access to the mound of documents with which France memorializes its politically suspect. Archival press clippings and police reports, supplemented by material from other depositories, enable the reader to follow closely the anarchist trail of the black-draped, mournful Commune who preached revolt—and occasionally practiced it—long after the Commune's demise. It is clearly the finest study available on this enigmatic saint of the Left, a well-documented narrative graced with bibliography, an index to proper names, and several appended, though poorly selected, illustrations. Here again, however, reservations follow from the writer's choice of format.

Although Édith Thomas drew extensively upon archival sources her purpose and perspective were those of the biographer rather than the historian. The author neglects secondary literature as she lets Louise Michel tell much of her own story up through the fall of the Commune. This dependence upon Michel's *Mémoires* and other writings appears uncritical in view of Mlle. Thomas's recognition that veracity was a weaker virtue of "la bonne Louise." While the treatment of her heroine's later years proves that the "red virgin" was more anarchist than red, and more probably a virgin than not, the historian would have welcomed further enlightenment regarding Michel's relations with the French Left. But for an avowed Marxist, Édith Thomas seems partial to the cult of personality and reluctant to discuss the implications for either socialism or society of a message that even Marxists found impractical and simplistic.

Future historians and biographers trained in

psychology will undoubtedly have the last word concerning Louise Michel. How could they resist this ugly, illegitimate woman whose poems, prose, and heroic life expressed a passion for mankind, mother, and cats, but an obsession with blood, fire, and the cataclysm of world revolution. Édith Thomas hints at an analysis of Michel's ambiguous personality, though she hesitates to go further. Perhaps this is simply an intelligent biographer's prudent refusal to venture beyond her competence. Even so, the reader senses the author's relief at having spared from analytical assault the revolutionary mystique of her priestess of liberty, the *ex-pétroleuse* who faithfully tended what Mr. Horne cites as "that little flame which never dies."

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GUY ROSSI-LANDI. *La drôle de guerre: La vie politique en France, 2 septembre 1939-10 mai 1940*. Preface by RENÉ RÉMOND. (Fondation nationale des sciences politiques. Travaux et recherches de science politique, Number 14.) Paris: Armand Colin. 1971. Pp. ix, 247. 37 fr.

Utilizing a familiar title, Guy Rossi-Landi has written a thorough and perceptive study of the political and parliamentary confusions that characterized France during the first eight months of World War II. Although the causes of military defeat in 1940 are not ignored, they remain an issue subsidiary to the author's objective of clarifying the political scene. The thesis advanced is that the wartime Vichy governmental experiment can be explained by a history of the Third Republic but that the last months of the republic cannot be understood or explained by Vichy. Subthemes explore the role of the Communists, charges of rightist plots to destroy the republic, and accusations of pacifism directed against the Left before and during the Phony War. Rossi-Landi concludes that most of the blame for a military disaster in 1940 falls on French military leaders; the political contribution to defeat was indecision and the inability of the Daladier and Reynaud ministries to disavow their familiar world of parliamentary crisis in time to face a national crisis.

*La drôle de guerre* has most of the merits and some of the defects of its original form as a

doctoral thesis. The usual research materials on the period have been fully utilized and skillfully supplemented by personal interviews with some three dozen political veterans. The appendixes are a useful reference tool with a detailed political chronology and the vote of every deputy on six key parliamentary issues. The reader is well advised, however, to have a thorough background in twentieth-century French politics, for a steady stream of minor political figures rush through the book's two hundred pages only to disappear into the obscurity that marked their political careers. Among the major politicians, Édouard Daladier most clearly reflects the unhappy meeting in 1939-40 between tumultuous events and indecisive leaders.

The number of books by scholars and popularizers on the critical months before the German invasion of France in 1940 grows steadily. Rossi-Landi has made a solid contribution to that body of literature. But if his study is not the last word on the politics of the dying Third Republic, one hopes that it will be the last with the same not so *drôle* title.

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ANTHONY HARTLEY. *Gaullism: The Rise and Fall of a Political Movement*. New York: Outerbridge and Dienstfrey; distrib. by E. P. Dutton and Company, New York. 1971. Pp. xv, 373. \$10.00.

FRANK L. WILSON. *The French Democratic Left, 1963-1969: Toward a Modern Party System*. Stanford: Stanford University Press. 1971. Pp. 258. \$8.95.

These two very different works are studies in political failure. In approach and methodology they have little in common. Yet they complement one another in throwing light on the very recent past. They tend to show that it is still true that "the new France," however transformed economically and socially since 1940, is far from having found a moderate democratic politics through which to express its aspirations and resolve its conflicts.

Anthony Hartley's well-written history of Charles de Gaulle and his politics is based on wide reading in the general literature. Disclaiming originality he nevertheless has the advantage of being able to survey the whole ca-

reer and make some estimate of the immediate legacy at least. His picture of de Gaulle is that of a man whose basic thought had crystallized long before he emerged from obscurity (allowing, all the same, that the general altered his views on the colonial problem considerably after 1946). To this rather rigid and Manichaean thought was added the guile and political skill learned in the great adventure of the war. "By 1946 it can be said that the main elements of what Gaullism was later to be under the Fifth Republic had been assembled, though the proportions in which they were to be present at any one time were to change considerably over the years." The RPF experience and the years of exile sharpened his cunning before the return to power in 1958. By 1962 this "prince of equivocation" (the phrase is Robert Buron's) finally brought the Algerian war to an end, but not without ending also the ambiguity of the presidential role, revealing that in the new circumstances of the Evian peace he intended to maintain his exceptional powers, to be not the "arbitrator" but the "guide" of the nation, and to find his support in popular election, in plebiscites, and, however reluctantly, in the leadership of a great electoral party. Ultimately of course the nation tired of him. The gap between foreign policy objectives and achievements widened, the proposals for domestic reform ran into massive opposition, and the common taint of scandal sullied the movement. What survived the wreck of the May 1968 events was in fact this large conservative party, increasingly remote from the original Gaullist mystique, devoted to "bread and butter politics," as Hartley says. In sum, he sees "the Gaullism of de Gaulle" as having been no more than "an episode" and judges it good that it will remain only as "a current of intellectual intransigence." The lingering moroseness of the *durs des durs* may be viewed as the highest compliment to the permanence and flexibility of the Fifth Republic's institutions, freed at last from the personality that shaped them.

It would be something quite different, however, to suggest that the politics of the Right has been reformed. Hartley alludes to "exemplary drama" yielding now to "the moneyed radicalism of the Third Republic." It is precisely France's failure to have the politics of

her modernized institutions that concerns Frank Wilson. His is a clear and thoughtful book on the collapse of the attempt of the non-Communist Left to break with the traditionalist, fragmented tyrannies of the past. Whatever the possibilities for the UDR to become a great modern "catchall party" (Otto Kirchheimer's notion, which Wilson adopts and considers to be both the prevailing model in West Europe and the real hope for France), the democratic Left evidently destroyed its chances to do as much. So unyielding in their parochial natures were the Socialist and Radical parties, so powerful was the pull of ideology, so jealous were the relationships between rival leaders, so intransigent was the position of communism and so serious its appeal, that the hopeful phenomenon of the political clubs (which emerged in the 1960s to renew the old parties) found itself swallowed up in the rivalries and doctrinaire inflexibilities of those it sought to reform. This is spelled out at length in illuminating chapters on the abortive presidential campaign of Gaston Defferre in 1965 and the fate of François Mitterrand's *Fédération de la Gauche Démocrate et Socialiste*, which, unlike the mayor of Marseilles, took the road to leftist unity by way of the shrines of the past and so naturally succumbed to the old priesthood whose disastrous archetypal figure is Guy Mollet. On this reading, Mitterrand's fundamental error was to seek to remake the Left by uniting old factions, observing ideological imperatives, and rejecting "the new institutions, techniques and styles" of the Fifth Republic. The consequence was defeat in the 1968 elections and, in Wilson's judgment, a non-Communist Left more divided than before the general's return in 1958. The Left had failed to adapt to the changed social and economic situation of postwar France and Europe; its ineffectiveness was made more profound by refusal to accept political change as well. All this is admirably laid out here. Wilson's conclusions follow of course from acceptance of the Kirchheimer "catchall party" model as being the most valid for the reshaping of French politics. He makes a convincing case in his useful, concise, and agreeably written study of an instructive episode.

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C. G. AMINOFF. *Nyuppsatta truppförband i Finland 1770-1808: Administrativ historia och personal* [The Newly Established Military Units in Finland 1770-1808: Administrative History and the Personnel]. (Historiallisia Tutkimuksia, Number 82.) Helsingfors: Suomen Historiallinen Seura. 1971. Pp. 319.

This study examines the administrative organization and the social composition of the officer and noncommissioned officer corps of the new military units established in Finland during the last forty years of Swedish rule. There exists a fairly extensive literature on the eighteenth-century military history of Finland, but C. G. Aminoff has broken some new ground and his work complements, in particular, Kaarlo Wirilander's studies of the military hierarchy. He makes some interesting observations concerning the changing distribution of officer vacancies between members of the nobility and the non-nobles, on different age groups, and on the nature and extent of nepotism. It appears, though, that the rather extensive data he has diligently assembled would have allowed for a more systematic analysis of these matters. Such an analysis and a concluding chapter, tying together several threads that remain disconnected, would have enhanced the value of the work. Its present value lies mainly in the sketches on military organization and in the compiled data for social historians.

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VOLKER PRESS. *Calvinismus und Territorialstaat: Regierung und Zentralbehörden der Kurpfalz 1559-1619*. (Kieler historische Studien, Number 7.) Stuttgart: Ernst Klett Verlag. 1970. Pp. 543. DM 84.

Based on a dissertation of 1966 this study relates in great detail the political and religious institutions of the Palatinate in the sixteenth century and the activities of the rulers and the court officials who gave life and meaning to the offices they held. The author thus extends the earlier works of Max Steinmetz, Gretl Vogelgesang, and Henry J. Cohn on the government of the Palatinate in the fifteenth and early sixteenth centuries. Manuscript materials from family and state archives were extensively used in the collection of numerous details about the

administration and policies of the Palatinate during the period 1508-1619. Primary attention is given to the administrations between the Religious Peace of Augsburg (1555) and the outbreak of the Thirty Years' War, catastrophic for the Palatinate, for it was during this period that the electorate played an important political and religious role in German and European affairs.

The personalities and functions of scores of secular and church officials and the groupings and factions in the administration are described. The impact on the rulers and policies of such officials, ranging from the Grosshofmeister, the chancellors, marshals, and justices to appraisers and customs clerks, is fully told. The author plans to extend the research for the present work and publish an alphabetical-prosopographical catalog of all officials in the central administration of the Palatinate during the period 1544-1622.

With the changing religious scene, which of course not only had political overtones but also meant changes in officials, these were exciting and important years in the Palatinate. Under the reigns of Ludwig V and his brother, Friedrich II, the country, and therefore also the officials, gradually became Lutheran with reforms instituted in church and university administration. At first this form of Protestantism was quite moderate, but in the reign of Ottheinrich (1556-59) a bitter controversy arose at the University of Heidelberg where Tilemann Heshusen, general-superintendent, professor of theology, and member of the consistory, pushed for a rigorous Lutheranism. Disturbed by this quarrel the next elector, Friedrich III, led the territory to the adoption of a Calvinism in which the spirit of Melancthon was noticeable. The ruler strengthened his own personal inclination to Calvinism by calling to Heidelberg the young scholars Zacharius Ursinus and Kaspar Olevian. These men worked with the elector in producing in 1563 the important and widely adopted Heidelberg Catechism.

Since Calvinism was not one of the official religions authorized by the Peace of Augsburg, the publication of the catechism and the growing Calvinism of the Palatinate created an issue in the Empire. However, Friedrich withstood the pressures exerted upon him and proceeded to introduce a firm church discipline.

The opposing views of the theologian and physician Thomas Erastus, who favored a system in which the civil government exercised church discipline, were later to give rise to the term "Erastianism." When Ludwig VI succeeded his father in 1576, a Lutheran reaction occurred and the Calvinistic professors, preachers, and officials were expelled—to spread their Calvinism elsewhere. A similar change-over of officials occurred after the accession of Johann Casimir in 1583 when the Reformed religion was restored.

A bibliography of unpublished and printed sources and an extensive index of persons and places complete this lengthy and interesting study of the officials, many of whom had been previously little known, who worked with and influenced the policies of the rulers of the Palatinate during an important period in the history of this state.

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FRITZ WOLFF, editor, with the assistance of HILDBURG SCHMIDT-VON ESSEN. *Acta Pacis Westphalicae*. Series III-A, *Protokolle*. Volume 4, *Die Beratungen der katholischen Stände*. Part 1, 1645-1647. Münster: Aschendorffsche Verlagsbuchhandlung. 1970. Pp. lxxvii, 585. DM 112.

In 1957 a number of German historians founded the Vereinigung zur Erforschung der Neueren Geschichte to facilitate the publication of documentary material and studies dealing with sixteenth- to twentieth-century history. As one of its major projects this society decided on the publication, under the general editorship of Max Braubach and Konrad Repgen, of documentary material of the Peace of Westphalia. At first sight it might appear surprising that so much money, effort, and talent would be devoted to the publication of purely political material of the seventeenth century—an undertaking more characteristic of nineteenth-century historiography than modern historical interests. But there is no doubt that the Peace of Westphalia not only profoundly influenced the European state system and the future course of German history; it was also the first example of a major European peace conference. Older incomplete collections of documentary material, published in the eighteenth century, are hard to get and, in any case,

do not meet modern scholarly standards. A modern and complete edition of the relevant material will prove a boon to seventeenth-century historians. The publication is divided into three series, each of which will have several volumes: first, instructions; second, correspondence; third, transcripts, diaries, and documents. The present volume, the fifth that has appeared so far, is devoted to the transcripts of discussions in the *Corpus Catholicorum*.

For the purpose of solving the divisive religious problems that had helped turn the empire into a battle field, the Estates met in 1645 not as the three houses of the Imperial Diet where the Catholics had the majority, but divided along religious lines into the *Corpus Evangelicorum* and the *Corpus Catholicorum*. The subject of the negotiations was the *gravamina*, the grievances, that the Protestants had last presented to the Imperial Diet in 1640. The grievances mainly dealt with the disputed provisions of the Peace of Augsburg of 1555 and, therefore, with the political structure of the empire and the future of Catholicism and Protestantism in Central Europe. As it turned out, the Catholic Estates were quite intransigent, forcing the emperor to negotiate on his own the religious questions with Sweden and the Protestants.

An official transcript of the deliberations in the *Corpus Catholicorum* was not kept. But the twenty ambassadors of the fifty-five Catholic Estates were free to take notes of the discussions and decisions. Fritz Wolff, the editor of the present volume, has collected transcripts of the various ambassadors in twenty-three archives and identified with much skill their dependence upon each other. He based his edition upon the most complete and detailed manuscript, the transcript kept by the ambassador of the city of Cologne. Historians will be grateful that he published, apart from repetitions and minor points, the entire original text of the transcript. The text of the Cologne manuscript was supplemented by the transcripts of other ambassadors whenever the latter contained additional or more detailed information. The editor is thus able to offer a text that, by and large, records verbatim the entire discussions, revealing in fascinating detail the position taken by individual ambassadors. Altogether seventy-eight transcripts appear in this

volume, covering the period from October 1, 1646, to April 2, 1647. The editor also provides an extremely useful scholarly apparatus. In short, this volume is a fine example of intelligent scholarship. I look forward to the appearance of future volumes of this excellent publication.

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DIETRICH GERHARD, editor. *Ständische Vertretungen in Europa im 17. und 18. Jahrhundert*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, Number 27. Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions, Number 37.) Göttingen: Vandenhoeck & Ruprecht. 1969. Pp. 437. DM 44.

The history of parliamentary institutions, so well explored in France and in England, had not attracted the attention of German scholars until after World War II. Perhaps the main reason for the rather late flowering of revisionistic studies of this problem was the dominance of the schematic and typological theory of political development elaborated by Gustav Schmoller and modified by Otto Hintze and his students. According to the article by Sven Palme that is included here, in Sweden as well as in Germany historians friendly to the cause of state building and absolutism long denigrated representative assemblies. Consequently, the study of the parliamentary bodies of the Old Regime became the study of institutions that were weak and that failed or, more charitably, the study of a vanished form of political life. This collection of articles, however, incorporates new interpretations and fresh evidence in an overall survey of the *Stände* at national and provincial levels for Central, Northern, and Eastern Europe. No specific articles on France, Holland, or Russia have been included. The contributions, mostly by German scholars, are of high quality.

In his valuable introduction to the collection Gerhard indicates that the work of Roland Mousnier, L. B. Namier, R. R. Palmer, and Franklin L. Ford has enabled German specialists to revise their views on the Estates. The *Stände* were not always rivals of monarchical or princely power; they sometimes were associates of it. In fact in Sweden, Bavaria, and

Poland the deputies of the Estates sometimes obtained the upper hand over the ruler for a period only to be subordinated later; periods of absolutism alternated with periods of the *Ständestaat*. Particularly influential in developing this revisionist position, so evident in most of the contributions, was the pioneering work of O. Brunner, *Land und Herrschaft* (1965).

Foreign policy and war played a very large role in allowing particular rulers to achieve a short-term dominance over their Estates. The Thirty Years' War and the wars of Louis XIV enabled the rulers of Bavaria, Sweden, Poland, Hanover, and Hesse to secure control of their Estates because the latter feared military invasion. In Hesse, as Karl Demandt indicates, the practice of renting out of soldiers was instituted by the ruler after 1677 partly to escape financial bondage to the Estates. During the relatively peaceful years of the eighteenth century the Estates were able to recoup their ancient authority. Hanover fell more and more under the dominance of its Estates following the departure of the elector to become king of England. As Rudolf Vierhaus indicates, the Hanoverian Elector had to negotiate with six different Estates and could not, perhaps, maintain his grasp while in England. John Sobieski was never an absolute monarch, only a very popular leader of the multitude of Polish nobles; when his work was finished his successors could never obtain wholehearted support from the Diet. The eighteenth-century "renaissance" of the Estates throughout Europe may have been due to generally more peaceful conditions on the international scene than had previously existed.

Hintze did not always adhere to his earliest interpretation of the development of the *Ständestaat* to the *Gesamtstaat*, as Gerhard Oestreich indicates in his excellent discussion. Primarily in the later, post-World War I years Hintze saw the Estates as forerunners of modern constitutional and representative governments. Thus he modified his earlier view that the Territorial State was "dualistic" and consisted of a stalemate between its two component parts: *Stände* and ruler. This latter-day interpretation has led many of the contributors to this collection to maintain that there was indeed a continuity between the Old Regime Estates and nineteenth- and twentieth-century

parliamentary bodies. Gerhard in fact maintains that one cannot understand the latter without examination of their *Ständestaat* predecessors. Palme reveals that a number of contemporary Swedish historians have re-examined the "Freedom Period" of the Swedish parliamentary supremacy of certain decades of the eighteenth century in order to clarify the rise of Social Democracy and representative government later. The Estates of Württemberg were preludes to modern constitutional assemblies because they were closer in outlook to the common people, despite their unrepresentative character, than was the ruler. Gerhard A. Ritter sees a similar continuity in the "balance of power" in the English governmental system.

But most of the contributors remain dubious about the validity of this concept. Hartmut Lehmann sees the dependence of the Württemberg Estates on legal precedent their greatest strength, but also their greatest weakness. While old rights and concessions were zealously guarded by *Ständestaat* lawyers, nothing was done to institute new legislation or a new political relationship. This extremely important conclusion must be kept in mind in regard to all of the other states covered in this survey. Perhaps the Estates everywhere, except in England and possibly Sweden, were really antiquated strongholds of petty local privilege, as much captives of the Old Regime as the rulers.

On a less ambitious level many of these articles provide useful information on the actual workings of the Estates. Hans Roos claims that the Polish aristocracy, eight per cent of the total population, erected a kind of "gentleman's democracy" and influenced political radicalism in Europe; this is provocative but exaggerated. Palme's "party system" in Sweden may have been nothing more than the usual battle of cliques or cabals that characterized the life of most small and large Estates in Europe. How did the Swedish parliament, for example, reflect the desires of the majority of Swedes? György Bonis condemns the Hungarian Estates for their "baroque sterility." Peter Baumgart shows that the Electoral Mark *Landschaft* remained important during the eighteenth century in Prussia: this is a valuable contribution. Herbert Hassinger provides information on the important local administrative services main-

tained by the German Austrian Estates until 1848. Generally, the Estates throughout Europe were occupied, routinely, with a multitude of small complaints and petitions, "private members' bills" as Bonis aptly calls them.

These articles not only pinpoint some of the central issues of the history of the European Estates, they also introduce the student of comparative history to the local literature. Palme, for example, gives a valuable introduction to the sources for Sweden. The shadow of Hintze, while still covering much of the work of these scholars, is beginning to retreat. Günter Birtsch recommends more intensive and extensive research, particularly on social and economic factors. The paucity of information has doubtless persuaded most of these writers to follow the known road of constitutional and political history rather than to embark on the trail provided by Fernand Braudel and his followers.

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INGRID BÁTORI. *Die Reichsstadt Augsburg im 18. Jahrhundert: Verfassung, Finanzen und Reformversuche*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, Number 22.) Göttingen: Vandenhoeck & Ruprecht. 1969. Pp. 210. DM 21.

By the eighteenth century the Free Imperial City of Augsburg had lost much of its former glory. In a thoroughly documented and extremely detailed study, Dr. Bátori tries to refute the commonly accepted view that it was without historical interest. Unlike other recent historians of European cities, the author devotes little attention to social and cultural affairs. Instead she concentrates on the interaction between Augsburg's constitutional arrangements and its financial practices. She is intrigued by the contrast between the prosperity of its private citizens and the disarray of its public finances. Much of the latter she ascribes to an antiquated constitution. Based on the *Regiment* imposed upon the city by Charles V in 1548, the government was hierarchical, exclusive, overwhelmingly patrician, and rife with nepotism. Public offices were too numerous for a city that had lost half of its population since the sixteenth century. Because of the

necessity for religious parity between Catholics and Protestants in higher positions, public offices were also often overstuffed.

A final decline began with the harvest failure of 1770-71, which cut off Augsburg's usual sources of supply and forced the magistrates to spend vast sums buying grain abroad. From then until the end of the century increasing expenditures were financed by borrowing at high interest rates, while attempts at internal financial changes were frustrated by the cumbersome political structure. Various reform commissions came to nothing and efforts to broaden the constitutional base were rejected by the imperial authorities. After 1795 opposition to the magistrates came most prominently from the merchants, who were still largely excluded from the government. This opposition, in the author's opinion, was nonrevolutionary and constructive. It was also ineffective, as was one last vast expenditure by the magistrates, a bribe to the French to preserve Augsburg's independence. The government had become isolated. Even many patricians no longer supported it. "Voulez-vous être de la Bavière?" Napoleon asked a delegation of patricians and merchants. The financial problems of an independent Augsburg were at an end.

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HEDWIG BEHRENS. *Mechanikus Franz Dinnendahl (1775-1826), Erbauer der ersten Dampfmaschinen an der Ruhr: Leben und Wirken aus zeitgenössischen Quellen*. (Schriften zur rheinisch-westfälischen Wirtschaftsgeschichte, Number 22.) Cologne: Rheinisch-Westfälischen Wirtschaftsarchiv zu Köln. 1970. Pp. xvii, 579.

Unlike the other volumes in this series of studies on Rhine-Ruhr industrial history published by the Rhenish-Westphalian Economic Archive in Cologne, Miss Behrens's well-edited book is not a monograph at all but a compilation of documents relevant to the activities of Franz Dinnendahl, machinist. The son of a miller, educated only in rural grade schools, at first a miner's helper before he learned the mechanist's trade, Dinnendahl was one of the founders of the Rhenish machine tool industry as well as a leading mining entrepreneur. The cohort of other Rhineland founder families,



for example the Krupps, Stinneses and Springorums, he belonged to an artisan class that learned and applied a new industrial technology and laid the foundations of West Germany's later industrial revolution. Capital was scarce—most of it was borrowed from wealthy aristocrats who played a banker's role. Manufacture of steam engines (as good as those of Boulton and Watt, Dinnendahl thought) led him into metallurgy and mining enterprises. The boom of the Napoleonic period ended in a depression that proved disastrous, and as he had overextended himself while coal markets collapsed after 1815, Dinnendahl went bankrupt. Yet his businessman's vision was not wrong. The speculative extension of deep mining failed in 1815–22 because capital was scarce, government regulation was excessive, market conditions were poor, and the machines available could not yet prevent flooding in deep shafts.

The author used Dinnendahl's interesting autobiography (from the family firm's archives), and sources from local town and mining company archives. There is also a useful technical glossary and a good introduction by Hermann Kellenbenz. As the history of early industrialization is often fragmentary, further studies of this sort for other parts of Germany ought to be encouraged.

HELEN P. LIEBEL  
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SIEGFRIED SCHMIDT. *Robert Blum: Vom Leipziger Liberalen zum Märtyrer der deutschen Demokratie*. Weimar: Hermann Böhlhaus Nachfolger. 1971. Pp. 336. DM 24.

Robert Blum is remembered more for his death before the muzzles of Field-Marshal Windischgrätz's firing squad than for his life as a leader of Saxon and German democratic forces before 1848. His martyrdom for the German revolutionary cause has assured him a firm place in the authorized pantheon of German working-class heroes: a recent collection of workers' songs recorded in East Germany, for example, begins with a *Robert-Blum-Lied*. The undeniable national popularity of Robert Blum led to a spate of literature and song after his death (including a standard biography by his son) followed by decades of comparative neglect.

Now Siegfried Schmidt of Jena seeks to rescue Blum from the "shadow of falsifying bourgeois interpretation" (p. 7). The preface, which acknowledges Franz Mehring's biographical canon of mixing admiration with criticism, awakens hope that this new work might qualify or overturn the widely held impression among nonsocialist historians that Blum was overrated as a politician and confused or vacillating as a political and social analyst.

Such a hope is not rewarded. The author clearly admires Blum greatly, too often gliding over failings of character and tactics. In really serious cases he attributes such failings to the impersonal force of petit-bourgeois false consciousness. Following Engels (but not improving on him) Schmidt emphasizes Blum's transcendence of his milieu rather than the milieu itself. As a surrogate he presents an excessive amount of general background material already available in East German textbooks. Despite references to widespread archival work, the author leaves the impression that he was unable to unearth much new information, for he sometimes resorts to undocumented speculation. Blum's childhood and youth flash by in a handful of pages, while 1848 consumes a good half of this biography. Psychologically Blum emerges flat and one-dimensional.

Despite Schmidt's somewhat repetitive style, he must be credited for rendering his uncomplicated explanations in uncomplicated language. To add unnecessarily to the woes of this book, the publisher has provided no index but has allowed many technical errors to slip into print. With all these faults *Robert Blum* is a pedestrian but readable political biography centered heavily on the background of 1848. It could have been done better; but unless more information can be discovered, it is doubtful whether it must be done over.

CHARLES E. MCCLELLAND  
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WALTER WAGNER. *Geschichte des K. K. Kriegsministeriums*. Volume 2, 1866–1888. (Studien zur Geschichte der österreichisch-ungarischen Monarchie, Number 10.) Vienna: Hermann Böhlhaus Nachf. 1971. Pp. 287. DM 56.

This second volume of a projected three-volume history of the Austrian War Ministry from 1848 to 1918 covers the twenty-odd years

following upon the Prusso-Austrian War of 1866. It deals with the reorganization of the army after Austria's defeat in that war and the establishment of an autonomous Hungary within the Empire. The resulting administrative, organizational, and linguistic difficulties and their solutions are covered with expertise and thoroughness. Dr. Wagner also discusses at great length the troubled relations between army commanders and civilian authorities, with Emperor Francis Joseph serving as a rather unpredictable arbiter of their quarrels. Specialists in Austrian military history will find the book a rich source of information on the period in question.

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HANSGEORG LOEBEL, editor. *100 Jahre deutsche Geschichte*. (Niedersächsischen Landeszentrale für Politische Bildung.) Leer: Verlag Gerhard Rautenberg. 1971. Pp. 179.

The centenary of the proclamation of the German Empire on January 18, 1871, has produced a flood of books, pamphlets, and articles in the German Federal Republic seeking to interpret the significance of national unification in the light of the historical experience of the last hundred years. Some of this literature is of interest primarily for the world of scholarship, but most of it was intended for the general reading public. Hansgeorg Loebel has collected eight articles in the latter category for inclusion in a thin volume dealing not only with the Bismarckian era but with the history of Germany since then as well. Although the authors are trained scholars their purpose is to summarize and popularize the conclusions of professional historiography rather than to present the findings of original research. The result has some of the quality of a Sunday supplement, yet it is a superior example of that genre. Avoiding the quaint and sensational it attempts to enlighten not to titillate. Its interest for the historian, nevertheless, lies in what it reveals regarding the view of the past offered by the popular press to the educated public in Germany.

The articles themselves, though they are all interpretive rather than investigative, vary considerably in quality. The one by Theodor Schieder analyzing the changing concepts of the German nation from the eighteenth cen-

tury to the present is a fine example of popular history. Sound and thoughtful, it seeks to familiarize the intelligent layman with the judgments of scholarship. Ingomar Bog presents a solid yet readable account of economic developments since 1871 that successfully avoids the extremes of simplification and complexity. Carl Jantke describes with skill the relations between employers and employees during the last hundred years and their shifting attitudes toward one another. On the other hand, the article by Friedrich Klemm depicting technological progress is little more than a lengthy catalog of names and inventions unencumbered by any ties to political, economic, or social history. Richard Nürnberger offers a familiar and pedestrian summary of German foreign policy, especially since 1918, popular in tone but undistinguished in style. And Hansgeorg Loebel himself, while entitling his contribution "Germans on the Other Side of Our Borders," deals exclusively with the Teutonic minority in Latvia, Estonia, and Romania. Many historians will want to leaf through these pages for their flavor, but only a few will consider it worthwhile to read them with intensive care.

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BRADLEY F. SMITH. *Heinrich Himmler: A Nazi in the Making, 1900-1926*. (Hoover Institution Publication 93.) Stanford: Hoover Institution Press, Stanford University. 1971. Pp. ix, 211. \$6.95.

Each of us, when reflecting upon the formation of his conscious personality, is aware of the complexity, ambiguity, and even chanciness of the steps. Few modern men seem to believe that they "made themselves" or even that they just "unfolded" out of innate tendencies. Yet when many scholars, even historians, confront the lives of leading national socialists, attempts are made to establish uniformities, integrities, continuities, and causal sequences based almost exclusively on individuals' personal choices. Although Bradley Smith's subtitle might suggest a rather preconceived outcome for his study of Heinrich Himmler's childhood and youth, his book is truly a milestone on the road to scientific biography applied to those normally stereotyped as "Nazis." Working carefully from the

Himmler family's personal papers, which Himmler's personal staff had assembled, as well as from Himmler's boyhood diaries and from correspondence scattered in party, SS, and other captured German records, Smith reconstructs a believable human being with whom one can sympathize and occasionally identify. There is more contour and contrast in the portrait of this awkward, talkative, eager boy than in the earlier sketch Smith did with Werner Angress in 1959 (*Journal of Modern History*, 31 [1959]: 206-24).

Compared with Peter Loewenberg's intensely interpretive (but highly schematic) essay on Himmler's "unsuccessful adolescence" (*AHR*, 76 [1971]: 612-41), Smith's narrative is richer with data and evidence and more restrained in generalization. Smith has no need to succumb to tempting allusions and analogies with the career of the *Reichsführer SS*; he has adequate factual material to describe a twenty-one year old or an eighteen year old, and that is what he writes about. While Smith is very effective in bringing out what Himmler's parents "did to" him, the art of the book lies in our ability to watch the youth "try on" alternative roles and behaviors offered by the kaleidoscopic processes of 1918-22. His effort to integrate for himself the contradictory values and tendencies of his background and his expanding horizons is extremely well portrayed. Gradually (1923 is sketchy due to missing diary sections) Himmler builds himself a niche in the Hitler movement and as he does so becomes more self-assured and competent. Without a doubt, as he becomes more mature he becomes more comfortable. The needs he was able to meet in the fascist political milieu made it worth his while to follow and serve Hitler. Yet his personality was no carbon copy of Hitler, Strasser, or Roehm. He was on his own. Smith gives us just enough glimpses of the man in his mid-twenties to enable us to acquire a distaste for this busybody, prude, and intrigue artist. But now he is a real live, three-dimensional participant in history.

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WILHELM DEIST, editor. *Militär und Innenpolitik im Weltkrieg 1914-1918*. In two volumes. (Kommission für Geschichte des Parlamentarismus und der politischen Parteien Bad Godesberg; Militärgeschichtliches Forschungsamt Freiburg im Breisgau. Quellen zur Geschichte des Parlamentarismus und der politischen Parteien. Second Series, Militär und Politik, Number 1, parts 1 and 2.) Düsseldorf: Droste Verlag. 1970. Pp. clxxiii, 647; 651-1530. DM 184 the set.

Although many military records of Imperial Germany, including practically all files of the Prussian army authorities, were lost when the Potsdam *Heeresarchiv* burned down in 1945, meaningful research on the army's role in the Second Reich is nevertheless still possible. In addition to the archives of the imperial navy, which survived the last war almost intact and constitute a fair source of information on some army matters, the private papers of many prominent Prussian officers have lately become accessible as well—most of them now being in the custody of the *Bundesarchiv-Militärarchiv* at Freiburg. Moreover, since some *Bundesstaaten* of the Second Reich had war ministries and certain other semi-autonomous army institutions of their own, valuable evidence, including many reports and directives from the "central" army authorities in Berlin and, during the war, also from the field headquarters of the general staff, was preserved in various regional archives.

In preparing this edition of documents on the German military and domestic policy during the First World War, Dr. Deist has made thorough use of all West German archival holdings. The privately controlled Hindenburg *Nachlass* as well as documents in the DDR were unfortunately not accessible to him. Expertly introduced and annotated, the material he has assembled—official papers, private letters, diary entries, etc.—offers much fresh information on the activities of the German military authorities on the home front, particularly as regards their attitude toward the civilian branches of the government and their role in the maintenance of public order, in censorship and propaganda, and in the economic mobilization of the country. On certain issues that transcended the realm of *Innenpolitik*, such as the submarine controversy, the debate on war aims, or developments in Alsace-Lorraine, the documentation has been deliberately kept to a minimum, but these space-saving limitations do not materially affect the value of the collection.

Recent studies, notably Gerald D. Feldman's *Army, Industry, and Labor in Germany, 1914-1918* (Princeton, 1966), have already shown that some German army officers in positions of influence during the war were relatively "progressive" in their approach to economic and social problems. Numerous documents in Deist's collection not only reinforce these findings but also indicate that there were a good many "deputy commanding generals" and other military functionaries who carried out their administrative responsibilities on the home front with far more political acumen and sensitivity to the mood of the civilian population than they have usually been given credit for. That many other officers were reactionary in outlook or inept in handling domestic affairs is, of course, well known—and amply documented here as well.

The collection is rounded off by several useful charts on organizational matters and a map of the corps command areas. The *Register*, prepared by Hans Umbreit, is over a hundred pages long, but many entries are rather imprecise. General von Bissing, for example, is listed as governor general of Belgium until 1918 (he died in April 1917), and General Wild von Hohenborn is identified as deputy war minister during a period when he was actually commanding a division at the front or serving as quartermaster general in the OHL. It is to be hoped that in a new edition these and many similar slips in the index will be eliminated from what is otherwise a meticulously edited and informative work.

ULRICH TRUMPENER  
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CLAUS GUSKE. *Das politische Denken des Generals von Seeckt: Ein Beitrag zur Diskussion des Verhältnisses Seeckt-Reichswehr-Republik*. (Historische Studien, Number 422.) Lübeck: Matthiesen Verlag, 1971. Pp. 283. DM 45.

Mr. Guske has written a thorough, complex, and organic presentation and analysis of the political thought of General Hans von Seeckt and of the relationship of his political thought to his actions in the major crises of the Weimar Republic's early years. After a brief sketch of Seeckt's background and early life, the author turns to a careful consideration of the development of Seeckt's political ideas and conceptions during the course of World War I, providing a

survey both of the enduring skeleton of his political frame of reference and the variations upon this theme that indicated the influence upon the general of contemporary events and currents of thought. His third chapter is devoted to a systematic exposition of Seeckt's political thought, with primary emphasis upon its constant elements. The fourth and final chapter considers and presents the thought and actions of Seeckt during his period of activity as a senior officer or the *de facto* commander in chief of the Reichswehr. A short summary concludes the book.

In general, Guske has achieved his purpose. His presentation is well organized, clear, and effective, and his arguments are generally compelling. He has used the available sources and the secondary literature in the field in an exemplary manner, exploiting it for evidence and examining it for coherence, accuracy, and contradictions. His second and third chapters, however, suffer to some extent from unnecessary repetition both of Seeckt's ideas and of Guske's commentary on these ideas. Had these chapters been pruned the book would be easier to read and have a sharper impact.

The book is valuable because it pulls together Seeckt's ideas regarding politics and examines them for consistency, for applicability, and for their influence upon his actions. The analysis is fair and balanced, even though it is clear at times that the author is not at one with Seeckt in his beliefs, which must have made his task sometimes difficult. Guske corrects errors and refutes allegations regarding Seeckt's ideas, attitudes, and actions in the work of such historians as F. L. Carsten, Walther Goerlitz, and Wolfgang Sauer on the basis of factual evidence and Seeckt's turn of mind and political concepts.

On the debit side, aside from repetition, some of the author's points seem to be heavily belabored. And in viewing the factors leading Seeckt to take a conservative viewpoint and to see life in terms of a continual struggle, Mr. Guske might well have considered more specifically the impact of the military life and professional values, along with those he presents. He might also have given more consideration to the years after 1924 in Seeckt's active career and particularly to the circumstances of his departure from office. Finally, in a book dealing

with the general's political thought one misses any mention of his specific reactions to National Socialism in the later years of the republic and the early years of the Third Reich.

In sum, Mr. Guske has provided the historian of Weimar Germany and the student of political science and political thought with a valuable compendium and appraisal of Hans von Seeckt as a political thinker.

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DAVID FELIX. *Walther Rathenau and the Weimar Republic: The Politics of Reparations*. Baltimore: Johns Hopkins Press. 1971. Pp. xii, 210. \$9.00.

The tortuous history of reparations has often been told; but this study, concentrating on the early period (1921 and 1922) when Rathenau became a key figure and a symbol for the politics of fulfillment, makes a valuable contribution to the subject. The slender volume is based on very thorough research in the various archives, including the German Central Archive in Potsdam, and hence enriches our knowledge at several points. The Stinnes-Rathenau dispute at Spa, for example, appears less black and white; we learn more about Rathenau's important negotiations with the British government late in 1921; the character and the issues of the German Cabinet crisis late in May 1922 over the Paris agreement of Finance Minister Hermes become clearer. The book thus fills a gap in the literature in English since Ernst Laubach's *Die Politik der Kabinette Wirth 1921-1922* (1968) has not been translated.

The book, in spite of dealing with heavy technical material, reads exceedingly well. As to the inner contradictions of the reparation story and reparation politics, they are only too well known: the key role—not yet assumed—of the United States; the unwillingness of the Allies to let Germany earn, by way of foreign trade, the cash to pay reparations; the insoluble transfer problem; the contradiction in the French position of wanting a Germany rich enough to pay indemnities and at the same time one that was ruined; the gap between economic reality and public expectations that forced every official to lie or at least to obscure the truth. Most important is the point made by

the author—that any drastic attempt by the German government through radical taxation or confiscation of property to pay the quantities of wealth the Allies demanded would have meant revolution in Germany, that is, violent resistance, and this was something the Allies did not want either.

As for Rathenau himself, the picture that emerges, while drawn critically, is well balanced, full of psychological insight, and the appraisal altogether judicious. Where it remains ambiguous this is due less to the author than to the person he portrays. However, Felix does Rathenau less than justice in not stressing enough his awareness that he was merely one of the first, or the first, who had to jump over the hurdle; that the Ruhr occupation was unavoidable; and that he was simply a sacrifice, not necessarily the last one, which had to be offered for the sake of his country. To describe as simply "morbid vanity" Rathenau's expressed expectation that he sooner or later would be assassinated, underrates the tragic element of his fate.

Where I have to register dissent is in the last two chapters of Mr. Felix's otherwise valuable study. In the first of these, "Tendency to Acts of Violence," the author finds no pattern and tries to discredit the findings of E. J. Gumbel on rather flimsy evidence, while at the same time interpreting Helfferich and his political attitude and activities in too harmless a fashion. Helfferich was much too shrewd not to know what he was doing. Also, anybody who (like me) experienced the mass demonstrations on the occasion of Rathenau's assassination is aware that the workers too knew what the murder meant—that "the enemy stood on the Right" and that "the great tramping of boots and shoes and their heavy marching" was unfortunately the only way they could express their concern for democracy and freedom. Finally the attempt, in only two and a half pages, to "use the present day as starting point . . . and suggest Rathenau's importance in the Republic's life and death and in the world's progress from one great war to the next" (p. 175) is somewhat forced, tenuous, and inadequate. But the book can stand on its own merits, which are considerable, without its "conclusion."

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MARTIN VOGT, edited and with an introduction by. *Die Entstehung des Youngplans, dargestellt vom Reichsarchiv 1931-1933*. (Schriften des Bundesarchivs, Number 15.) Boppard am Rhein: Harald Boldt Verlag, 1970. Pp. 396. DM 30.

This book on the genesis of the Young Plan is entangled in the crooked history it seeks to put straight. It began as a rebuttal of the penultimate liar and greatest survivor of German modern times, Hjalmar Schacht, who was skillfully dissociating himself from the Young Plan. He was so successful, although he had been the leading German negotiator, that Hitler reinstalled him in office and then promoted him to a higher level of nefariousness. The rebuttal, in the form of a résumé of the government documents, was ordered by the Brüning cabinet in March 1931, completed in the beginning of 1933, and tactfully forgotten until recently.

*Die Entstehung des Youngplans* achieves its original purpose honestly. Indeed, the archivist editor, armed as he is with irrefutable documents, cannot quite avoid overkill despite his professional dispassion. Beyond that, the book, assisted by Vogt's fifty-eight-page introduction, is useful as a supplementary account of the later developments in reparations.

The character of the dispute and its wider implications derive from the inherent insolubility of the interwar problem of international indebtedness. The United States was demanding war debt payments without permitting the Allies to build up trade surpluses. The Allies, for their part, were protecting their economies by passing the burden on to Germany in the form of reparations. Germany, similarly inhibited in its foreign trade, had only fiction as defense. Without being called to order, Schacht could alternatively pose as the exponent of purity in economic policy and as an economic statesman authorized to overrule his chancellor on political issues. He had his opportunity because no German cabinet could bear the full responsibility for forcing a hopeless sacrifice upon the German people. The Müller cabinet, too unsteady to risk a confidence vote when it was formed in June 1928, hoped that Schacht would divert upon himself part of the disgrace for dealing with the Allies, but he redirected the disgrace back to the government. The depression, itself partially a product of the situation created by war debts and reparations, bur-

ied the Weimar Republic along with all such payments.

Use of the book must be accompanied by reference to other data. The researcher should read his own selection of the documents rather than trusting to the archivist's summaries, however conscientious. The Young Plan, furthermore, was an international bastard, and most of its origins are to be sought outside Germany. Besides the documents of the other governments concerned, the expressions of public opinion are important, while pointed economic statistics are needed to prick the swollen political nonsense.

DAVID FELIX

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HANS GÜNTHER HOCKERTS. *Die Sittlichkeitsprozesse gegen katholische Ordensangehörige und Priester, 1936/1937: Eine Studie zur nationalsozialistischen Herrschaftstechnik und zum Kirchenkampf*. (Veröffentlichungen der Kommission für Zeitgeschichte bei der Katholischen Akademie in Bayern. Series B: Forschungen, Number 6.) Mainz: Matthias-Grünwald-Verlag, 1971. Pp. xxv, 224. DM 35.

This monograph deals with the approximately 250 lawsuits initiated in 1936 and 1937 by the National Socialist regime against Catholic priests and lay brothers on charges of homosexuality. The proceedings were given all the publicity that Joseph Goebbels's propaganda machinery could bring to bear; they were and still are on that account widely dismissed as false and have been compared in style and content to the contemporaneous Moscow "show trials." The charges were in fact substantially true, particularly in the case of the lay brotherhoods. The problem is the more subtle one of the uses to which law may be put. Utilizing a wide variety of sources, including the oral and written testimony of participants in the legal processes, the author has produced a fine case study of justice in the National Socialist state and a valuable addition to the growing body of literature on the *Kirchenkampf*.

The motives behind the decision to prosecute religious congregations were in part a matter of National Socialist biopolitical quackery, especially Heinrich Himmler's obsession with racial cleanliness. More important considerations were Nazi impatience with the develop-

ing stalemate in Church-state relations and a growing unwillingness to tolerate independent sources of authority. In using morals charges to discredit the Church in the eyes of the faithful the Nazis were prepared to take some very considerable risks, including the danger that the regime might itself be discredited at home and abroad. Traditional legal authorities and civil servants, attempting to cling to the pathetic shreds of the *Rechtsstaat* concept of objective apolitical neutrality, were caught and then compromised in what became a massive, systematic, and lurid propaganda campaign. All too typical was a statement of Justice Minister Franz Gürtner in 1937: "All material from the morals lawsuits is at the unconditional disposal of any party authority. How it shall be used is the concern of the Propaganda Ministry."

The propaganda campaign backfired and did not lead to an "inner crisis of loyalty" in the Church. Most Catholics on the contrary remained steadfast, and Church membership, which had been falling off in the Weimar Republic, actually increased. The test of 1936-37 also demonstrated just how much the hierarchy and faithful needed the support of each other if the Church were to be successful in its principal aim vis-à-vis the regime: preservation of its autonomy. Both the limits of National Socialist control as well as the strengths and limitations of Catholicism's resistance to the regime had thereby become quite plain.

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JAMES DOUGLAS-HAMILTON. *Motive for a Mission: The Story behind Hess's Flight to Britain*. With a foreword by ALAN BULLOCK. [New York:] St. Martin's Press. 1971. Pp. 290. \$8.95.

J. BERNARD HUTTON. *Hess: The Man and His Mission*. Introduction by AIREY NEAVE. New York: Macmillan Company. 1971. Pp. xv, 262. \$6.95.

Of these books dealing with one of the more bizarre incidents of World War II, only Douglas-Hamilton's needs to be taken seriously. The author is the second son of the duke of Hamilton, the man Rudolph Hess came to England to visit one Saturday evening in May 1941 during the height of the battle of Britain. Since childhood young Hamilton had been fascinated by the story and after graduating from

Oxford set out to discover what had actually happened. The result is an impressive first book. As Alan Bullock, who helped guide the research, has noted in his foreword the study would have been of value even if it did little more than give the English side of the story and present documents made available by the duke of Hamilton.

There is, however, a great deal more. This is, in effect, a dual biography, for inquiry into Hess's motives inevitably led back to Albrecht Haushofer, son of the famous professor and Hess's personal adviser. Unwittingly he planted the idea in the Nazi's simple and fanatic mind that Hess could gain a separate peace with England by approaching Haushofer's English friend, the duke of Hamilton.

Albrecht Haushofer's life, pictured here for the first time with sensitivity and understanding, reveals one of the more tragic aspects of Hitler's Germany. Like many of his contemporaries, young Haushofer was torn by the conflicting demands of patriotism and humanity. He found himself, for mixed motives, serving on diplomatic missions for Ribbentrop and Hitler, while telling himself—as so many others had—that he could mitigate tyranny by working against the system from within. Distrusted and despised by both the Resistance and the Nazis he became his own most bitter critic and welcomed death at the hands of the Gestapo as escape from a life he felt to be hypocritical and base.

There are no major surprises in the chapters on Hess, but the author has given the first authoritative and richly detailed account of a story known previously only in outline form. Hess emerges as a devout Nazi of limited capacity whose loyalty to his Führer was canine. He hoped to recoup his own declining influence by fulfilling what he took to be his master's desire for a separate peace with England. Hitler had not known of Hess's flight, was appalled by it, and understandably discomfited by his own official explanation that his trusted deputy had been off his head for years.

The author draws on his father's papers for a valuable discussion of Britain's reaction to the Hess mission. Englishmen were bemused by his abysmal ignorance of their temperament and his puerile efforts at a separate peace. The government quite wisely decided that Britain

could profit most from the incident by letting the Nazis stew in the juices of their own embarrassment. Hess could never understand why his frenetic demands to see the king and the prime minister remained unanswered. Winston Churchill received the startling news over after-dinner brandy and cigars that Hitler's personal deputy had arrived in England. In a latter-day version of Drake and his game of bowls, the great man observed, "Well, Hess or no Hess, I'm going to see the Marx Brothers."

Hutton's regrettable book contains a good chapter on Hess's trial and makes an effective plea for the release of the addled old prisoner of Spandau, but generally this is less history than it is second-rate thriller. The plot is thickened by invented incident and manufactured conversation. The author imagines, for example, that Hitler knew about Hess's flight all along and had plotted it with him. This assertion, like many others drawn from the rumor mills of the time and the not disinterested memories of Frau Ilsa Hess, is given without benefit of evidence, documentation, or bibliography. Mr. Hutton's book, however, is not lacking in an element of suspense: one keeps wondering how wrong he's going to be and why a distinguished house should wish to publish such a manuscript.

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H. G. KOENIGSBERGER. *The Habsburgs and Europe, 1516-1660*. Ithaca: Cornell University Press, 1971. Pp. xv, 304. \$8.50.

PHILIPPE ERLANGER, with the assistance of ERIC NEWEKLOWSKY. *L'empereur insolite: Rodolphe II de Habsbourg (1552-1612)*. Paris: Éditions Albin Michel, 1971. Pp. 262. 24 fr.

Readers who have referred to Koenigsberger's concise and judicious chapters on the Habsburgs in the volumes of the *New Cambridge Modern History* will find it helpful to have them collected under separate cover, along with his extended essay on the European civil war of the seventeenth century, reprinted from H. R. Trevor-Roper's *The Age of Expansion* (1968). The author skillfully synthesizes modern work on the dynastic and political events of the period, taking into account pressures from aristocratic estates and popular religious and social movements and culminating

in the genuine crisis of societies and their political constitutions that resulted from the Thirty Years' War.

Although the author states that he wrote the three separate essays for a single work, the book is awkwardly organized and tends to neglect the Austrian Habsburgs in the first two parts, and the Spanish in the third. Inevitably the account of the far-flung activities of the dynasty merges into a retelling of the diplomatic history of two centuries. Though this is done with great selectivity and balance one misses the coherent sense of the *Casa d'Austria* itself, its inner sense of mission, more effectively portrayed in Wandruzka's *The House of Habsburg* (1956). Koenigsberger's work is useful rather than definitive. It lacks notes or bibliographic references, which may limit its utility to nonspecialists consulting what is otherwise a valuable and sophisticated introduction to the subject.

Erlanger's biography of Rudolf II, the work of a distinguished French literary craftsman who has done perceptive studies of Louis XIV, Richelieu, and other seventeenth-century figures, is a readable essay aimed at deciphering that monarch's enigmatic personality, though essentially directed toward a popular audience. As a popular biography it does not surpass Gertrud von Schwarzenfeld's 1961 account but may be of some interest because of the real dearth of any modern scholarly treatment. For particulars of the reign the serious student must still rely on the 1868 biography by Anton Gindely, and in some instances, on Ranke. Unlike the older works Erlanger's book devotes considerable space to Rudolf's artistic and scientific activities, though the Emperor's importance as a patron of modern science in supporting Tycho Brahe and Johannes Kepler is exaggerated. Erlanger's treatment of the role of Melchior Khlesl in the *Bruderzwist* is superficial. The book lacks footnotes and a bibliography.

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GRETE KLINGENSTEIN. *Staatsverwaltung und kirchliche Autorität im 18. Jahrhundert: Das Problem der Zensur in der thesesianischen Reform*. (Österreich Archiv. Schriftenreihe des Instituts für Österreichkunde.) Vienna: Verlag für Geschichte und Politik, 1970. Pp. 234. Sch. 140.



We tend to view censorship in terms of the struggle to mitigate it, but that perspective can do violence to the historical setting in which censorship systems have functioned. Grete Klingenstein, a promising Austrian scholar, here firmly implants the problem in its context, thus transforming the issue. In eighteenth-century Austria the critical question concerned control over censorship, not its existence. Censorship, then, was an aspect of Josephinism, and the nature of Josephinism provides the key to understanding its role.

Though erudite, Klingenstein does not provide just another specialized monograph. Blending a striking command of both archival and interpretive sources with a keenly developed historical consciousness, she offers an incisive view of eighteenth-century Austrian political and religious development. In treating Josephinism, for example, she recognizes the false dichotomy between *Staatskirchentum* and Reform Catholicism: Josephinism was both, and the balance between these elements resulted from changes within the Church as well as within the government. Klingenstein argues that the institutionalization of censorship, partially an anti-Jesuit campaign and partially a secularizing of ecclesiastical functions, generated increasing Church-state tensions. Thus *Staatskirchentum* and Reform Catholicism both combined and conflicted to produce more carefully defined structures and jurisdictions in Church and state. She does not entirely eschew the advantages of a modern perspective, however, for she indicates that the erosion of Van Swieten's influence after 1767 altered the character of censorship by making it utterly a decision of state, thus less liberal than that exercised by Van Swieten and the other "bourgeois intellectuals" (for example, Martini) who had initially formalized the censorship procedures.

Klingenstein's resolution of the puzzler, "How could the devout Maria Theresa approve policies inimical to the state?"—that the empress separated her religious concerns, which were ultimately private, from her monarchical concerns—seems simplistic in light of her emphasis on the complexities of the historical process. Furthermore, her discussion of Jansenism, particularly in connection with Van Swieten, is thin (she did not have available Frank Brechka's recent work on Maria Theresa's

physician-adviser, *Gerard van Swieten and His world, 1700-1772* [1970]). But these are minor flaws in a work notably well done.

WILLIAM J. MCGILL  
Alma College

FRIEDRICH PRINZ. *Prag und Wien 1848: Probleme der nationalen und sozialen Revolution im Spiegel der Wiener Ministerratsprotokolle.* (Veröffentlichungen des Collegium Carolinum, Number 21.) Munich: Verlag Robert Lerche. 1968. Pp. 180. DM 19.

This book delivers exactly what it promises in the title: a re-evaluation of the relations between the authorities in Prague and Vienna in 1848 based on the hitherto little-used protocols of the Vienna Council of Ministers. Since the protocols naturally offer only a one-sided image, the author has attached to his essay some twenty-five documents drawn mainly from the archives of the Austrian Ministry of Interior and the papers of Alexander Bach. The documents are highly relevant, as is the entire study. It is clear, precise, brief, persuasive, and without pretensions. It is in the best tradition of German political historiography. The point is to show why, in all of revolutionary Europe, the counterrevolution first triumphed in Prague. The author finds the reasons in the forceful personality of Prince Windischgrätz and in the conservatives' skillful exploitation of the clashes between liberal-democratic and nationalist ideologies and interests. These clashes allowed the return of the conservatives and postponed the Central European revolutions by three generations. Professor Prinz turns with equal indignation against historians of the old school, who had seen only the struggle of nationalities in 1848, and against the new, mainly Marxist historians, who see only the supranational class struggle. What these historians have generally neglected, and what the author energetically emphasizes, is the dominant conflict between centralist and federalist forces. While the liberal Vienna government, composed mainly of imperial bureaucrats, fought for Josephinian centralism, the new liberal Prague authority, coalescing around Count Leo Thun and František Palacký and composed mainly of Bohemian aristocrats and Czech *grands bourgeois*, wished to set up an autonomous Bohemia and thus re-

turn to a pre-Maria Theresan state of affairs. Unfortunately for both governments, neither was able to cope with its domestic radical opposition. The military, which came to the aid of the embattled authorities in Prague in June and in Vienna in October 1848, replaced both liberal centralism and liberal federalism with reactionary neo-absolutism. The activities of Windischgrätz, Thun, Pillersdorf, Doblhoff, and Bach are clearly described; the court and the camarilla remain somewhat obscure. The Czech-Moravian-Silesian problem is masterfully explained; the Hungarian question—ultimately more important—was strangely neglected by the Vienna Council of Ministers and could have been enlarged upon by the otherwise most conscientious author.

ISTVAN DEAK  
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RONALD FLORENCE. *Fritz: The Story of a Political Assassin*. New York: Dial Press. 1971. Pp. viii, 337. \$8.95.

The growth of antiwar sentiment in the United States during the Vietnam conflict lends an air of topicality to this biography of Dr. Friedrich (Fritz) Adler, the assassin of the Austrian prime minister, Count Karl Stürgkh, during World War I. Fritz was an intellectual and a radical, the son of the founder of the Austrian Social Democratic party, and a man more activist than his patient father. Stürgkh was the bureaucratic despot who guided Austria from parliamentary paralysis into dictatorial *fortwursteln*, unruffled by the outbreak of war.

Such initial enthusiasm for war as existed in the Austrian half of the Dual Monarchy dissipated rapidly, but Stürgkh would not summon parliament into session. The "Imperial and Royal Social Democrats," writes Ronald Florence, were afraid of political repression and unwilling to harass the Stürgkh regime. Loyally, they waited for an end to the war. Fritz Adler, a doctor of physics despairing of the timidity of his father's associates, finally decided that a violent act was necessary. On October 21, 1916, in an act of revolutionary defiance he shot Stürgkh. Fritz turned his public trial into a forum for the indictment of Austria-Hungary and the war, but events passed him by, and the path of revolution was traveled by others. He died in 1960.

Using published and unpublished sources Florence has probed the intellectual and emotional life story of Friedrich Adler in search of the background and motives for the assassination. He weaves his narrative through a thin description of the last years of Austria-Hungary. The lack of footnotes presents a problem, especially when whole phrases from Robert Musil's *Man Without Qualities* keep turning up in some of the early chapters. The author has smooth prose of his own, however, and has researched his subject well. Despite occasional instances when enthusiasm has led Florence into an inaccurate observation *Fritz* is an interesting and thoughtful study.

GERALD R. KLEINFELD  
Arizona State University

ERWIN BUCHER. *Die Geschichte des Sonderbundskrieges*. Zurich: Verlag Berichthaus. 1966. Pp. 595.

Bucher's massive book presents a detailed day-to-day account of the critical months in 1847 when a majority of Swiss cantons took up arms to prevent seven Catholic members, who had formed a special alliance (*Sonderbund*), from seceding from the confederation.

"The last of the Swiss civil wars" had its origins in the archaic and rigid structure of the confederation, which dated back to 1815 and had no built-in mechanism for change. The efforts of the liberals to modernize the make-up of Switzerland became increasingly bitter and violent as religious issues, foremost among them the activities of the Jesuits in Lucerne, began to poison the political conflict. The unyielding and at the same time provocative stance of the conservative Catholic cantons, united in their alliance, led the liberal leaders into actively preparing a military intervention.

War broke out late in October 1847 and came to an end barely a month later when Lucerne surrendered to the federal army. The commander of the federal troops, General William Henri Dufour, making superb use of his strategic advantages and material superiority, had devised a war plan that allowed him to defeat the enemy after a small number of skirmishes and a minimum of losses in human lives and property. His humanitarian and generous treatment of the defeated and occupied cantons led to a rapid healing of the wounds

and prepared the ground for the reconciliation between the two parties. Within a year after the end of the war the country underwent the fundamental change that created modern Switzerland when it adopted the constitution of 1848, "the most fortunate and most significant deed" in Swiss history.

Bucher writes political and military history, concentrating on the debates of the delegates to the Federal Diet on the one hand and on the description of the campaigns and skirmishes on the other. The story is frequently interrupted by critical evaluation of the sources, and many documents are quoted at length in their German or French original. Except for Dufour, Ulrich Ochsenbein (president of the Diet and federal colonel), and Constantin Siegwart-Müller (leader of the *Sonderbund*), few personalities are treated in enough detail to achieve plasticity.

Bucher's interpretations and evaluations are well balanced and noncontroversial. The special value of his work lies in its depth of research, revealed not only in the text itself but also in the extensive bibliography. Bucher has made use of all available source materials and has written the definitive history of this important episode in Swiss history. Numerous illustrations, mainly photographs of leading personalities, enhance the appeal of this beautifully manufactured book.

HEINZ K. MEIER

Old Dominion University

ERICH GRUNER, with the collaboration of ANDREA BAECHTOLD *et al.* *Die schweizerische Bundesversammlung 1920-1968: L'Assemblée fédérale suisse 1920-1968.* Bern: Francke Verlag. 1970. Pp. 287; tables A-LL, 13 charts. 80 fr. S.

ERICH GRUNER, editor. *Die Schweiz seit 1945: Beiträge zur Zeitgeschichte.* (Helvetia Politica, Series B, Volume 6.) Bern: Francke Verlag. 1971. Pp. 401. 28 fr. S.

Both of these books on the recent history of Switzerland were edited by Erich Gruner, who is in charge of the research center for history and sociology of Swiss politics at the University of Bern. They contain selections in German and in French and fill important lacunae in Swiss studies. The volume on the Swiss parliament since 1920 has each article in both lan-

guages, but the volume on Swiss development since 1945 contains no translations.

*Die schweizerische Bundesversammlung 1920-1968*, a much more formal study, treats a longer period of time and focuses on a single topic—the composition of the Swiss parliament. In it Professor Gruner and his four collaborators have produced a successful example of teamwork. They have deliberately avoided a multiple biography approach in favor of a statistical study that includes tables and graphs appended to the volume in a separate folder. The work reflects the current interest of European scholars in quantification as a technique. Modern in its methodology, the volume is also up to date in the kind of questions it asks. Various selections deal with the social origins of parliamentarians, their geographical origins, their level of education, and their professional achievements. In addition the matter of membership of these parliamentarians in student societies and other associations such as interest groups or lobbies is explored. Readers who accept the notion that a technocratic or politically professional elite dominates the parliamentary life of a technologically advanced nation may be startled by some of the results reported in this work.

The second book is quite different. In the winter of 1969-70 the *Volkshochschulen* of Zürich and Bern celebrated their fiftieth anniversary with a series of lectures on the theme of Swiss development since 1945. Seventeen authors collaborated on the project. Besides university professors, they included experts on various topics, as well as public figures. For example, Max Petitpierre, a former president of Switzerland, contributed a thorough examination of the complex problem of Swiss neutrality and its implications for foreign relations. Two major theologians, one Protestant and the other Catholic, discuss the role of religion in their country. In another vein, Hanspeter Matter, a Bernese lawyer and an author-performer of satirical songs, examines recent developments in Swiss society as seen by the younger generation. Other contributions deal with the whole gamut of contemporary concerns, including education, ecology, national defense, and economic questions.

The two books provide a well-integrated survey of present-day Switzerland. The quality of

these useful and informative books appears to be in large part due to the skill of the editor, Erich Gruner, who provides a most thoughtful analysis of the directions of the societal problems in a period of rapid change in the final essay in the work on Switzerland since 1945. Both volumes should be of interest to those concerned with current problems of a small, highly developed, densely populated country with a truly multinational system. They will be useful to any student of modern Europe and are invaluable for the specialized student of modern Switzerland.

SABINE JESSNER

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BEAT GLAUS. *Die Nationale Front: Eine schweizer faschistische Bewegung, 1930-1940*. [Einsiedeln:] Benziger Verlag, 1969. Pp. 503.

A number of authors have dispelled the popular myth that the bastions of Western European democracies were immune to the virus of fascism in the interwar era. Most were infected by the contagious malady but were able to ward it off, since it did not represent a significant political force.

The present study deals with a fascist movement in one of these countries—Switzerland. The author, a Swiss sociologist, has painstakingly perused the available literature, primarily the fascist press and pamphlets, and conducted a number of interviews with veterans of the period. The result is a detailed account of the rise and fall of the National Front, a movement spawned in 1930 at the University of Zurich, gaining bourgeois support primarily in the German area of Switzerland, and playing a vocal role in public affairs until its demise a decade later.

Glaus views the rise of the National Front, as well as the short-lived New Front (1930-33), which he also dissects thoroughly, as symptomatic of the malaise sweeping across Europe at that time. The democratic systems were on the defensive as they struggled with a major economic crisis and with the fissure dividing Marxists and the bourgeoisie. Switzerland was not spared the rash of competing fascist groups arising in its midst.

The failure of these groups to mobilize mass

support speaks well for the democratic ethos of the Swiss polity. The National Front, with perhaps ten thousand members, generally did not receive more than two to three per cent electoral support, although it had pockets of strength in a number of conservative communities. Its fascist ideology, rallies, demonstrations, anti-Semitism, and pro-German stance did not endear it to important segments of Swiss society, especially the workers. It was beset by leadership struggles, factionalism, secessions, and the competition of powerful conservative movements. Thus it could not survive the outbreak of the war and the neutralist policy of the government.

This volume, based on the author's doctoral dissertation, has its weaknesses. Glaus, so immersed in his subject matter, fails to put the National Front into the wider perspective of the Swiss party system or European fascist movements.<sup>6</sup> He fails to assess the impact of the Front on the Swiss body politic. He clogs the narrative with too many minutiae. But to the specialist on European fascist movements, the work represents a valuable addition to a topic of historical and contemporary significance.

GERARD BRAUNTHAL

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Amherst*

GIUSEPPE LO GIUDICE. *Comunità rurali della Sicilia moderna: Bronte (1747-1853)*. Istituto di Storia Economica, Collana di studi e ricerche, Number 3.) Catania: Università degli Studi, Facoltà di Economia. 1969. Pp. 337. L. 4,000.

Bronte in Sicily is primarily identified by the English-speaking peoples as the dukedom and domain on the western slopes of Mount Etna that, along with a diamond-hilted sword once owned by his father, a grateful monarch of the Bourbon Sicilies (Ferdinand IV) pressed on a reluctant Horatio Nelson in October of 1799. The admiral, who had gained the endowment for his aid in reducing the French-sponsored Parthenopean Republic, estimated for hypothecation purposes Bronte's annual revenue at three thousand pounds sterling.

Economic historian Giuseppe Lo Giudice chooses to study Bronte's land system and cultivation in the years from 1747 to 1853 "to offer a suitable analysis of some important aspects of

the island's agriculture and in particular of the pertinent changes that unfolded in its structure." It is a difficult subject, he confesses, but by investigating closely this farm and pasture district and by matching it with other districts, evidence may be assembled for meaningful new interpretations of Sicily's social, economic, and political history since the seventeenth century. The author did research at Bronte, Catania, Palermo, Florence, and in the Nelson archives; he displays numerous maps, charts, graphs, statistical tables, and a suitable bibliography. To aid the reader the old-fashioned terms of area, bulk, and coinage—from an age when news of winning numbers in the lottery still outdistanced reports of public events—are rendered into the metric system. What Lo Giudice concludes is that alodial or free property practically vanished and steady private usurpation of ancient communal rights discouraged growth and stifled initiative. The latifundia controlled by the nobility and especially by the new land-purchasing bourgeoisie increased, grain farming reduced grazing areas, mulberry trees yielded somewhat to garden vegetables, and the peasantry steadily became more miserable. The decade and a half from Nelson to the collapse of the liberal Sicilian constitution of 1812 allowed hope of serious land reform; this aborted, however, and small proprietorships never materialized. Pietro Burgarella, whose review of this book appeared in *Il Risorgimento in Sicilia* (6 [1970]: 224–25), hailed it as "a real contribution to knowledge of the economic and social life of the entire island." The undersigned concurs.

DUANE KOENIG  
University of Miami,  
Coral Gables

HOWARD R. MARRARO, editor. *L'unificazione italiana vista dai diplomatici statunitensi*. Volume 4 (1861–1866). (Istituto per la Storia del Risorgimento Italiano. Biblioteca scientifica. Second Series: Fonti, Volume 62.) Rome: the Istituto. 1971. Pp. 411.

For over forty years the late Professor Marraro faithfully and successfully delved into Italo-American relations. A prolific pioneer in this field, he contributed to it many excellent studies, monographs, and editions of documents.

Under the title above he published the diplomatic dispatches of the American envoys accredited to the court of the king of Sardinia, later of Italy, in Turin from 1838 on.

The present volume, the fourth, covers the period between 1861 and 1866, a perilous time for both the United States and Italy, when both countries were struggling to preserve their national unity against domestic strife and foreign intrigues. The dispatches, mostly from George P. Marsh, ambassador to Italy from 1861 to his death in 1882, reflect the concern of American diplomats in Europe: the unfriendly neutrality of the great powers, the dangers to American trade from Confederate privateers, and the questionable fidelity of many American agents to the Union.

Marsh felt remarkably free from most of these worries in his official post. Both people and government in Italy were unequivocally in favor of the Union, with the exception of a few clericals and reactionaries who were opposed to Italian unity as well. In contrast with the prejudices encountered in most European politicians, Marsh wrote of meeting several "Italian gentlemen, in and out of public life, who show a comprehension of our condition quite surprising in persons who never trod our soil" (p. 108). A comprehension, we may add, most likely due to a conscious or subconscious equation they must have made of the economical, social, and political differences between North and South in both countries—liberal equalitarian, industrial, better-educated masses in the North, in contrast with conservative, stratified, agricultural, unschooled masses in the South. This understanding through identification gave Marsh, too, a keen comprehension of Italian political temper, events, and prospects of the day. His analyses of the Roman question, the completion of Italian unification, and the widespread resentment at French tutelage of Italian life are remarkably sharp and clear.

The editorial lapses in this volume are few. One occurs on page 35 where Marsh writes of the "doubtful fidelity" of many "appointees of the late and Gen. Peirce [*sic*] administration," in obvious reference to the administrations of James Buchanan and Franklin Pierce. The editorial footnote on "Gen. Peirce" gives a biographical sketch of General Ebenezer Weaver

Pierce instead of Franklin Pierce; President Pierce, too, had attained the rank of general during the Mexican War. Another lapse is the erroneous insertion of the word *sic* after spellings that were common a century ago and are still sanctioned in the latest editions of *Webster's International Dictionary*: amical, apprise, colours, commonalty, connexion, diplomate, enrolment, guaranties, kilogramme, and visaed. Needless to say, these are minor, negligible flaws, mentioned here only for the sake of completeness. We only hope that the next and, presumably, the last volume of the series will be edited as accurately as the first four.

JOSEPH ROSSI  
University of Arizona

SERGIO I. MINERBI. *L'Italie et la Palestine, 1914-1920*. (Publications de la Faculté des Lettres et Sciences humaines de Paris-Sorbonne. Series "Recherches," Number 60.) Paris: Presses Universitaires de France. 1970. Pp. 297. 50 fr.

Italy evinced no political interest in Palestine until 1916 when the prospect of dismembering the Turkish Empire raised appetites everywhere. Even so, Palestine always remained subsidiary to Asia Minor as a sphere of Italian ambitions in the Near East. Nevertheless, after World I Rome worked consciously for an Italian presence in Palestine. Italian policy to achieve this was muddled, settling belatedly on internationalization—just in time to be thwarted by British insistence on a Palestinian mandate. Italy inevitably had to consider Zionism, although distrusting it as "a pawn in the British game." The most interesting contacts were made by Commander Levi-Bianchini, whose biography Minerbi terms "a side-product" of his research. The Zionists, understandably, were not captivated by sophisticated Italian overtures; nor were the Pan-Arabs who could not overlook the Libyan conquest. Italy's strongest card in Palestine was a claim to share in the administration of the Holy Places; in this she had Vatican support, although Minerbi suggests that it was less than others have maintained. But here, too, Italian hopes were frustrated, not by Britain but by France. All in all, Italy's role in Palestine was, perforce, marginal. Consequently, Minerbi sheds as much light on Allied disparagement of Italy, a cause

of the "mutilated victory" syndrome, as on Palestine itself.

Minerbi's account is based on thorough research, principally among unpublished records of the Italian foreign ministry. He also employs material from the British Public Record Office, Zionist archives, and private papers including those of Levi-Bianchini. There are abundant historiographical footnotes and a good bibliographical essay marred only by a list of secondary works apparently not proof-read; how else may one explain the citations D. Mac (*sic*) Smith and P. (*sic*) Chabod, and the dating of Frank Manuel's important article as 1938 (although given correctly in the text as 1955)?

Minerbi traces Italian policy toward aspects of the Palestinian issue in turn—Zionism, Pan-Arabism, the Holy Places, and so on. While not without intellectual validity, this organization involves backtracking and repetition and, more important, obscures the interplay between one factor and another. The continuity of argument suffers from too many one-sentence paragraphs. The book, in short, possesses the merits and demerits of most doctoral theses: solid research and sound information presented in somewhat indigestible form.

ALAN CASSELS  
McMaster University

*La Toscana nel regime fascista (1922-1939): Convegno di studi promosso dall'Unione Regionale delle Province Toscane, dalla Provincia di Firenze e dall'Istituto Storico per la Resistenza in Toscana, Firenze, Palazzo Riccardi, 23-24 maggio 1969*. In two volumes. (Unione Regionale delle Province Toscane, Biblioteca di storia toscana moderna e contemporanea, Studi e documenti, Number 8.) Florence: Leo S. Olschki Editore. 1971. Pp. xvi, 411; 416-787. L. 8,000 the set.

As Ernesto Ragionieri said in his closing remarks at this symposium on fascism in Tuscany held in May 1969 in Florence, Italian historians are beginning to be objective about fascism and are moving from a political to a historical judgment of it (2:476). The papers presented at this meeting and now made available in book form seem to support this. Partisan controversy is muted. The tone of both speakers and discussants, among whom were Stuart

Woolf, Adrian Lyttelton, and Max Gallo, exhibits little political bias. What emerges is a thoughtful and thought-provoking analysis of many aspects of fascism studied at the regional, rather than the national, level. With few exceptions, historians of Italian fascism have tended to look at it from a much broader perspective. Yet much may be learned from a close analysis of local developments. In this instance Tuscany was chosen for a number of reasons, primarily because, like Emilia, it played a key role during the important years 1919-22. In May 1922, for example, twenty per cent of all *fasci* and eighteen per cent of all fascists in Italy were concentrated in Tuscany.

As in any collection the contributions are of different caliber and scope. Some, like Giorgio Luti's overview of Florentine periodical literature, Mario G. Rossi's analysis of the Church's position, and Carlo Francovich's profile of militant Tuscan antifascists, indeed limit themselves to illustrating fascism in Tuscany. Others, like Giorgio Mori's informed study of industrial developments in this region and Emilio Sereni's somewhat polemical discussion of Arrigo Serpieri's views on agriculture and *mezadria*, tend to embrace Italy as a whole.

Two points emerge from a careful reading of these two volumes: that, despite the spate of books and articles on fascism that have emerged in the last twenty-five years, much still remains to be done; that now the most rewarding approach may very well be to study fascism in its local manifestations. While the shortcomings of such a type of investigation are well illustrated in some of the contributions to these volumes, the value of studies focusing on local or regional developments is also clearly shown by the total impact of the various papers. Taken together they represent an informative addition to the historiography of fascism.

EMILIANA P. NOETHER  
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HARRY FORNARI. *Mussolini's Gadfly: Roberto Farinacci*. Nashville: Vanderbilt University Press. 1971. Pp. xiv, 237. \$8.95.

There is general agreement among students of modern Italian history that fascism failed to achieve the totalitarian control of the state en-

visaged by some of its leaders. What is less well known, especially among the English-reading world, is that within the Fascist party itself Mussolini's authority was not always, if ever, completely total. Among those who challenged it successfully on occasion and who, even when forced formally to submit to the Duce, ever remained refractory was Roberto Farinacci, Fascist "boss" of Cremona, self-appointed custodian of a "pure" Fascist faith, and proponent of an ever-continuing totalitarian revolution. Mussolini's reputation for violence is based more on his rhetoric than on his personal acts; Farinacci was violent in words and deeds; and if such a combination be essential to fascism, the author is justified in calling the hierarch of Cremona the "archetypal Fascist."

That Farinacci was Mussolini's veritable gadfly for more than two decades is proved superabundantly by Fornari in a work based mainly on a wealth of archival and other primary sources. From this mass of material, well digested and clearly presented, there emerges a Farinacci more Fascist than the Duce, the most violent of the *squadristi*, and perhaps the chief protagonist of a totalitarian dictatorship. But with the regime well entrenched by 1926—in large part because of Farinacci's activities—and aspiring to respectability at home and abroad, there was less use for the goader who preached incessantly for more vigilance and violence. Hence Farinacci's relegation to his provincial fiefdom of Cremona where he could play the role of Duce on a reduced scale, but forever importuning Mussolini to proceed with a permanent revolution, to be answered by peremptory orders to behave alternating with affectionate letters of gentle persuasion.

All this is presented with exceptional clarity by the author, whose pioneer work suffers from one significant defect: in his account of the general background to the actions of his protagonist, Fornari relies excessively on two works, Luigi Salvatorelli and Giovanni Mira, *Storia d'Italia nel periodo fascista* (1964), and Renzo De Felice, *Mussolini il fascista* (1966-68). This criticism notwithstanding, Fornari's work may well serve as a model for future studies on other Fascist chieftains, such as Italo Balbo and Dino Grandi, whose autonomous power in their respective fiefdoms was one of the many

checks on Mussolini's allegedly absolute personal dictatorship.

SALVATORE SALADINO

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LEO VALIANI *et al.* *Azionisti, cattolici e comunisti nella Resistenza*. (Istituto Nazionale per la Storia del Movimento di Liberazione in Italia.) [Milan:] Franco Angeli Editore. 1971. Pp. 449. L. 6,000.

This informative book is the product of a conference held in Milan in November 1968 under the auspices of the Istituto Nazionale per la Storia del Movimento di Liberazione in Italia. Earlier conferences had dealt with methods of studying the Italian Resistance and its historiography. The 1968 conclave focused on the theme of the political parties in the Resistance. The authors of the essays have illuminated numerous hitherto somewhat obscure aspects of the internal development of three of the five parties that made up the Committee of National Liberation (CLN) and played an important role in Italy's Armed Resistance of 1943-45. These three currents were the newly formed Action party (Partito d'Azione), intransigently republican and liberal-socialist in its orientation; the nascent Catholic party (Democrazia Cristiana), successor to the defunct Italian Popular party that had emerged after World War I; and the greatly revitalized Partito Comunista Italiano, about to become the strongest Marxist force in Western Europe. The other two parties in the CLN were the Socialists, whose strength lay chiefly in the cities, and the Liberals, whose appeal was greatest in the south and who were the most conservative and monarchist-minded political force in opposition to fascism. It is regrettable that the planners of the conference did not include discussions of these parties as well.

The contributors to this volume were members of the parties listed respectively above. Moreover, they are professional historians. As such, they have generally tried to look back on the period with as much detachment as can be reasonably expected of "engaged" scholars after the passage of a quarter of a century. Although all the essays make contributions to our knowledge of the subject, that of Leo Valiani, who was northern secretary of the Action party dur-

ing the war, is the most satisfactory. He is the eldest and most versatile writer of the three. His essay emphasizes that the story of the origins of the Action party was also the story of the politicization of a large number of Italian intellectuals under fascism. The party had an important antecedent in the "Giustizia e Libertà" movement founded by Carlo Rosselli in 1929. Even more influential was the "Liberal-socialismo" movement founded by Guido Calogero a decade later, for it represented anti-fascism of a new style—one that side-stepped the problem of the Concordat and that broke ranks with Benedetto Croce in its willingness to advocate some degree of socialism. Indeed, nationalization was to be the issue that aroused most debate within the Action party after its birth in 1942. Valiani gives much credit to Ugo LaMalfa, the party's leader in Rome, for preserving a flexible stance on this issue. Valiani also has much to say on the party's opposition to the House of Savoy, as well as its sharp criticism of the about face of Palmiro Togliatti, who in March 1944 led the Communist party into Marshal Badoglio's government for the purpose not only of gaining prestige for the party but of promoting national unity. Though the Action party continued to play a role in the Armed Resistance that was second only to that of the Communists, internecine strife and inability to attract lasting support from working-class groups brought about its quick collapse as soon as the war came to an end.

Gianfranco Bianchi, a professor at the Catholic University of the Sacred Heart in Milan, does not write as persuasively as Valiani, but he sets forth considerable useful information regarding the various currents that coalesced into the Christian Democratic party.

Ernesto Ragionieri, youngest of the contributors, was still in his teens at the time of the Armed Resistance. Now a professor of history at the University of Florence and a specialist on Marxian socialism, he is editing the collected works of Palmiro Togliatti. He is also a member of the Central Committee of the Communist party and responsible for its review, *Critica marxista*. As might be expected, Ragionieri contends that the Communist party was a *partito nuovo* and the prime advocate of national unity in the Resistance. He strongly defends Togliatti's twists and turns.



The book is handsomely printed and contains a helpful index of everyone mentioned.

CHARLES F. DELZELL  
Vanderbilt University

F. ROY WILLIS. *Italy Chooses Europe*. New York: Oxford University Press. 1971. Pp. xiii, 373. \$8.50.

Professor Willis has written a first-rate volume on Italy's integration into the West European economic and political community. The author has wisely divided the book into two parts. The first ("The Process of Choice") is a lucid chronological review of early Italian ideas on European unity, Italy's postwar problems, alternative prescriptions, and the decisions to adhere to the Marshall Plan, NATO, WEU, and the European Communities. It is capped by a compelling evaluation of the results of the integration process. The topically organized second part ("The Choice Determined") explores the many internal factors that shaped the substance and consequences of those decisions. Here the writer skillfully relates such problems as Italian industrial and agricultural conditions, overpopulation, unemployment, migration, and the south to both the larger integration issue and the perspectives and goals of Italy's principal pressure groups and political parties.

This format permits Willis to achieve an enviable breadth of scope, though at the inevitable price of occasional redundancy. There is a great wealth of both primary and secondary documentation, but the inclusion of a larger portion of the statistical data in table form might have enhanced the book's readability. Also, by grouping most of his conclusions at the end of each part rather than in a final summary chapter, the author conforms to his stated objective of rendering each complete in itself but at the cost of disjunction. Nevertheless, Willis's convincing style and his meticulous concern for tightly organized chapters more than offset these structural deficiencies.

According to Willis there was a powerful and continuous interaction between the process of joining Europe and the ongoing domestic political struggle in Italy, one that mainly concerned the issue of economic and social reform. As he correctly points out, the creation of a

reformist-oriented Center-Left coalition in Italy owes much to the incontestable achievements of integration. Although the goals of the former have not been met entirely, Europeanism has ceased to be a political issue.

While Willis might have speculated further on the several unresolved economic and social questions he raises and refrained from unfairly castigating the Nenni Socialists for delaying internal reforms, the book must certainly rank as one of the really thorough and competent studies on contemporary Italy. As such, it will undoubtedly appeal to European generalists and Italian specialists alike.

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CONSTANTINE CAVARNOS. *Modern Greek Thought: Three Essays Dealing with Philosophy, Critique of Science, and Views of Man's Nature and Destiny*. Belmont, Mass.: Institute for Byzantine and Modern Greek Studies. 1969. Pp. vii, 10-115. Cloth \$3.50, paper \$1.95.

The author of this brief history of modern Greek thought, Constantine Cavarinos, is a distinguished philosopher-scholar, who traveled and studied in Greece and who published reviews, articles, and books covering a wide spectrum of Greek religious art, life, and thought. In the present volume, which introduces the reader to Greek thought from the middle of the eighteenth century to the present, Professor Cavarinos describes the philosophy of modern Greece (pp. 9-38), the role of the positive sciences in solving the problems of man (pp. 39-56), and the modern Greek view of man's nature and destiny (pp. 57-85). The book ends with seven pages of notes (pp. 87-93), thirteen pages of a selected bibliography of philosophical works (pp. 95-107), and two indexes—one of names (pp. 109-12) and one of subjects (pp. 113-15).

Chapter 1 begins with an analysis of the nine characteristics of modern Greek philosophy: existential orientation, personalism, idealism, primacy of philosophy over science, primacy of Christian teaching over philosophy, Christian eclecticism, relation to ancient Greek philosophy, independence from medieval Western philosophy, and independence of the phi-

losophies of the Middle and Far East. These sections, which could fill nine volumes, provide a valuable study of the relation of modern Greek philosophy to Byzantine, ancient, Western, and Oriental philosophy. Chapters 2 and 3 synthesize the attitudes of the Greek intellectuals to the value of the positive sciences and the nature and destiny of man.

The unique quality of modern Greek intellectual life is that in contradistinction to the West, which has divorced philosophy from religion, Greek thinkers have kept both and have allowed their Orthodox faith to guide philosophical inquiry. Professor Cavarinos makes this clear when he asserts that "modern Greek philosophy can best be understood as a continuation of Byzantine philosophy in modern times."

Although there was a continuous cultural life during the Ottoman occupation, it was not until the middle of the eighteenth century that the Greek Renaissance reached its apex in the Neo-Hellenic Enlightenment, which preceded the Greek War of Independence. This period witnessed an outburst of scholarship, education, intellectual inquiry, and professional philosophy. In this regard Eugenios Voulgaris (1716-1806), a notable intellectual figure and the "father of modern Greek philosophy," and Athanasios Psalidas (1767-1829), one of the foremost intellectuals of the Greek Enlightenment, were trained outside of Greece and transmitted the new spirit of rational inquiry, secularism, and the scientific method to their students. Voulgaris, in the tradition of Leibniz and Wolff, believed that truth could be attained by the scientific experimental method, but this did not prevent him from preaching that ideas are innate because of divine revelation. Psalidas likewise adhered to rationalistic doctrines; yet in his book on *True Happiness or the Basis of All Religion* (1791), he defends effectively the existence of God, the immortality of the soul, reparation after death, and human freedom.

Despite its brevity and at times somewhat challenging assertions, this book is not only thought-provoking but should become a fundamental part of present and future research on modern Greek thought.

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VLADIMIR DEDIJER. *The Battle Stalin Lost: Memoirs of Yugoslavia, 1948-1953*. New York: Viking Press. 1971. Pp. x, 341. \$8.50.

Since the Yugoslav expulsion from the Cominform on June 28, 1948, millions of words and countless numbers of articles and books have flowed from presses round the world. In spite of all the verbiage, however, the story of the celebrated Tito-Stalin feud has remained incomplete. Dedijs's new volume helps to close at least a few of the gaps.

*The Battle Stalin Lost* is a warm, moving, and captivating drama, in which the playwright is also one of the leading actors—to paraphrase a characterization from the brightly colored book jacket. It is as much a chapter from the life of Vladimir Dedijs as it is an account of the Russo-Yugoslav struggle of 1948-53. The life of the author, as revealed in the pages of this fascinating volume, has been one of both tragedy and triumph. He lost his first wife on a Partisan battlefield and a son to suicide. As a member of the Yugoslav Communist Party (YCP) he rose to the ranks of the elite and was entrusted with important posts in the party and the party-controlled state, including that of director of information, which he held at the time of the war of nerves that he so graphically describes.

In his narrative of the memorable conflict between Tito and Stalin, Dedijs points the accusing finger at the Soviet party boss. He feels that the leading role played by Russia in World War II went to Stalin's head and led him to formulate an ambitious plan in which all of Eastern Europe was to be brought under the Soviet yoke. Yugoslavia occupied a special place in those plans, and Stalin anticipated an easy victory. Once Yugoslavia was subdued, it would not be difficult to bring the other Eastern European countries under the hegemony of Moscow.

After a bit of preliminary shadowboxing Stalin launched his open attack on Yugoslavia with the Cominform resolution of expulsion. Out of his bottomless bag of tricks the Soviet leader pulled a number that he was certain would not miss. Included among those were "silent strangulation" through direct pressure, provocation of conflict within the Central Committee of the YCP; ideological excommunication of the YCP; public anathema through

the Cominform; an all-out economic blockade; and infiltration of the key positions in the Yugoslav army, security forces, YCP itself, and major ministries.

Though the odds were stacked against her, deserted as she was by the communist states and parties everywhere, Yugoslavia did not capitulate. Her success Dedijer attributes partially to the regional defense pacts and international assistance, especially the economic and military aid provided by the United States, England, and France; but "the decisive factor," he says, "was the unity of the peoples of Yugoslavia and the readiness of the Yugoslav army." However important all of those forces might have been in saving Tito's homeland, the critical observer of the international scene cannot help but call attention to the remarkable recovery of Western Europe and the possibility of the escalation of the local conflict into another global war as influences on Stalin's decision not to attack Yugoslavia.

Although Dedijer's effort is a distinct contribution to the fund of knowledge regarding this historic controversy, it must be noted that it is based solely on Yugoslav evidence, since Soviet sources remain inaccessible. The author incorporated a great variety of primary and secondary materials into his work. His use of diaries, memoirs, interviews, government reports, and archival materials lends considerable authenticity to the volume, but the absence of footnotes and bibliography often leave the curious reader hanging.

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VLADIMÍR V. KUŠÍN. *The Intellectual Origins of the Prague Spring: The Development of Reformist Ideas in Czechoslovakia 1956-1967.* (Soviet and East European Studies.) New York: Cambridge University Press. 1971. Pp. v, 153. \$8.95.

IVAN SVITÁK. *The Czechoslovak Experiment: 1968-1969.* New York: Columbia University Press. 1971. Pp. 243. \$10.95.

In contemporary Czech and Slovak thought the events of 1968 have acquired the status of a turning point comparable in importance with the Communist takeover of 1948, the dismemberment of the first Czechoslovak Republic in

1938, and, indeed, the foundation of the state itself in 1918. On August 20-21, 1968, literally overnight the Czechoslovak experiment was transformed from living reality into history, and some of its makers became its historians. As a collection of articles, speeches, and statements composed before August 1968 Ivan Sviták's book is a document in the intellectual history of the period; the part written after August attempts to explain what happened. Vladimír V. Kušín, like Sviták, lived in Czechoslovakia throughout the period he describes and interprets and, like Sviták, is one of the protagonists of his own narrative.

Kušín argues that there has existed "incongruence" between communism and the Czech "national disposition," which includes, he writes, national, democratic, and "heretically socialist" values. According to Kušín the takeover of 1948 and the introduction of the Stalinist system destroyed democratic political structures in Czechoslovakia but failed to eradicate those less tangible but nonetheless real elements of political life, such as traditions, national character, and personal and public attitudes. The Twentieth Soviet Party Congress (1956) provided a stimulus for the reassertion of the national tradition in a new environment. Kušín reviews the gradual emancipation from Stalinism that took place after 1956 among the jurists, philosophers, writers, literary scholars, and historians.

When it became evident, in the early 1960s, that the Czechoslovak economy was in serious trouble, the party was forced to emancipate the economists. The "rehabilitation" of sociology (a discipline suppressed under Stalin) and the birth of political science soon followed. Before long, fundamental questions of national purpose and existence were debated, in accord with the tradition established by the earlier generations. When Karel Kosík analyzed "The Czech Question" in a series of articles on "Our Present Crisis"—thus reviving the titles of two famous books written seventy years earlier by T. G. Masaryk—it seemed that the broken link with the past had been restored. In effect two programs for change emerged in Czechoslovakia. One, the subject of Kušín's book, was the broad intellectual movement seeking a synthesis of socialism, democracy, and European cultural tradition. (Kušín views Stalinism as a

product of Russian tradition and to him the Czech reform signified a move away from Russia and back to Europe.) The other was the more limited trend within the party itself. With the elevation of Dubcek to party leadership in January 1968 these two trends merged into a single movement aimed toward a new model of socialism.

Kusín is lucid and well informed, but he idealizes the reformers and their predecessors. His treatment of the Communist problem in prewar Czechoslovakia and the Slovak question in general is rather superficial. He never considers the possible inner flaws or weaknesses in the Czech political "disposition."

Sviták, taking a broader view of the Czech experiment, sees in it a challenge not only to Soviet supremacy in East Europe but also to the Russian position in the multinational Soviet Union, which he compares with the tsarist empire. The suppression of the Czechoslovak experiment, he says, will further aggravate the nationality question in the USSR. And yet, though he is so disillusioned with the Soviet policies, Sviták asks if the intervention could have been avoided. He wonders if the reform movement should not have been more concerned with power and more moderate and self-restrained in its proposals for internal change. Citing the cases of Finland, Romania, and Yugoslavia he also asks if the intervention might have been prevented had the Czechoslovaks been ready to defend themselves. Ruling out "armed struggle against the aggressor was fatally wrong . . . what are armies for, if not to fight when their country is attacked by an enemy?"

The question of resistance brings us back to the Czech political tradition. The spiritual and moral character of the pre-August movement reminded Kusín of the "moral sincerity and genuineness" in the outlook of T. G. Masaryk. But this is a rather one-sided view of the founder of Czechoslovakia. Masaryk's decision in 1914 to organize armed struggle against Austria from abroad was fully consistent with his previous career and with his teaching that evil should be resisted with force. When Masaryk and his legions returned to an independent Czechoslovakia it seemed that his activism (if not "adventurism") was vindicated as an element of the Czechoslovak political tradition.

Subsequent events in 1938 and in 1948 did not conform with the Masaryk style of political action. The reader of Kusín and Sviták may wonder whether the Masaryk of 1914 or the Beneš of 1938 was closer to the Czechoslovak leaders in 1968. It is impossible to imagine a form of political protest more at variance with the moral precepts and personal example of Masaryk than the suicide of Jan Palach in 1969.

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RICHARD PIPES. *Struve: Liberal on the Left, 1870-1905*. (Russian Research Center Studies, Number 64.) Cambridge, Mass.: Harvard University Press. 1970. Pp. xiii, 415. \$10.00.

With the exception of the Bolsheviks, the pre-1917 generation of Russian leaders has been cheated of historical recognition and evaluation; the efforts of emigré and Western scholars have hardly been able to offset the Soviet scorn. Professor Pipes's two-volume biography of Peter Struve, one of the greatest Russian intellectuals of that period, is therefore all the more welcome.

Drawing the dividing line at the point of Struve's return to Russia in the wake of the October Manifesto, the first volume (the second, one gathers, is well charted) covers Struve's intellectual pilgrimage as a publicist, scholar, and political thinker from the vague liberal patriotism of a precocious and largely German-educated youngster to Marxism and hence to radical liberalism. Professor Pipes has taken great pains to reconstruct the details of Struve's personality, his style of life and work, and above all his thought, faithfully plowing through the complex and abstract arguments—so exciting at the time and so musty and dated now—by which Struve established his reputation. He has done so with admirable command of the relevant schools of philosophy as well as of the shifting ties and alliances among the revolutionary intelligentsia. As an act of scholarly loyalty to a great Russian liberal, written in an eminently knowledgeable style with an occasional touch of iconoclasm, this volume is highly successful. There is available now to Western scholars a statement, as precise as they will ever get, of Struve's contin-

uously evolving thought, with page after page of direct quotation in readable translation with appropriate commentary—all based on the most thorough compilation of Struve's writing (listed in the bibliography).

From a methodological point of view the volume represents the self-consciously nominalist (or positivist) scholarship so prevalent in Russian studies. Its attention is brilliantly focused on the documentable foreground. Yet for that very reason it lacks depth; it does not cover with similar perception the various backgrounds that complete the historical portrait. Backgrounds intrude, but in the blurred form of generalizations that smack of one-sidedness or bias. Marx's attitude toward a separate path for Russia, for instance, would seem to be more hesitant and complex than one gathers from the author's account (pp. 46-47). Did, furthermore, in the perspective of a hundred years the course of Soviet development follow the pattern predicted by Chuprov and Vorontsov or rather that of the Social Democrats (p. 43)? In the Russian setting of those times—viewed in totality and not from the Struve perspective—did Lenin's offhand remark that Struve "ought to be killed," justify the author's stricture, in a quotation from Santayana, that Lenin had forgotten his vision of a better society (p. 279)? The four-page introduction to the chapter on "The Philosophy and Politics of Liberalism" likewise seems too facile; it does not, incidentally, do justice to Bernard Pares.

Why, above all, should the present generation of students and scholars be content with viewing Russian reality through the eyes of Struve? If proof were still needed, this volume supplies it: the Russian intelligentsia, even in as alert and learned a thinker as Struve, had very little sense of the problems confronting the men in charge of governing the Russian Empire in a competitive world order. The Russian intelligentsia lived in fragile, forever redesigned glass houses of illusions derived from Western European experience—a fact that a strictly nominalist approach will not catch. Should we not first realistically assess the condition of the Russian Empire before proceeding to sort out the factual minutiae?

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## NEAR EAST

GEORGE M. HADDAD. *Revolutions and Military Rule in the Middle East*. Volume 2, *The Arab States*. Part 1, *Iraq, Syria, Lebanon and Jordan*. New York: Robert Speller and Sons. 1971. Pp. 587. \$12.50.

This is the second of a three-volume series by Professor Haddad on this subject. Volume 1, *The Northern Tier*, gives a general background of the role of the military in the Middle East throughout history and then deals in detail with Turkey, Iran, Afghanistan, and Pakistan. A planned volume 3 will cover Egypt, the Sudan, Yemen, and Libya.

This volume begins with a general background chapter in which the author brings out several important facts often forgotten or overlooked. Of particular importance is his explanation that since Jordan and Iraq were British creations and continued their strong ties with London after World War II they were constantly at odds with their Arab neighbors. While never forgiving the British and French for fragmenting geographical Syria, the foes of the Hashemite monarchy (especially Egypt) could not bring themselves to allow any realistic union of the Fertile Crescent states because they believed it would have been Hashemite and British-dominated. Also, Syrian leaders feared that they would lose their positions.

Haddad brings out the long history of alliances between the politicians and the military in nearly all Arab countries, except the Muslim Brotherhood in Egypt. Of interest is the fact the first modern Arab state to become independent (Iraq in 1932) was the first in which the army officers entered into politics and carried out a coup (1936). The author makes a comparison of conditions in Iraq under the royal regime in 1958 and after nearly five years of Kassem's rule. By the time Kassem was overthrown in 1963 the country was more disunited, economically stagnant, diplomatically isolated, and more authoritarian than under the monarchy. Since then under Ba'thist rule little has changed. There have been nine coups since the 1958 revolution, with each coup leader accusing his predecessor of tyranny but none daring to establish democratic rule where the people could have a voice.

In his treatment of Syria, which he knows

thoroughly, Professor Haddad emphasizes the fact that Syria's political instability stems in large part from its geographical location, which has made it an arena for contending Arab nationalist forces. This situation was further complicated by Nasser and other Arab nationalists portraying the Syrian leadership as the enemy of reform. It is the author's contention that the common ideas—that since Syria is a mosaic of nationalities and religions it lacks national unity—is wrong. He stresses that the country's ethnic minorities are too dispersed and too small to cause disunity. He rightly points out that since the Ba'thist takeover in 1963 sectarianism, especially Alawite and Druze, has become more intense. An important point that Haddad brings out is that army officers in the Arab states did not participate in the struggle for independence in the period between the two world wars. Of particular note is that the Syrian army did not form a cast by itself and that army intervention in civilian affairs in large part was brought on by civilian politicians seeking the support of army officers. Many coups reflected the struggle for power between contending military factions supported by civilian personalities and parties or by other Arab states.

Lebanon's army, on the other hand, has played the role of guardian of the constitution. It was the determining factor in the ouster in 1952 of President Khouri, who misruled the country. The army forestalled an unpopular second term for President Chamoun by refusing to give him backing in the Civil War in 1958. The Commanding General, Chehab, then succeeded Chamoun not by a coup but by vote of a parliament whose majority had been favorable to Chamoun.

The turbulent Jordanian monarchy has long been supported by its army. Buffeted by outside influences, particularly Nasserism, Jordan has had its share of army conspiracies, but the Hashemite dynasty has managed to survive, largely because of its strong ties to the Bedouin tribes whose members compose the backbone of the army. The regime's victory over the fedayeen in 1970-71 was due almost entirely to the army's loyalty and its antagonism toward the radical Palestinians.

Professor Haddad, former chairman of the department of history at the Syrian University

in Damascus, was an on-the-scene observer during Syria's most turbulent era, and he gives a number of valuable insights into that period. His sympathies lie with those Arab leaders who fought for their country's independence. He gives them greater benefit of doubt than may be their due. It is my belief that they failed to meet the test of their time. That is certainly not to say that their military successors in Syria and Iraq have done better; they have failed miserably. Certainly neither country is better off for all the repression and killings that these military leaders have brought with their coups. The author has utilized a wide span of sources. However, in my opinion, a number of statements extracted from these sources are inaccurate, and many are hearsay.

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#### AFRICA

*Colonialism in Africa, 1870-1960. Volume 2, The History and Politics of Colonialism, 1914-1960*, edited by L. H. GANN and PETER DUIGNAN; Volume 3, *Profiles of Change: African Society and Colonial Rule*, edited by VICTOR TURNER. New York: Cambridge University Press. 1970; 1971. Pp. x, 563; viii, 455. \$17.50 each.

With these volumes we find an ambitious publishing enterprise at midstream; *Colonialism in Africa* will eventually run to five volumes. The first two deal with history and politics (1870-1914 and 1914-60), the third with social change throughout the colonial period. The fourth is to treat economic development, and the last will be a guide to documentation. At this juncture, in reviewing volumes 2 and 3, we are afforded the opportunity of comparing the perspectives of authors of varied backgrounds, although predominantly from the disciplines of history and anthropology. Each of the twenty-six contributors has his own understanding of colonialism and the colonial experience, and the editors, Lewis Gann and Peter Duignan in one case and Victor Turner in the other, acknowledge that general conclusions cannot be drawn from these collections.

While there are some stimulating syntheses and a few polemics all too many contributions are complacent summaries of dated materials and interpretations, and too few exemplify the

liveliness of contemporary scholarship about the colonial epoch. Victor Turner in his introduction to the social change volume reflects disappointment at the disarray of his volume and makes some stimulating remarks about history and anthropology as distinct disciplines in need of exchanging insights and data. His contributors are almost without exception eminent names in their fields, but they lack the appreciation of many younger anthropologists that the use of historical documents may afford valuable insights not available through oral data or static models. Historians will be disappointed, too, with the continued vagueness of citation; we are more often referred to other anthropological works than to the empirical sources.

Whereas Victor Turner obviously regrets the conventional methodology and the particularistic effect of his volume, it is to a degree inevitable, he suggests, that anthropologists would take the part of local societies, which are often at odds in their priorities and sensibilities with the central institutions of the state. Gann and Duignan, who are the editors of volume 2 and also the general editors of the series, have as their focus the center. History and politics from 1914 to 1960 is presented in terms of administration and constitutional development, elite formation, and institutionalization of colonial premises. Colonialism, as mediated through the central organs of government and guaranteeing a certain order and dominance in social, economic, and political terms, begs for definition and for a more dynamic approach to historical processes than this volume affords. The editors have a manifest political commitment to view colonialism and white societies as culturally heroic in goading, coaxing, and engineering Africa toward modernity. This commitment assumes the dimensions of counterinsurgency. The very succession of the volumes, from history and politics to social change and economics, seems to be calculated to forestall rival syntheses. The record of colonizers and the experience of the colonized, it is implied, may be understood without economic and social analysis.

Elite formation and the institutionalization of colonial premises are topics treated in both volumes. Elites in Africa have an especially ambiguous history, growing up as they did rela-

tively alienated from indigenous cultures and frequently rejected, on racial grounds, by the European community. The raised and dashed expectations of the *évolué* group in the Congo is Roger Anstey's main theme, which he treats with the guiding assumptions that European policy alone determined the fortunes of these aspirants.

Two chapters in the second volume make useful and stimulating general statements about elites. Martin Kilson distinguishes the upper echelon from the subelites, drawing mainly upon West African examples, with Ghana as the archetype. Immanuel Wallerstein, a political sociologist, is unique less for his fine and original synthesis on changes in social structure than for the fact that he alone of the authors in either volume has read and absorbed recent historical writing. The issue is not merely one of perspective. Neglect of this literature by others means that they do not benefit from the range of new sources that have been consulted in the preparation of many recent historical works.

Elite formation is one indicator of the institutionalization of colonial premises and class differentiation in the colonial context. Other ways of approaching social structure are also represented. Michael Banton describes the patterns of occupational discrimination and race correlations in urban South Africa, while Hilda Kuper points out the variations in color perception among the Swazi. The Swazi, she argues, did not originally see white and black in oppositional terms. The closer the person to Western culture and patterns of thought, the greater the tendency to racist thinking, both reactive and submissive. A third position is taken by Max Gluckman, who in a very personal essay relates and elaborates his theory of social interdependence in Southern Africa where, in his view, the values of the dominant minority are so internalized by the majority that radical action becomes virtually impossible. Whether or not one agrees with Gluckman, he has been an important contributor to the literature of Central and Southern Africa for some four decades. He is now busy defending and to a lesser extent moderating his views, and it is valuable to have such an explicit statement of conviction.

Something has already been said about the

stance of Gann and Duignan. They contribute extensively to volume 2, with an introduction, a chapter on the white elite of Rhodesia, and an epilogue. Students of colonial Africa will find much stimulation and incitement in these pieces. Nowhere is the attack upon African political talent so blatant as in the opening paragraph of the introduction, where in the guise of summarizing the period from 1885 to 1914, the editors condemn blacks as political incompetents. Clearly Africa was partitioned and claimed by European powers, but a more suggestive and objective thesis would consider as contributing factors the economic invasions of the immediate precolonial period and the technical insufficiency, to use Yves Person's phrase, rather than the political inability of Africans. We need to raise questions of politics in the light of such economic penetration and postcolonial perpetuation before evaluating the importance of the colonial experience in the total history of Africa. In their epilogue, Gann and Duignan point out a seeming difference of opinion between Jacob Ajayi, the African scholar who summed up the period to 1914 in volume 1, and Adu Boahen, who carries out a similar commission in volume 2. Whereas Ajayi saw colonialism as but an interlude in African history, Boahen emphasizes its importance. Adu Boahen's tempered but critical comments cannot be taken, however, as condoning the preoccupation of many chapters with the colonialists, and he deserves to be carefully read, preferably as an introduction rather than conclusion.

An example of a polemic from the Left is to be found in volume 3, where Aidan Southall sets out to show that old cities in Africa were undermined by imperialism. His thesis is illustrated by examples from early Portuguese contact with Kongo and Monomotapa, by some cities of Yorubaland, and by certain cities of North Africa in the premodern period. Southall has done fine work in rural and urban anthropology in Uganda, and the modern portions of his chapter are not so questionable, but in general his sweeping statement that "western imperialism had a negative and destructive impact upon many of those lines of development in African society that could have led towards the foundation of urban living properly rooted

in African culture" serves emotions more than scholarship. The interpretation of Kongo takes us no further than the work of Georges Balandier and does not benefit from the valuable critique of that work by Jan Vansina. The treatment of Monomotapa is based upon an altogether false association of the Great Zimbabwe with the dynasty that became puppets of the Portuguese. The Monomotapas had moved northward to the Zambezi in the century before Portuguese contact, and the history of Zimbabwe in the time of the Changamire Dynasty in the sixteenth and seventeenth centuries was only indirectly affected by the Portuguese. Fuller reference to archeological, architectural, ethnohistorical, and sociological data will make possible the kind of discussion that early urban history deserves. Meanwhile we are warned by the failures of this essay that on the basis of presently completed research, the topic does not lend itself to the demonstration of African-imperial confrontations.

A reviewer of such a varied collection is bound to dwell upon its shortcomings and striking demonstrations of commitment. Some very valuable and solid synthetic essays must also be mentioned in brief. They include James Duffy's beautifully composed review of Portuguese Africa, 1930-60, and Crawford Young's summary of (political) decolonization. An interesting contrast exists between C. G. Baeta's positive attitude toward missionary and humanitarian interests, 1914 to 1960 (vol. 2) and F. D. Welbourn's sharp attack upon missionaries as colonial collaborators (vol. 3). Readers will find rewards, according to their special interests, throughout the social change volume and each will react to the case studies on land, education, chiefship, or modern local party leadership according to his own needs and background.

*Colonialism in Africa* will certainly become a standard library reference series, but it falls short of being a classic in the sense of achieving balanced proportions. Classic syntheses will not be reached in the study of the colonial period in Africa as long as the major themes are too closely tied to alien dominance and research adheres to narrow understandings.

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## ASIA AND THE EAST

A. I. CHEREPANOV, *Severnnyi pokhod Natsional'no-revoliutsionnoi armii Kitaia (Zapiski voennogo sovetnika)* [The March to the North of the National Revolutionary Army of China (Notes of a Military Adviser)]. (Akademiia Nauk SSSR, Institut Narodov Azii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 303.

This book, the second volume of General Cherepanov's memoirs, is one among a number of solid reflections on China in the 1920s that have been published in the Soviet Union in the last decade. Cherepanov himself implies (p. 298) that the Stalinist strait jacket no longer need be worn by people who write on China, and the book as a whole is good evidence of this change.

It deals in firsthand and secondhand accounts with the progress of the Northern Expedition until shortly after the coup of Chiang Kai-shek in Shanghai in 1927, when the author, who had been a military adviser to the National Revolutionary Army, sailed home to Russia, wondering with his comrades what they had wrought in so ably building up the military arm of the Kuomintang. Apart from its value as military history it should be noted that the book carries on the posthumous rehabilitation of M. M. Borodin (at the expense, of course, of J. V. Stalin). Unlike Stalin, Cherepanov does not pretend to be a great political theoretician (his favorite word for the events he is trying to describe is "complicated"); but the fact that he was an eyewitness in China gives his interpretation great appeal, all the more so because many Comintern agents were lost in the purges of the 1930s.

In this respect the memoir of Cherepanov does much in contributing faces and feelings to that group of Bolsheviks who were, in their way, going out to save China quite as much as any American missionary. I suppose that its greatest value in the Soviet Union is its revival of that sense of "Leninist internationalism" so profoundly felt by these Russians. Today it may be of incidental regret that the mission in China miscarried (although the Russians can take heart that the Methodist and capitalist sides of Chiang Kai-shek have also been rather disillusioning); the more important reflection is that the effort was made, that the Soviet

Union gave more encouragement to the Chinese revolution than any other foreign power.

It should be noted, finally, that some of the Soviet works on China of this period have been translated into English and make fascinating reading for anyone wishing to compare the Russian involvement in China with our own. An informal translation of this book has already been done at Columbia University.

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JAMES CHIEH HSIUNG, *Ideology and Practice: The Evolution of Chinese Communism*. New York: Praeger Publishers. 1970. Pp. viii, 359. \$11.00.

JOHN WILSON LEWIS, editor, *Party Leadership and Revolutionary Power in China*. (Contemporary China Institute Publications.) New York: Cambridge University Press. 1970. Pp. viii, 422. Cloth \$9.50, paper \$2.95.

FRANK H. TRAGER and WILLIAM HENDERSON, editors, *Communist China, 1949-1969: A Twenty-Year Appraisal*. [New York:] New York University Press for the American-Asian Educational Exchange. 1970. Pp. xii, 356. \$7.95.

The ascendancy of communism in China, aside from disturbing the placidity of many minds, has given rise to the curious exercise of what is commonly called "China-watching," invariably from a distance. While the "China-watchers" presumably watch the same object, what emerges from their perceptions seems to vary greatly both in general pattern and in minute detail. Two principal factors have been responsible for this baffling phenomenon. On the one hand, the isolation of China for close to two decades has rendered an intelligent reading of the breathtakingly fast pace of change in China exceedingly difficult, for even carefully considered opinions have often been overtaken by the unfolding of new events. On the other, American response to a China in the throes of revolutionary frenzy has rarely been free from some form of emotionalism that tends to make a "watcher" see what he chooses to see.

The passage of time, however, has provided a clearer picture of the salient features of China in the Maoist phase. It has also had a sobering effect on those who saw in the emergence of the New China either the beginning of

the Communist millennium or immanent doom. The dramatic turn of events since mid-1971 has, among other things, opened the doors of China a crack, through which a steady trickle of Americans, admittedly selected, has gone in and out. Thus the exercise of "China-watching" is now complemented by "China-witnessing," which promises to bring the field of Chinese studies to a more sophisticated level.

Except for the recency of their publication (all three appeared in 1970) these books represent major different approaches to the study of China and Chinese affairs since 1949—a single author dealing with the crucial matter of ideology and practice, tracing the evolution of Chinese communism; a group of eleven scholars writing monographs for a conference in 1968 on various aspects of the equally crucial problem of Communist party leadership and mutations of power; and fifteen scholars and experts seeking to sum up their findings on a wide range of major subjects concerning China in the two decades of Communist rule.

In Professor James Hsiung's delightfully readable treatment of what is essentially the Maoist ideology and practice one sees the happy combination of political science expertise and an admirable degree of historical insight. Though divided into three parts, nearly half of the volume is devoted to the highlighting of major events since 1949, leading to the remaking of the party in the wake of the Cultural Revolution. His is a dispassionate examination of the key factor of ideology and the inner workings of political practice in consonance with that ideology. Through his analysis the tortuous path of zigs and zags along which China under Mao advanced and retreated has been traced and clarified. Historians will be most interested in the first two parts, dealing with historical and ideological perspectives respectively. Here Hsiung has subjected some of the earlier and more thought-provoking works in intellectual history and political sociology to closer scrutiny and has come up with ideas and observations that have the virtue of placing modern China against a balanced perspective. In other words he has succeeded in identifying the unique historical experiences of China in modern times without losing sight of either her long traditional legacy or the impact upon her of such alien ideologies as Marxism-Leninism.

Consequently, the very baffling events since the early 1950s through the Great Leap leading up to the Cultural Revolution and its aftermath can now be better understood, while Mao, the central figure, has emerged as neither a prophet nor a demon.

The work *Party Leadership and Revolutionary Power in China* grew out of collective labor under the sponsorship of the Contemporary China Institute in London. The eleven monographs are selected from the papers presented at a conference in 1968. Obviously a good deal of discussion must have gone on at this conference, presumably resulting in some consensus with respect to the general themes of this publication. Edited by Professor John W. Lewis, the book is divided into four parts, with three chapters in each of the first three parts and two in the fourth. The major problems investigated include the changing role of the Communist party, the power elite in theory and practice, party and Chinese society since 1949, and the new view of power in the Cultural Revolution. The topics cover a rather wide range spanning chronologically from the introduction of Marxism in China to the aftermath of the Cultural Revolution and topically from broad theoretical inquiries into legitimacy and the concept of power to specific institutional and operational analyses. One outstanding feature of this volume seems to be the unity within diversity. Each contributor has developed his thesis on his chosen aspect of the larger problem of leadership and power, and yet there is a thematic thread, as expressed in the title, that ties everything neatly together. This perhaps is partly attributable to the success of the conference format of joint scholarly endeavor but certainly to the rare ability of its editor, who provides a well reasoned and eloquently presented synthesis in the form of an introduction. Indeed, thanks to this group's efforts, those in the contemporary Chinese studies field are now "better prepared to grasp the full implications of how the Chinese Communist Party came into being in the first place and how Communist leadership and revolutionary power will develop in the future."

*Communist China, 1949-1969*, as its title indicates, is a twenty-year appraisal. Beginning with a historical overview under the chapter heading of "Communist China in the Light of

Chinese History," the ten chapters that follow deal with ideology, administration and control, party politics, the military, economy, agriculture, education, literature and art, the intellectuals, and the national minorities. The last three chapters discuss China and the Communist world, China's approach to the outside world, and United States-China relations. Each represents the considered opinion and judgment of a scholar whose competence is recognized by the editors, and each contribution stands by itself. In the absence of both an introduction and a conclusion one is inclined to believe that all thirteen chapters serve to provide the necessary background for the final one, which is essentially a policy position paper on United States-China relations. Although the editors are not primarily identified with the China field, most of the contributors are known for their sustained interest in the special subjects on which they write—Yuan-li Wu on economy, C. T. Hsia on literature, and Theodore Chen on education, to mention a few. There is a wealth of information and factual data, which may prove particularly useful to nonspecialists. Professor Rupen's chapter on the national minorities is perhaps the only comprehensive study on a long-neglected aspect of contemporary Chinese affairs.

The chapters dealing with the historical background, the ideology of Mao, the role of the army, and party politics form interesting parallels to parts of the Hsiung and Lewis volumes. They testify not only to the wide divergence of views regarding China but also to the variety of theoretical and methodological approaches adopted. As for the appraisal of the Communist record, the overall impression one receives from the Trager and Henderson volume is largely negative. It suggests that the ideological "baggage," the vast population, the sociopolitical instability, and the arbitrariness of party rule combine to pose insurmountable problems to the Communist leadership, which itself is suffering from acute disunity. Somewhat paradoxically the editors warn us: "Communist China today is not yet strong enough to challenge the world. But Peking does have the capacity to endanger the world with ruin."

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DAVID BERGAMINI. *Japan's Imperial Conspiracy*. New York: William Morrow and Company. 1971. Pp. xxxviii, 1239. \$14.95.

A Chinese general once told David Bergamini that "Japan was governed by an unworthy emperor who hoped to conquer the world." Resurrecting wartime passions and fanning embers of the Tokyo trials, *Imperial Conspiracy* might gratify that general. It is fitting that Sir William Webb, the Australian chief justice of the international military tribunal, provides an introduction. Bergamini's thesis, simply put, implies that high policy subverted justice by saving the emperor from trial as a major war criminal. "Hirohito," the author insists, "was a formidable war leader: tireless, dedicated, meticulous, clever, and patient. He had inherited from his great-grandfather a mission . . . to rid Asia of white men." The emperor "had not only led his nation into war by stamping military orders but, through his coterie, had also intimidated those who opposed him by conniving in bizarre Oriental intrigues, including religious frauds, blackmails, and assassinations." A Byzantine vocabulary stresses cabals, plots, and brotherhoods; 1,100 pages of text and 125 pages of notes and bibliography exude the smell of smoke.

First, to identify the author of this "strange" and "ugly" story (the adjectives are his): Bergamini was born in Tokyo and brought to China at the age of eight; he spent 1941-45 in a Japanese prison camp in the Philippines and afterwards attended Dartmouth, won a Rhodes scholarship, and joined Time, Inc. He has written a novel and books on Australia, the universe, and mathematics. Having "learned how to speak Japanese again, still badly, and for the first time how to read it—slowly," he returned to Japan in 1965. In Kyoto he enlisted an M.A. candidate and "an army of research assistants." The exposé was written in America.

How did scholars miss Bergamini's "disclosures" for decades? First, a "heavy halo of taboo" protects the throne. Second, perplexed by language and culture, foreigners miss things not called to their attention. Third, the emperor, courtiers, and army counterintelligence obfuscated or destroyed the record when doom approached. Bergamini admits reliance on circumstantial evidence in part; "in most parts of the Orient," he avers unconvincingly, "the idea

of a public record does not exist." He disdains gray economics and demography, savors genealogy and black-or-white biography. Many informants are privileged, supposedly because danger "still exists for those who speak too freely in modern Japan." Bergamini nevertheless maintains that he has "invented nothing but . . . put together much."

Spasmodically organized and crammed with gossip, the book plunges into the era of Sato and Mishima, far beyond the prospectus. The countless nicknames are unnecessary and vulgar; for example, "Peerless Pimp" Tatekawa and "Mad Dog" Cho. The text abounds with non-negotiable errors. In one segment on which I am an expert (Changkufeng, pp. 691-96), twenty-one factual mistakes appeared. Numerous references sampled throughout the book were erroneous, and the otherwise excellent and up-to-date bibliography contains considerable padding. It is surprising that the declassification of General MacArthur's histories eluded the author.

Most upsetting is the selective, misleading use of sources to buttress a tortured thesis wherein accidents are inconceivable, honest mistakes improbable. The object is to incriminate the emperor personally in every crime and aggression, from long before the rape of Nanking, by the technique Charles Elliott calls "the flypaper gambit." A particularly flagrant instance derives from the *Sugiyama* [Gen] *Memo-randa* (Tokyo, 1967), incorrectly characterized as the general's own daybook jottings. This source, Bergamini claims, revealed that about a year before Pearl Harbor the emperor "personally ordered" Onishi to conduct "a secret evaluation . . . of the feasibility of a surprise attack." The ungrammatical original mentions Admirals Yamamoto and Onishi but not the emperor and not an imperial order. Bergamini, however, consulted a translator of the *Tale of Genji* who "states flatly that the expression . . . is absolutely unambiguous in this context and means 'the Emperor ordered.'" This forced interpretation misunderstands the chain of command. The emperor would not send personal orders to Onishi, a junior rear admiral, down the ladder. Navy sources confirm it was Yamamoto who in January 1941 dispatched a very private communication (*shiteki shokan*) to Onishi. The flypaper does not smudge the em-

peror but, having deduced a "major historical revelation," Bergamini erects upon it an edifice of imperial culpability and prevarication. Was it not Alice who stamped her foot three times and made it so?

For each source called upon to indict the emperor's cruel power, another could have been invoked to depict the circumscribed monarch's mildness and lifelong devotion to constitutionality. He could act to stop the war in 1945, the emperor has said, only because Prime Minister Suzuki formally requested his opinion. The only other time the emperor acted in his own right was during the 1936 mutiny, when he feared the premier was dead. The monarch freely admits that things occurred about which "I really do not know personally."

The author thinks he has uncovered a vast scoop; Morrow should have known better. Still, it would be wrong to suspect a hoax; Bergamini, I am convinced, believes fanatically in his theory. Nor is crowd-pleasing popularization at issue; what distresses is that the author did not aspire more modestly. Endowed with energy, imagination, and time, he has collected immense amounts of detail. Presented objectively, without overstatement, they might have convinced us that the emperor was better informed, more industrious, and more influential than believed. Instead, Bergamini has brewed a concoction that is extremely disturbing, but not for the reasons intended.

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J. P. SHARMA. *Republics in Ancient India: C. 1500 B.C.-500 B.C.* With a foreword by A. L. BASHAM. Leiden: E. J. Brill. 1968. Pp. xvi, 278. 51 gls.

In the wake of a strong nationalist movement in India K. P. Jayaswal wrote his *magnum opus*, *Hindu Polity* (1924), in which he emphasized the importance of republics in ancient Indian history, implicitly arguing thereby that the Indian people, as in their past history, were capable of ruling themselves democratically without the British imperial tutelage. Several monographs with variations on Jayaswal's theme have been written since 1924 by eminent scholars such as D. R. Bhandarkar, R. C. Majumdar, B. K. Sarkar, A. S. Altekar, and U. N.

Ghosal. All these authors, however, treated the subject of republican institutions as a subdivision of their main study of the political institutions in ancient India. Dr. J. P. Sharma in his book mainly concerns himself with the study of northeastern republics that existed in the age of the Buddha. Fortunately, Dr. Sharma has no "nationalist" axe to grind, as was the case with the previous authors dealing with republics in ancient India.

By thoroughly examining the terms, such as *sabhā*, *saṃiti*, and *vidatha*, having political connotations, Dr. Sharma sees the origin of Indian republics in the Vedic age. He conceives of four types of republics. Although monarchy was the usual form of government in the Vedic period, Dr. Sharma attempts to construe *sabhā* and *saṃiti* as *de facto* political communities, occasionally acting independently of a monarch and at other times having an effective voice in the decision making of a monarch. While convincingly refuting the view of Professor R. S. Sharma that *vidatha* was the earliest folk assembly, the author appropriately states that it was merely a local congregation convened for religious activities.

Having discussed the Vedic origin of Indian republics, the author examines in detail the northeastern republican tribes such as the Licchavis of Vesali, the Videhas, the Sakyas, Nayas, Mallas, and Koliyas. Each of the republics is described in terms of their name and etymology, geographical location, origin, forms of government, and so forth. The author gives the Licchavis and Indo-aryan origin, deriving their etymology from the Sanskrit word *Ṛkṣa*, meaning bear. The migratory routes of the Licchavis, before they settled down in Vesali, have been outlined. In dealing with the Videhas the author objects to the generally held view that the king Ajātasattu was born of the Kosalan princes. However, in the Pali text itself, of which the author seems to be unaware, the king Pasenadi of Kosala acknowledges Ajātasattu as his sister's son. In the absence of any evidence whatsoever I must take objection to Dr. Sharma's rather otiose suggestion that "the Licchavis seized Videha from some weak king after Janaka without much conflict and established a republic on the ruins of the old monarchy" (p. 149). Despite the overwhelming textual evidence that the Sakyas were the tributaries of Pasenadi, the

king of Kosala, Dr. Sharma, disagreeing with it, says that they were independent political communities during Kosalan's regime. On page 92 of his book the author makes a sweeping statement that "there is no clear evidence of true totemism anywhere in the whole Vedic and Buddhist literature," while on page 220, contradicting his own statement, the author says that the Moriyas "looked on the peacock as their totem."

Dr. Sharma's book at its best is a useful and detailed collation of available materials on the republics at the time of the Buddha. However, he is a bit overzealous in sharply criticizing the views of the other scholars (see pages 6, 9, 11, 14, 81, 102, 119); some of his own suggestions, too, based as they are on the same insufficient and fragmentary evidence used by the other scholars, are wide open for different interpretations. Certainly a final word as to the nature of republican institutions in ancient India has yet to be written; Dr. Sharma's book has made us aware of the difficulties inherent in studying them during the period from ca. 1500 B.C. to 500 B.C.

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STEPHEN P. COHEN. *The Indian Army: Its Contribution to the Development of a Nation*. Berkeley and Los Angeles: University of California Press. 1971. Pp. x, 216. \$7.50.

Professor Cohen emphasizes in his introduction that this book is not a history of the Indian military. It is "a study of the development of a modern army in South Asia, and its relationship to its own political and social environment." Historical data are employed from a perspective "informed by contemporary theories of military organization and nation-building." Fair enough; but for many historians Cohen's distinctions about what is and is not history are perhaps too subtle. This is clearly not a conventional military history, but it is just as clearly an exceptionally fine piece of historical scholarship. In its discussion of the army's role as an instrument of British power and its new role as the military arm of a secular democratic state, this book makes a major contribution to the South Asia literature. The contribution is twofold: it provides for the first time a consideration over time of the changing

social character of the army; second, and perhaps more important, it clearly spells out the relationships between the military and politics in South Asia and thereby acts as a corrective to much modern political history, which has seriously failed to account for the important influences of the army in the subcontinent. (If the book helps lay to rest the myth that the idea of nonviolence has been the dominant operating factor in modern Indian politics, its contribution will be even more profound.)

The author organizes his ideas around several basic themes, but he emphasizes the changing perceptions of the character and role of the army in the British Indian period and in the new India. For me, five of the seven chapters in this concise volume are especially revealing. Chapter 2 on recruitment and ideology examines the organizational integrity of the British Indian army, the development of an able officer corps and an effective army of soldiers, and the idea of the "martial races" that bound the army into a loyal and committed whole. Chapter 4 touches all too briefly on defense problems and the nationalist movement, defining precisely the points at which personalities and ideologies in the recent nationalist period responded to the military. The reactions of Tilak, Subhas Chandra Bose, Gandhi, and Nehru and the impact of these reactions on subsequent attitudes and policies require more than the twenty-six pages Cohen has given us; they require another book. But this is true of most of the chapters in this volume and it indicates the richness and importance of Professor Cohen's work. My informal check list of research problems suggested in these pages has a dozen notations.

Taken together, chapters 5, 6, and 7, dealing with the professional officer, World War II, and the army after independence, provide the book's major statement on the training and quality of the growing number of Indian officers before and during the war, their significance as a leadership pool during and after the war, and their function as a force for national integration in the years of independence.

This integrative role of the army derives from two sources. First, since 1962 the Indian army has doubled in size and has perforce become more egalitarian, reflecting thereby the democratic sentiments of India's political lead-

ership (though the army also continues to recruit from those classes, castes, and regions on which it has traditionally drawn, a procedure Cohen aptly identifies as the "secular theory of the martial races"). The second factor contributing to the integrative role of the military is its function in free India as the nation's defender against external enemies. The shock of the 1962 China war contributed drastically to this new Indian conception of its army, and it is fair to say that the success of the army in the 1971 December war against Pakistan is for India the culmination of this process. Military might has become for India a symbol of nationhood, a perception that Mrs. Gandhi articulated in specific terms vis-à-vis the United States and the Soviet Union following the December war.

The creation of Bangla Desh and the restructuring of power relationships in South Asia which that event has effected involve the military in a profound way. Something of the extent and nature of the impact of India's involvement has been suggested. For Pakistan the consequences for the military and for the nation must be even deeper. Professor Cohen's excellent book helps us understand some of these new circumstances. For an explanation of others we await his further scholarship.

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## AMERICAS

PAUL A. ROSSI and DAVID C. HUNT. *The Art of the Old West: From the Collection of the Gilcrease Institute*. New York: Alfred A. Knopf. 1971. Pp. 335. \$30.00.

This lavishly illustrated volume was envisioned as a descriptive social commentary that "would offer the reader a broad spectrum of activities concerning the development of the Old West." The themes around which the paintings, sketches, and sculptures are clustered are obvious ones: the wilderness, Indians of the Plains, life on the Missouri, trappers and traders, troopers, the cowboy, and others. Even so, the artists themselves dominate the book. While the illustrations were selected primarily for their value as a historical record rather than for esthetic reasons, most of the artists included here saw the West through European

eyes and training. The result is a near-legendary golden West. The authors feel that since the majority of those "who documented life on the American frontier at whatever time and for whatever purpose, perpetuated a romance . . .," the artists were therefore being "realistic" by painting in a romantic vein those subjects that interested the public most. They have a point, for the paintings reveal a recurrent theme of violence in the form of Indian warfare, cowboys and guns, and the slaughter of wild game.

Beyond this the paintings prove to be limited social documents, for although the authors analyze weapons, clothing, saddles, and other items described in the more accurate illustrations, there are almost no paintings of Indian society (Catlin's are the great exception), frontier family life, railroading, towns, and women, omissions that the authors themselves lament. Ironically the twentieth-century paintings of Indians by Woodrow Crumbo and of the Old West by W. R. Leigh, a Gilcrease favorite, add the qualities of nostalgia and sentiment to that of romance.

Many of the selections from the five thousand works of art housed in the impressive Gilcrease Institute collection at Tulsa are by such well-known artists as Catlin, Bodmer, Miller, Bierstadt, Remington, Russell, and Seltzer. Of the sixty artists included at least two who are relatively unknown stand out as valuable rediscoveries. The first, Charles Bécard de Granville, was a seventeenth-century French-Canadian cartographer whose unique sketches of animals anticipate some of Audubon's humorous drawings, while Granville's renditions of people demonstrate a primitive style that almost seems to reflect the influence of Indian skin drawings. The second, William de la Montagne Cary of Tappan, New York, toured and painted the nineteenth-century West for the fun of it. Nevertheless his sketches capture a more sober and mundane West of sleeping roustabouts on riverboats, Mormon haymakers, a lonely campsite, towns, and ordinary people.

Rossi and Hunt have tried to place the illustrations in context by providing a running narrative of Western history in each section. But the artists' emphases on Indian warfare, buffalo, cowboys, and dramatic events force the narrative to be lopsided and often superficial. Still the authors never lose sight of the fact

that the "painter's West" was neither the real nor complete West but rather "as much a state of mind as Boston." They also explicitly reject the Turner frontier hypothesis in their account, although the revelation that the artist has romanticized wilderness and frontier America for four centuries tells us much about the profound impact of Rousseau and romanticism on European and American culture, and even helps explain the origin and perennial success of the Turner thesis itself.

Less complete in terms of biographical information than Robert Taft's *Artists and Illustrators of the Old West, 1850-1900* (1953) and less focused than Pauline A. Pinckney's fine *Painting in Texas in the Nineteenth Century* (1967), this volume is a valuable addition to the growing list of good art books concerned with the Western scene. Except for the fact that the pictures of sculptures did not reproduce well on slick paper the book is a visual feast, and the Gilcrease Institute deserves every praise for publishing such a representative cross-section of their rich holdings.

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BEN B. SELIGMAN, *The Potentates: Business and Businessmen in American History*. (Two Centuries of American Life: A Bicentennial History.) Reprint; New York: Dial Press. 1971. Pp. 402. \$3.95.

Whether or not the bicentennial celebration of the nation's independence pulls itself together and "comes to earth I know not where," it has already stimulated publishers and authors. One such response plans a bicentennial series of thirty volumes on a topical basis and here presents Mr. Seligman's history of American business and businessmen. His primary strategy has been to assemble a roster of approximately 525 businessmen and to divide into four time periods their "antics" and those of the governments with which they worked and which they corruptly controlled. The lines between these eras are not sharply drawn, for history is a "seamless web" (p. 4). The first era extends from colonial days to the Civil War; the last three periods extend from Reconstruction to the present day. This organization inevitably caters to *ad hominem* analysis and to anecdotal history. Perhaps without design, it enables the

last chapter, on the period since World War I, to differ somewhat from those that have gone before. The rhetoric becomes less relentless, and, if I understand a generally opaque style, "managerial interest appeared to focus on maximizing size rather than profit" (pp. 329-30). Perhaps the change in tone is due to a reliance on a different bibliography. Hitherto, though reluctant to be a muckraker, Seligman apparently based his analysis upon Gustavus Myers, Henry D. Lloyd, and Matthew Josephson. More recently, as a labor-union bureaucrat, he seems to have come in contact with more sophisticated sources, like *Fortune*, the *Wall Street Journal*, and John Kenneth Galbraith.

Be that as it may, Seligman's preface states his fundamental convictions: "Business cannot survive without profit. . . . The search for profit is axiomatic." Though the author has argued the matter with friends he still stubbornly believes that "America is predominantly an acquisitive society" (p. 4). At no time does he define "profits" or show any appreciation as to how variously they operate within our culture. To him profit is greed, it is self-interest. "Wealth was equated to virtue" (p. 66). Profits were the more censurable because they "came easy" and were usually "enormous" or "handsome." Finally, save the mark, he writes, without a touch of humor, a man "could pick a fortune from the branches of trees" (p. 115).

For one thing, to take a concrete example, the presence and practice of business philanthropy challenges this framework. The author resorts to the stereotype that philanthropy was a penance for a guilty conscience. When he comes to a subtle case like Carnegie his explanation is an awful mixture of twisted chronology and omits relevant considerations. Above all he pays no attention to what businessmen or their captive journalists and politicians said, for they were all hypocrites and deceivers. Much better to fall back upon the language of the demagogues. By and large the traction interest was dominated by financial syndicates who "for twenty-five years behaved like fabled vampires living on human blood" (p. 158), or when workers died in Guggenheim's enterprises, "the Moloch of capital accumulation demanded their sacrifice" (p. 193).

Inflexible in its determination to make a case at all costs, enmeshed in clichés and con-

traditions, omitting or treating shallowly whole areas of business experience, more at ease in the rhetoric and ideology of yellow journalism than with the thinking and moderation of scholarship, this book lacks the urbanity, the insights, or the balanced judgment to cope with its complicated subject. It is a work of little merit.

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VIRGINIUS DABNEY. *Virginia: The New Dominion*. Garden City, N. Y.: Doubleday and Company. 1971. Pp. xvi, 629. \$12.50.

In the tradition of Douglas Southall Freeman and David J. Mays, Virginus Dabney belongs to a sizable group of Virginians who combine an interest in the past with active participation in the present, to whom the study of history is an avocation, and to whom historical perspectives are natural, important, and assumed to be within the consciousness of others. As editor of the *Richmond Times-Dispatch* for thirty-three years, Mr. Dabney has been a close observer of the Virginia scene over the past four decades and a serious student of the state's history. In this book these interests and skills have merged, and it is important to the appreciation of the work to understand both Mr. Dabney's involvement in recent events and his scholarly interest in the Old Dominion's long past. So ingrained are historical perspectives in his patterns of thought that Mr. Dabney changes in this work almost imperceptibly from a narrator of the historical record to an observer of the contemporary scene. But this transition takes place as the narrative approaches the present, and assessments and evaluations become more common in relation to recent developments.

Mr. Dabney has aimed at providing a comprehensive survey of Virginia history, but the emphasis remains on traditional political history. There are important departures to introduce economic, social, cultural, and intellectual developments, and the author has made a conscious effort to avoid neglecting the role and contributions of blacks in Virginia history. But the account is basically political, and it is mainly chronological. Governorships, whether colonial or contemporary, provide the principal organizational framework. The narrative is factual, informative, and, while sympathetic, is



not a glorification of the Virginia past. The author clearly takes pride in his Virginia heritage, but he makes evident the deficiencies in that past, describes the backwardness of the state in the post-Civil War decades and the early twentieth century, and reports false turns in recent years.

In view of the great disparities in historical literature about Virginia, Mr. Dabney, who largely relies on secondary accounts except for the recent period, has produced a remarkably well-balanced survey with respect to chronological coverage. Historians have extensively examined colonial Virginia and the Civil War in Virginia, but they have given much less attention to other periods, especially the late nineteenth and the twentieth centuries. Mr. Dabney compresses Virginia's 169-year colonial experience into about 150 pages; his Civil War survey moves from Manassas to Appomattox in fifty pages; two hundred pages are devoted to the period since Reconstruction. In this last segment of the work the author has combined reliable scholarly studies with his own knowledge of the immediate past to provide a fresh summary. On the other hand, for the colonial period, though the accomplishment of synthesis and summation is commendable, the interpretations of leading historians are cited in an eclectic way, and there is no sustained interpretive evaluation of the colonial experience. Richard Morton's two-volume study of *Colonial Virginia* (1960) will be preferred by scholars interested in that early period. The coverage of the years since Reconstruction provides the most useful portion of the book, and Dabney is at his best writing about the period that he has observed at first hand. He is basically a political reporter and analyst, and the recent coverage is almost entirely focused on state politics and issues. Here he adds his editorial judgments and assessments.

Mr. Dabney has chosen the subtitle "The New Dominion" to display his conviction that "in almost every area—political, racial, educational, industrial, and cultural—Virginia has entered a new era." "The Byrd machine is defunct," he writes, "and the state's politics have become fluid and uncertain. The racial situation has been totally transformed" (pp. 580-81). He cites great progress in higher education, rapid industrial development, and an urban

population explosion. While he recognizes the achievements of Harry Byrd as governor in the 1920s, he reserves his highest praise for Mills Godwin, governor from 1966 to 1970, whose term Dabney sees as "one of a handful of truly exceptional gubernatorial administrations in the long history of Virginia, and second to none in modern times" (p. 575). Godwin, by abandoning Byrd's pay-as-you-go philosophy, getting a sales tax enacted, promoting constitutional revision, revitalizing education, and pressing other reforms, led Virginia "out of the rut in which she had been wallowing for decades" (p. 563).

The sparsely documented book is obviously designed for a wide audience, who will find an informative, interesting, and sympathetic survey of the history of the colony and commonwealth of Virginia and a knowledgeable assessment of its recent past.

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T. H. BREEN. *The Character of the Good Ruler: A Study of Puritan Political Ideas in New England, 1630-1730*. (Yale Historical Publications. Miscellany, Number 92.) New Haven: Yale University Press. 1970. Pp. xx, 301. \$10.00.

T. H. Breen's subject is the transformation of political ideas in early New England. His method is Bailynesque: pamphlets (including printed election sermons) are his major source, and, as he stipulates in the preface, he eschews "hidden desires" and presumes always that his authors meant exactly "what they said, no matter how outrageous their words might now appear" (p. xv). Expressed ideas are, moreover, directly related to events, for to Breen (commendably) ideas jog along behind action. The method dictates style. In narrative form Breen recounts events from the Winthrop migration through the political battling of Governors Dudley and Shute, interspersing in the narrative, through extensive quotations, the intellectual adjustments that follow upon events: the steady movement of the mind from a Winthropian unity, albeit marked by "family quarrels," to an unbridgeable division between "court" and "country," from a monolithic view of the magistrate as the Lord's vicegerent on earth once properly chosen by a covenanted

people, to dichotomous views of good rulers preserving the people's liberty and estates on the one hand, and, on the other, as gentlemen of culture and purse, wielding legitimate (that is, royal) power in the face of "democraticall anarkie" (p. 213).

The volume has its faults, certainly. Conscious of the difficulties of the "weasel word," "Puritan," Breen attempts to sidestep: "Puritan" in England, he stipulates, refers to the ministers of Haller's "spiritual brotherhood"; in the New England context "almost all Congregationalists" are to be considered Puritan (p. xviii). But the inadequacies of the sidestep are regularly apparent, as when he writes that "many of the men who supported Country views were not Puritans in a strictly religious sense" (p. 273). If Puritan equals Congregationalist and Congregationalism is to be taken in a religious sense, these "many" by definition were not Congregationalists, *ergo* they were Baptists or Anglicans or what-have-you with Congregational political ideas. One cannot help but wonder if the word is really worth such contortion. And there is an air of thinness to the volume. On one level it is merely irritating—half-errors of logic and fact that a bit more work would have erased. On another it provokes major disappointment. Shifts in the progression of ideas are too abrupt. The roots of a new twist in the view of the good ruler—roots both in New England and in an ever-changing old England—are unexplored. The tie to events is too often a pronouncement rather than a demonstration, as is Breen's statement in the epilogue that court and country appealed "to different ideas, sources, and traditions" (p. 271); he had already confessed ignorance of the sources of both.

Nevertheless, the volume is a decided contribution. The very consideration of ideas in New England as vital, dynamic, a product rather than determinative of a historical process, is refreshing. The historian of early New England, reading quickly for the mass and order of Breen's quotations, will find much to ponder.

DARRETT B. RUTMAN

University of New Hampshire

ROBERT J. TAYLOR, editor. *The Susquehannah Company Papers*. Volume 10, 1789-1800; Volume 11, 1801-1808. (Sheldon Reynolds Me-

morial Publications.) Ithaca: Cornell University Press for Wyoming Historical and Geological Society. 1971. Pp. xlii, 606; xxxvii, 565. \$20.00 each.

These two volumes mark the completion of a labor begun over forty years ago, and, in the words of the chairman of the present Susquehannah Company Papers Publications Committee, "a dream has come true." The story of eighteenth-century land claims that resulted from the loosely worded colonial charters is an important chapter in American history. It reveals aspects of politics of the time and the growth of land speculation not obtained easily from the readily available history of the early colonies and states. Following the Revolution it became a vital part of the early westward expansion of the new nation. The eleven volumes of *The Susquehannah Company Papers* published for the Wyoming Historical and Geological Society document this segment of American history more fully by far than any other single collected source. Historians are in the debt of this ambitious and persevering local historical society. They are further in debt to Julian P. Boyd for editing the first four volumes and to Robert J. Taylor for his very capable work on volumes 5 through 11. Both the record of this land company and land speculation are covered from 1750 to 1808. The National Publications Commission and the Pennsylvania Historical and Museum Commission deserve credit for financial support of this venture through the final volumes.

The tenth volume highlights the famous law suit *Van Horne's Lessee v. Dorrance*, a key court case in the continued dispute over private land ownership in this northeastern Pennsylvania area, even though the usual historical treatment of the period implies that the issue was settled by the famous Trenton Decree in 1782. This ruling validated the Pennsylvania claim to the territory in dispute with Connecticut but did not settle private ownership problems. The Susquehannah Company continued to sell land well into the 1790s and to authorize townships within Pennsylvania. Pennsylvania also became excited when the issue of Connecticut's cession of its Western Reserve land claims to the United States came to the front in 1799. Some Pennsylvanians thought acceptance of Connecticut's cession implied a va-

lidity to Connecticut's total original boundary claims with a consequent threat to the Trenton decision's validity.

The last volume represents a wise editorial decision to drop the initial plan to publish twelve volumes by eliminating repetitious material such as newspaper extracts and some letters and documents reviewing the previous history of the controversy. Volume 11 is concerned mainly with the roles of Thomas Cooper and Tench Coxe in implementing the Compromise Act that enabled Connecticut claimants to establish title to their lands within the fifteen Connecticut towns along the Susquehanna. It records also Pennsylvania's efforts to force other Connecticut settlers outside these towns to buy their lands. Each of the volumes includes an excellent introductory essay summarizing the significant developments of the period each covers. These are then illustrated by the documentary selections.

S. K. STEVENS

*Pennsylvania Historical and  
Museum Commission*

FREDERICK MERK, with the collaboration of LOIS BANNISTER MERK. *Fruits of Propaganda in the Tyler Administration*. Cambridge, Mass.: Harvard University Press. 1971. Pp. x, 259. \$9.00.

Students of Professor Frederick Merk's brilliant monographs on manifest destiny, the Monroe Doctrine, and the Oregon question will be disappointed in this loosely argued collection of essays. In it Merk attempts to demonstrate that John Tyler used bribery and propaganda to effect the peaceful settlement of the Maine boundary dispute in 1841-42, and that to advance the cause of Texas annexation in 1844-45 he employed Senator Robert J. Walker's ingenious "safety-valve" thesis as administration propaganda.

It is not news that Tyler and Webster engaged at least five paid undercover agents to swing Maine opinion toward a boundary settlement. Tyler candidly admitted his own complicity in the scheme in May 1846. What is new is Professor Merk's imaginative detective work in reconstructing just how the settlement was managed. It is a fascinating story although the author fails to see that Tyler's devotion to states' rights (which he clearly compromised in Maine) was entirely negotiable when the Presi-

dent's political ambitions and his egocentric concerns with his role in history were involved in his decision making.

More controversial is Merk's attempt to link Tyler to the Walker thesis that Texas annexation would eventually solve the racial and slavery questions in America. Few students of Texas annexation would deny that the "safety-valve" argument played an important propaganda role in the annexation debate. But there is no persuasive evidence that Tyler embraced the thesis in 1844, or that he actively worked to popularize it at the time. Merk's case for a Tyler-Walker propaganda alliance is based almost entirely on his contention that Tyler and Walker were close personal friends, that Tyler endorsed the Walker thesis in 1847, and that two members of Tyler's Cabinet, William Wilkins and David Henshaw, supported it in 1844-45.

The fact of the matter is that Tyler skillfully argued Texas annexation in a British-encirclement context and as a national economic desideratum—not in the sectional or slavery terms he knew would defeat an annexation treaty, as indeed it initially did in June 1844. His passing remark in September 1847 that Walker's thesis had "unveiled the true merits of the question" was little more than a political gesture toward the moderate Southern Democracy whose support he sought for the presidential nomination in 1848. Nor were Tyler and Walker personal friends. Henshaw was no longer in Tyler's cabinet during the annexation debate; Wilkins was related to Senator Walker by marriage. Their support of the Walker thesis says nothing very convincing about Tyler's view or utilization of it. And to suggest, as Professor Merk does, that John Tyler introduced the Monroe Doctrine into the annexation debate because he "was clearly seeking a bond to unite the country in support of his policy of acquiring slave territory" is near nonsense. The allegation demonstrates little grasp of Tyler's complex and often contradictory views of race, slavery, and slavery extension.

In sum, this book fails in its attempt to sustain an interpretive overview of Tyler's diplomatic methodology in propaganda terms. Perhaps this is because Tyler's Anglophilia in Maine cannot easily be harmonized with his

Anglophobia in Texas or his Anglo-ambivalence in Oregon. Unfortunately for Tyler-administration hypothesis makers, the tenth president was not a consistent person in his public life. He was, after all, the states' rights theoretician who eagerly accomplished Texas annexation in March 1845 by joint resolution—hardly a strict constructionist concept.

ROBERT SEAGER II  
University of Baltimore

ZANE ALLEN MASON. *Frontiersmen of the Faith: A History of Baptist Pioneer Work in Texas 1865-1885*. San Antonio: Naylor Company. 1970. Pp. ix, 219. \$7.95.

Writers of religious history are sometimes so secularly oriented as to have scant regard for religious concerns, while others are activist churchmen who seek acceptance of their personal views. Professor Mason is obviously dedicated to religious values but approaches his data objectively. His task is made easier by the terminal date for his study (1885), before the impact of the new geology, evolution, and Biblical criticism caused sharp tensions in conservative church circles.

The author identifies five basic Baptist principles: (1) separation of church and state, (2) individual responsibility to God, (3) conversion as a condition to church membership, (4) congregational church government, and (5) baptism by immersion. The volume parallels somewhat the study of Texas Disciples of Christ (Carter E. Boren, *Religion on the Texas Frontier* [1968]), although the latter carries the story well beyond 1885. Both religious groups held similar viewpoints, the Baptists stressing immersion while the Disciples emphasized a creedless adherence to Christ as the cornerstone of faith. In each group some opposed Sunday schools and missionary societies as not grounded in Scripture, but lack of support for missionary activity among the Baptists seems to have been due largely to financial problems.

The author summarizes Baptist beginnings before 1865 and then discusses the Texas frontier, northwestern, western, and southwestern. He has consulted many sources including secular and religious newspapers, church records, and secondary works. The average reader may find little interest in individuals and small congregations that made contributions to the Baptist enterprise, but the author attempts a thor-

ough presentation. He supports historians such as William W. Sweet, asserting that pioneer churches moderated the frightful loneliness of frontier life and both by example and by disciplinary action aided law, order, and decency.

The difficulties encountered included a lack of financial resources and of a trained ministry, Indian depredations, droughts, grasshopper invasions, the prominence of the Catholic tradition among Mexican settlers and of the Lutheran faith among Germans, and splintering divisions among the Baptists. At times relations between white and black Baptists and widespread intemperance also presented real problems. Restricted basically to a twenty-year period and to one denomination the book makes a limited but significant contribution to American religious history.

FRANCIS PHELPS WEISENBURGER  
Ohio State University

BEVERLEY BOWEN MOELLER. *Phil Swing and Boulder Dam*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xii, 199. \$8.50.

Although the history of the American West is a much tilled field the record of Western growth in the twentieth century is still sketchy and obscure. And yet it is a vital part of the national experience. Hence this well-written monograph is especially welcome, for it illuminates not only an important aspect of Western development but presidential politics and public power policies during the twenties as well.

Mrs. Moeller's compact volume is mainly concerned with the efforts of California's Congressman Phil Swing to secure congressional approval for the building of Boulder Dam. Beginning in 1921 Swing fought singlemindedly for establishment of this project. Arrayed against him were private utility interests, large-scale real-estate developers like the Hearsts, and uninterested Easterners. The author carefully traces the tortuous path of Boulder Dam legislation during more than seven years of bitter struggle. Only in 1928 did congress finally authorize construction of what was then the biggest dam on earth. Mrs. Moeller concludes that the redesignated Hoover Dam was grossly misnamed since throughout the twenties Herbert Hoover had shown himself to be an inveterate opponent of public power and consistently opposed the project. Nevertheless, many historians have as-

sociated Hoover Dam with the Hoover administration. To correct such distortions, Mrs. Moeller feels quite rightly, Congressman Phil Swing should be accorded his due right to be considered as the father of Hoover Dam. This carefully researched and objective monograph should provide an effective antidote for recent efforts to revitalize the fictitious myth of Herbert Hoover as the true progenitor of the New Deal.

Assiduously grounded in exhaustive research, mostly in primary materials, this erstwhile dissertation is a model of careful historical scholarship. It should be of interest not only to those concerned with the West but to all students of twentieth-century America.

GERALD D. NASH

*University of New Mexico*

NATHAN IRVIN HUGGINS. *Harlem Renaissance*. New York: Oxford University Press. 1971. Pp. xi, 343. \$8.95.

Rather than being a formal history of the Negro literary and artistic flowering in the 1920s, Huggins's *Harlem Renaissance* is a series of explorations of black identity and the problems of being a black artist in a white, racist society.

The reader of this long-awaited volume is going to be disappointed if he expects a study taking advantage of the wealth of hitherto unused manuscript sources available. Instead Huggins bases his study on a few interviews and the existing published literature, chiefly the writings of the Renaissance authors. This fact necessarily limits Huggins's analysis of the dynamics and personalities, white and black, involved in the development of the Renaissance and even makes his discussion of certain key topics, central to his book, less rich than they should have been. Thus, to cite just one example of materials Huggins should have consulted, James Weldon Johnson's notebooks contain materials that shed considerable illumination on the whole question of cultural nationalism and ethnic identity as viewed by a leading figure in the movement.

On the other hand when Huggins, in his opening chapter, seriously attempts a historical treatment, he commits a number of obvious factual errors and digresses into irrelevant topics such as Du Bois's support of Wilson in 1912 and the World War I campaign, led by NAACP board chairman Joel Spingarn, for a

black officers' training camp. (Curiously, however, Huggins fails to discuss Spingarn's role in encouraging the Harlem Renaissance, both through the literary prizes given in his wife's name and through his activities as a founder of Harcourt, Brace.) Unfortunately while irrelevancies are discussed, Huggins fails to provide an adequate treatment of the intellectual origins of the Renaissance, nor does he make some of the relevant background data he does present functional to his later analysis.

The heart of the book—dealing with the cultural and ethnic ambivalences of the Renaissance artists and writers and the dilemmas they found in attempting to achieve artistic integrity in a milieu where, with their interests in the "primitive" and exotic, white patrons and readers determined "success"—is handled with great sensitivity and insight. The problem is that most of the ground has been pretty well covered before. Thoughtful as his somewhat discursive essays are, the author's failure to use manuscript materials or to attempt a more comprehensive treatment of the movement means that he has not brought us very much further than what we can learn from earlier works like James Weldon Johnson's *Black Manhattan* (1930), Alain Locke's *The New Negro* (1925), Langston Hughes's *The Big Sea* (1946), Gilbert Osofsky's *Harlem* (1966), and Robert Bone's *The Negro Novel in America* (1965).

An adequate history of the Harlem Renaissance is still to be written.

AUGUST MEIER

*Kent State University*

ROSS RUSSELL. *Jazz Style in Kansas City and the Southwest*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xviii, 292. \$12.50.

Popular myth would have it that jazz was "born" in New Orleans and traveled "up de ribber" to Chicago (which required a bit of doing, inasmuch as de ribber doesn't go to Chicago). A revisionist interpretation emphasizes the independent evolution of jazz in various geographic areas.

Ross Russell rather inclines toward the latter view. He makes much of the cultural isolation of Kansas City and of the "Pendergast prosperity" that lured musicians to Kansas City during the depression—two factors that permitted the

development of a distinctive jazz style. Unfortunately the average reader will be hard pressed to know just what this jazz style was—other than the suggestions that it was peculiarly influenced by folk song and that it emphasized the “riff” as a musical device. Jazz cognoscenti will understand what is meant by Kansas City style, but will object that Russell goes beyond mere style to make Kansas City jazz virtually *sui generis*!

Most valuable are the chapters describing the early jazz bands of the region. The author displays an unparalleled knowledge of Coy’s Happy Black Aces, Alphonso Trent, Troy Floyd, Peck’s Bad Boys, Art Bronson’s Bostonians (out of Salina, Kansas!), The Blues Syncopaters, Lloyd Hunter’s Serenaders, Buster Smith’s Blue Devils, and many other little-known musicians and organizations. There are excellent chapters on others that became nationally prominent: Bennie Moten, Count Basie, Andy Kirk’s Clouds of Joy, Harlan Leonard’s Rockets, and Jay McShann.

Saxophonists Lester Young and Charlie Parker are singled out for extended analysis. The latter was the central figure in the Kansas City inspired Bebop revolution and subsequent developments in modern jazz. Ironically Lester Young could be cited as a refutation of Russell’s thesis that Kansas City jazz was essentially uninfluenced by other styles. Young was raised and began playing music in New Orleans and was most influenced by the recordings of Frankie Trumbauer and Bix Beiderbecke. Indeed the existence of the phonograph record makes nonsense of Russell’s “cultural isolation” ideas.

Despite the vulnerability of some of his enthusiasms, Ross Russell’s book is the best that we have on the subject of Southwestern jazz and probably will remain so. It is well documented, with a modicum of errors, and includes a discography, a bibliography, and excellent photographs.

GEORGE A. BOECK

*University of Northern Colorado*

R. SERGE DENISOFF. *Great Day Coming: Folk Music and the American Left*. (Music in American Life.) Urbana: University of Illinois Press. 1971. Pp. 219. \$7.50.

This book tells the story of the cultivation of folk music by the Old Left of the 1930s and the

New Left of the 1960s. In the 1930s radical interest in folk music grew at first from unforced encounter with native tradition. During the Gastonia strike of 1929 and again in bloody Harlan County a few years later Ella May Wiggins and Aunt Molly Jackson made up songs for the struggle. Prompted by such experience, at Brookwood Labor College near New York City, Commonwealth College in Arkansas, and the Highlander Folk School in Tennessee new political words were set to traditional hymns and spirituals such as “We Shall Overcome.”

In the late 1930s this cultural style became party doctrine. Denisoff suggests several reasons. The Communist party had a high percentage of recent immigrants and wished to overcome its cultural separation from mainstream America. A prestigious model in the international Communist movement was the peasant art that did service as proletarian culture in the Soviet Union. And after the adoption of the “popular front” perspective in 1935 Communist parties sought alliances with groups less radical than themselves. Folk music helped to answer each of these political needs.

The results are familiar to those who grew up in New York City at this time and frequented Left gatherings. A Leadbelly or Woody Guthrie was brought to the big city, lionized in the party subculture, and imitated by countless college dropouts with guitars as well as groups like the Almanac Singers. The political folk song and the hootenany became the cultural side of the Communist party’s effort to represent “twentieth-century Americanism.”

The twists and turns of the party line after 1939, together with the blacklisting of individuals like Pete Seeger and the repression of radicalism generally, prevented these songs and their singers from winning wide acceptance, even in the trade-union movement. Yet in 1949–52, when repression was most intense, the commercial success of the Weavers with songs like Leadbelly’s “Good Night, Irene” and Guthrie’s “So Long, It’s Been Good To Know You” foreshadowed what was to come. Along with the revival of the political Left in the 1960s the folk song finally conquered Tin Pan Alley.

Victory came at a price. The Almanac Singers had understood their work as a collective

process: the song was ideally derived from anonymous folk tradition, the singer was often an unidentified member of a group that would draw its audience into the performance. In contrast Dylan, Baez, or Ochs composed songs that expressed their personal feelings, sold them on records, and did not shrink from the focus on themselves that followed. If the folk music of the 1930s at its worst produced a labored straining toward imagined sentiments of "the people," the folk music of the 1960s at its worst produced an equally artificial commercialized star system.

In his concern to call attention to these features of the truth Denisoff draws a caricature rather than a portrait. At its best the tradition of political folk song has been very good indeed. In their different ways Pete Seeger and Joan Baez have shown that musicians can enrich rather than cheapen their craft by plunging into politics; that the professional and the amateur have complementary contributions to make to a total art; that a performer can relate to an audience as equals without false deference or arrogance. They have created a model from which all professionals and craftsmen, not least historians, can learn.

STAUGHTON LYND  
Chicago, Illinois

WILLIAM C. BERMAN. *The Politics of Civil Rights in the Truman Administration*. Columbus: Ohio State University Press. 1970. Pp. xi, 261. \$8.00.

Professor Berman observes that Harry Truman was in deep trouble in his 1940 senatorial campaign. The Pendergast machine was out of power and his opponent in the primary was preferred by FDR. However, the "organized" black vote pulled Truman through. Ironically, four years later, he had the enthusiastic support of the South in its successful effort to dump "nigger-loving" Henry Wallace as Roosevelt's running mate. As Berman points out, Truman's appeal lay in his "centrist" links to both the liberal and conservative wings of the Democratic party. He believed in "fair play" and "equal justice" for Negroes, but not if that meant incurring serious opposition from Southern congressmen. As president he sought to avoid a major rupture between the party's two factions and to call upon the Southern

congressional bloc for support of his foreign policy and welfare programs. But the Republican landslide in the 1946 congressional elections convinced Truman that for 1948 he needed a civil rights record to appeal to the Democrats' Northern liberal wing—especially to black voters in key industrial states. Accordingly, while he still could not bring himself to fight for civil rights legislation, he willingly delivered important presidential statements and used his executive power on behalf of Negro aspirations. Truman issued an executive order creating a commission that studied race relations in America and called for the full integration of blacks. Another Truman order provided for the desegregation of the armed forces. But a third one, issued later, in 1951, which required corporations holding federal contracts to pursue a nondiscriminatory employment policy, was ineffectively enforced. Indeed, having won the 1948 election with overwhelming black support despite a torn-up Democratic party, Truman, as a true "centrist," thereupon gave top priority to healing the North-South breach in the party and in Congress. That task, of course, ended hope for civil rights legislation. Yet as Berman shows, Truman had done more for civil rights than any previous president and had used his office to help create a new climate of opinion. But frightened of the political dangers Truman refused to go further.

Berman has produced a highly competent study, carefully researched in the archival materials relevant to Truman and to Negroes. Berman correctly emphasizes the black vote in affecting Truman's actions. However, while the author also mentions the role of the black man's "white allies," the book could have been improved by an analysis of just how these whites actually functioned in the "politics of civil rights."

This volume is a welcome addition to the literature.

ELLIOTT RUDWICK  
Kent State University

JOSEPH BRUCE GORMAN. *Kefauver: A Political Biography*. New York: Oxford University Press. 1971. Pp. viii, 434. \$10.00.

It was highly unlikely that a man whose campaign emblem was a coonskin cap should be

elected president in the industrial, urban society that the United States had become by the 1950s. The fate of Bryan's aspirations a half century earlier should have taught that lesson. Moreover, most Americans enjoyed a complacent mood during the period of the Eisenhower equilibrium, and in many circles there existed little sympathy with a boat-rocking populist of the sort that Senator Estes Kefauver seemed to be. In addition, Kefauver of Tennessee labored under the misfortune of sowing his presidential oats at the time that Lyndon B. Johnson of Texas began working the same field, thus presenting a rare occasion in modern history when not just one, but two Southerners offered themselves as contenders for the White House. In the end Johnson proved to be the "slicker" of the two men.

Nevertheless it appears that the quest for the nation's highest office governed, in large part, Estes Kefauver's career; he had contracted, as Professor Frederick Merk used to put it, "the presidential fever." Almost from the beginning of his service as a congressman in 1939, and then as a senator after 1949, Kefauver had a knack for latching on to issues that were headline catchers: reform of congressional procedures, concentration of control in big business, juvenile delinquency, and, of course, the famous televised hearings on organized crime in the early 1950s. (One is reminded of Senator Joe McCarthy's thrashing about for a viable issue until he stumbled upon "the Communist menace.") Kefauver's name became well enough known so that twice, in 1952 and 1956, he could score impressive victories along the "primary route" to the executive mansion. Although he carried off his party's vice-presidential nomination in 1956, during the next few years he ran afoul of the complex maneuverings of the Johnson and Kennedy forces to such an extent that by late 1959 the public opinion polls no longer listed him among the Democrats' presidential hopefuls. Thereafter Kefauver settled down to become a Senate workhorse, especially in the field of consumer protection, until death unexpectedly claimed him in 1963.

Kefauver's biographer, Joseph Bruce Gorman, does not push the "presidential fever" theme as relentlessly as I have done in the

above summary. It may well be, as he suggests, that unselfish concern for the public interest motivated Kefauver's behavior more than I have allowed. In any event an "emotional moat" surrounded the man, according to the author, and consequently it is difficult to tell just why Kefauver followed the paths he did. As one who is skeptical about the reliability of biographical "psychoanalysis," I will not quarrel over the point. Gorman has written an interesting and competent account of Kefauver's public life, one that is about as thorough as available evidence allows. If Kefauver was a headline hunter he, unlike McCarthy, at least exploited causes that contributed to the general welfare. And if the *New Republic* in its 1963 obituary could feel that Kefauver's populism was "a legacy from the past" possessing "neither the intellectual style nor the restrained manner valued on the New Frontier or elsewhere in an increasingly technocratic society," there are perhaps today a growing number of Americans who are not quite so sure that "his faith is . . . largely discredited."

J. JOSEPH HUTHMACHER  
University of Delaware

ROBERT L. BRANYAN and LAWRENCE H. LARSEN. *The Eisenhower Administration, 1953-1961: A Documentary History*. In two volumes. New York: Random House. 1971. Pp. xi, 686; vi, 687-1414. \$55.00 the set.

The editors of these two large volumes appear to have defined their objectives as, first, the compilation of an interior documentary history of the Eisenhower administration during its years of power; second, the presentation of administration viewpoints rather than lengthy editorial analysis; and third, the maximum usage of manuscripts from the Dwight D. Eisenhower Library. Their most important contribution is the publication of many significant documents not already in print. Their chronological-topical organization imparts a good sense of the political structure of the 1950s.

This collection nevertheless seems to pass over some important problems. There is no section on the 1952 campaign, although a few 1952 documents are included under other topical headings. The book does not have a single speech or statement by Eisenhower's influential



first secretary of the treasury, William Humphrey; the equally important second holder of the office, Robert Anderson, is never even mentioned. A section on "the McCarthy problem" in 1953-54 fails to illustrate that part of the administration's difficulty in dealing with Senator Joe McCarthy stemmed from its own use of McCarthyism in Attorney General Brownell's revival of the Harry Dexter White controversy and Vice-President Nixon's congressional campaign speeches.

These omissions may indicate an absence of such material at the Eisenhower Library; if so, the editors have relied too heavily upon the papers there. Commendable though it is, their aim of reproducing as many unpublished documents as possible appears at times to have led them into the trivial. The book contains far too many unsubstantive communications of gratitude, personal friendship, or condolence—largely, no doubt, because Eisenhower's private personal correspondence remains closed. The illness and death of John Foster Dulles, for example, is covered by eight pages consisting mostly of Eisenhower's expressions of confidence in and affection for his secretary of state; the topic might have been better served by the selection of some published contemporary analyses of Dulles's accomplishments and the consequences of his passing.

Other subjects, apparently because of the availability of manuscripts, receive treatment disproportionate to their importance, among them the teapot tempest accompanying the trial of the American soldier William Girard by a Japanese court, the petty partisan maneuverings that delayed statehood for Alaska and Hawaii, and the flap over distribution of the Salk polio vaccine. A section on the interstate highway program, although interesting and informative, receives twice as much space as the Dixon-Yates controversy.

In their attempt to present administration perspectives the editors seem to suspend their critical faculties on some issues. Their comments and selections on McCarthyism, the Dixon-Yates fight, and civil rights treat the Eisenhower government much more gently than most accounts. In some cases the documents fail to convey the political dialogue; sections on agricultural problems and the 1958 recession

contain no statements of opposition alternatives or criticisms. Nor is there any skeptical analysis of the fuzzy and muddled concept of "modern Republicanism."

Errors are relatively few and usually obvious, but some readers may be unaware that it was not Walton, but Edwin Walker who commanded the troops sent to Little Rock (p. 1140). Perhaps because of space limitations, the bibliographical essay is disappointingly brief and incomplete.

Most of these criticisms involve matters of editorial judgment, and even the most carping reviewer should beware of making his conception of editorial priorities an absolute standard. Many of the sections of this book are informative and thoroughly satisfactory from any vantage point. The coverage of the organization of the executive branch, for example, is quite illuminating, and most of the diplomatic-military sections are very well done. Branyan and Larsen probably give Eisenhower the benefit of a doubt too often, but their mild revisionism may serve as a corrective to the biases of liberal historians. Far more complete and valuable than most documentary collections, this book will become a standard reference source.

Until now historians interested in the Eisenhower administration have had to make do with memoirs, journalistic accounts, and a few scholarly works by political scientists. This collection, along with Herbert Parmet's forthcoming study of the Eisenhower years, should mark the opening of a new era to serious historical inquiry.

ALONZO L. HAMBY  
Ohio University

WILLIAM L. O'NEILL. *Coming Apart: An Informal History of America in the 1960's*. Chicago: Quadrangle Books. 1971. Pp. ix, 442, xxvi. \$12.50.

*Coming Apart*, which lurches from the eloquent to the colloquial and from the shrewd to the commonplace, is a fiercely opinionated study of the sixties. Subtitled *An Informal History* this volume is an often impressive—and sometimes irritating—combination of personal journalism, cultural criticism, and historical appraisal that eschews interviews and archives and

rests largely on the journalism of the past decade. *Coming Apart* is perceptive and derivative, thoughtful and careless, full of judgments and often without enough analysis. It is a bold, ambitious, personal book that expresses the author's conclusions and prejudices but is weakened by the absence of any extended statement of his political and cultural philosophy.

In this richly informed survey O'Neill, building upon the essays of Murray Kempton and others, contributes to the emerging favorable reassessment of Eisenhower, though noting that the president's achievements were largely negative: Ike ended the Korean War, stayed out of new ones, and kept military spending down. John Foster Dulles, however, was "laughable when not sinister." O'Neill also joins the fun in puncturing some of the myths of Camelot and John F. Kennedy. Contending that the Kennedy of 1960 "was mostly blind to what would dominate American life in the sixties," O'Neill scolds the administration for the space race, regrets Kennedy's "obsession" with NATO, criticizes his support of the Bay of Pigs invasion and his handling of the missile crisis, and concludes that his government's commitment to civil rights was "real and misleading." Still Kennedy, "the existential hero," was "midwife" to the "new order struggling to be born." O'Neill stresses Johnson's "fakery" and deceit in foreign policy, contends that Johnson did not want peace without victory in South Vietnam, laments the President's failures in domestic policy, but finds (questionably) that he "never wavered" in his hatred of racism. Even the much-heralded Alliance For Progress, O'Neill charges, was a complete failure; he adds that United States policy in Latin America was "always exploitive." Most of these judgments seem sound (though controversial), are based upon stated assumptions, and undoubtedly express the notions of many who are reinterpreting the recent past.

More troubling, however, are some of the author's other views. For example he finds conservatives "more demented" than radicals in the early sixties, views the ghetto uprisings as "a kind of madness," stresses the achievements of Eugene McCarthy's candidacy, concludes that "radicalism does not work" in America, and believes that if the country is saved it will be by people like Ralph Nader. O'Neill also

dislikes the counterculture of drugs and hippies, castigates those radicals who use violence in America, and defends the universities on the grounds that their "evil" ("military research, officer training") could be done anywhere, but their existence is essential for disinterested inquiry and the search for truth and beauty. Underlying these beliefs, which have their echoes throughout much of the book, is a set of fundamental values or a philosophy that remains vague, dim, often implicit—yet central to understanding and evaluating much of the volume. O'Neill seems to be a liberal who, lamenting much of the self-laceration of other liberals, endorses the politics represented by Eugene McCarthy, opposes the war and racism, regards much black militancy as foolish and self-destructive, and hopes (perhaps uneasily) for change through the electoral process. America's postwar foreign policy has often been misguided, O'Neill emphasizes, and the domino theory is simple-minded and usually wrong. In these views he stays within the new liberal framework. Presumably O'Neill regards radical analyses of American imperialism and of the concentration of power as wrong, for these theories are not at the root of his judgments and do not receive more than a few acknowledgments, certainly not a rebuttal.

Even those who disagree with many of O'Neill's judgments and lament his decision to avoid a sustained dialogue with radicals will have to be impressed by his capacity to survey the sixties and to recapture much of their faith, fervor, and fear. Because *Coming Apart* pulls together so much and offers a critical perspective on the last decade, this lively book will be widely used in courses and is likely to provoke in less extreme forms the passions and antagonisms that have so often divided students from teachers, and colleagues from one another, in the past few years. Perhaps, in the last analysis, this will also be a testimonial to the author's courage, candor, and commitment.

BARTON J. BERNSTEIN  
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MIGUEL JORRÍN and JOHN D. MARTZ. *Latin-American Political Thought and Ideology*. Chapel Hill: University of North Carolina Press. 1970. Pp. xiii, 453. \$12.50.

The welcome deserved by this long-needed work on Latin American political thought and ideologies must be tempered by the realization that what Professor Martz has done is to complete a manuscript that the late Miguel Jorrín left unfinished at his death. This dual authorship also makes it difficult to tell how much of the book is the work of Professor Jorrín and how much comes from the mind and pen of Professor Martz, despite the latter's indication of the general lines of division in the preface (p. xii). The first half of the book, the historical background through the nineteenth century, is basically the work of Jorrín; the second half, dealing with the thought and ideologies of the twentieth century, is largely, if not entirely, the work of Martz. Considered together, their contributions constitute a pioneer work on a too much neglected area of modern political thought. The historian may find it less than satisfactory, however, particularly in respect to the transition from the nineteenth to the twentieth century, while the political scientist may find it too "historical." But both should find it highly useful.

Since previous studies of Latin American thought have come largely from historians, sociologists, and philosophers, it is appropriate, in connection with this work by two political scientists, to ask some general questions as to what a book of this kind, intended as a general guide to students of the subject, should be like. Should it be chiefly addressed to questions of political philosophy and theory, such as the nature and functions of the state, philosophy of law, and the nature of political behavior? Should it, rather, concentrate on political ideologies, and if so how should they be analyzed? How much attention should be given to the theoretical and philosophical search for norms and values and how much should be directed toward more situational, relativist, and "existentialist" questions? To what extent should it be historical? Neither Professor Martz nor Professor Jorrín seems to have addressed such questions directly, preferring an uncomplicated account of what Latin Americans have written and thought. But I am inclined to conclude that, despite the historical approach they have adopted and their ideological conceptualization, their preference is for a relativist-existentialist analysis that looks primarily at

ideologies and at the political events and circumstances that give them meaning or lack of meaning.

Several chapters make outstanding contributions to our understanding of the subject. Among these are a short but enlightening chapter (6) on anarchism in Latin America, a chapter (5) on "Idealism," which will disturb philosophers by its rather loose use of the term, but which brings out one of the outstanding characteristics of Latin American thought, and a very useful chapter (9) on "Marxism in Theory and Practice." One might wish that the section on Cuban revolutionary thought in this last chapter were less taken up with political events and devoted more to an analysis of the thought and ideology, especially that of Ernesto "Che" Guevara. There is a good chapter (13) on Christian Democratic thought.

While noting the many excellent qualities of this book, I must express regret that the authors have given very little attention to the important streams of traditionalism and conservative thought, to legal philosophy, to the considerable literature on the theory of the state, and to economic and sociological theory, in which Latin American writing also abounds. The index, limited to names, would have been more useful if it had included at least a few of the subject headings not covered adequately in the contents.

HAROLD EUGENE DAVIS  
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ERIC WILLIAMS. *From Columbus to Castro: The History of the Caribbean, 1492-1969*. New York: Harper and Row. 1970. Pp. 576. \$10.95.

The author's avowed purpose in writing this book was to cope with the fact that "there is no history of the Caribbean area as a whole." This fact is true despite the inclusion of Caribbean events within the histories of the metropolitan powers who colonized the area and the production of good national histories by a few of the republics. The term "Caribbean" has remained a geographical expression without definite political or cultural content. Even in the geographical sense there are differences of meaning as some scholars include the mainland countries that border the Caribbean Sea as well as the islands within it. The author includes only the islands but adds to them the Guianas

as areas with a political and institutional background linking them to the Caribbean.

The question of focus is important because if the large mainland republics of Spanish origin are included the small states of non-Spanish background become only quaint aberrants in the general picture of the region. A true Caribbean history requires also a prior analysis of the common factors in the various states if a comprehensive and meaningful history is to be constructed.

The author's contributions in this respect are impressive. His consistent study may be traced back to his doctoral dissertation in history at Oxford and the syntheses he presented in his book *Capitalism and Slavery* (1944). As prime minister of Trinidad and Tobago since 1956, Eric Williams's statements acquire value not only for what they are but for who he is. The factors he finds that give unity to the Caribbean include slavery; the black and his culture; sugar and the plantation system; and colonialism, mercantilism, and capitalism as applied by colonial powers in the area. The present book presents a notable advance from that of 1944. A principal part of this advance is the real effort Williams has expended to bring the non-Spanish islands more understandingly into his study.

His work on Cuba is especially good, and he generously makes the statement that "Nineteenth century Cuba produced more great men than the rest of the Caribbean territories combined throughout their entire history." There are some errors that appear to be more than typographical, but on the whole this is a provocative, useful book that should stimulate further development in the direction the author has taken. There are numerous controversial statements, as is only natural in such a trail breaker. The most serious objection the reviewer must make is the absence of footnotes to make available the many sources quoted in the texts. Since the book must be considered a scholarly book and not a popular one the omission of footnotes and marginal comments is unfortunate. It is true that there is a large bibliography with comments, but immediate page reference to quoted material would have been most useful.

In summary, a work indispensable to the stu-

dent of the Caribbean—in any definition of the term.

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ROBERT I. ROTBERG, with CHRISTOPHER K. CLAGUE. *Haiti: The Politics of Squalor*. (Twentieth Century Fund Study.) Boston: Houghton Mifflin Company. 1971. Pp. xvi, 456. \$10.00.

This work can be conveniently divided into three parts. In the first the author reviews Haitian history. Emphasizing the nineteenth century and United States occupation Rotberg pays close attention to factors in Haiti's troubled past that may seem causal when one is forced to explain the reign of Dr. François Duvalier. Rotberg emphatically finds such factors, and a central thesis of the book is that the Duvalier regime was a result, almost an inevitable result, of Haiti's history and "social and political matrix." The big question likely to be raised is whether one can be this much of a historical determinist, even in backward, relatively "uncomplicated" societies such as Haiti.

The author then turns to the Duvalier years, and although this section offers more narrative than analysis, it remains by far the best and most sober account to date, *pace* Diederich and Burt, Graham Greene, Gingras, and others.

Finally the author turns to a most detailed analysis of present-day Haiti, its health conditions, educational system, agriculture, and economy. Judiciously but scathingly he examines and demolishes many of the plans and campaigns that have been suggested or mounted to solve one or all of the country's multitude of problems. To replace such chimera Rotberg admits that he has little to offer. Small, pragmatic, local schemes may be of some help. Massive outside interventions or invasions have proved that they are not. Rotberg concludes, realistically, that if there is indeed a set of solutions to Haiti's appalling situation—and that in itself is doubtful—then it must be a slow, painful, and mostly Haitian one.

Much of the literature on Haiti is partisan, strident, and inferior in quality. It is difficult, therefore, to place this book—a serious, well-researched account—in its proper context. At-

tempts to claim that Haitians suffer from a national paranoia related to child-rearing practices leave me skeptical, as do the fortunately brief attempts to introduce magno-political concepts such as "the predatory state." At times the style, perhaps even the vocabulary, is needlessly journalistic. These reservations apart, there is little doubt that, *faute de mieux*, this is the best book on present-day Haiti to appear in recent years.

The appendixes on exports in the eighteenth and nineteenth centuries should be of use to historians, and the long bibliography of printed works is comprehensive.

MURDO J. MACLEOD  
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HARRY LEONARD SAWATZKY. *They Sought a Country: Mennonite Colonization in Mexico*. With an appendix on Mennonite colonization in British Honduras. Berkeley and Los Angeles: University of California Press. 1971. Pp. xi, 387. \$11.50.

In four centuries of preserving their Anabaptist beliefs Mennonites have migrated many times while searching for a country and rural environment willing to accept them. Progressive members have made adjustments to urban life; the more conservative had formed closed colonies successively in Prussia, south Russia, and Canada by 1870. The Privilegium under which they maintained their private schools and exemption from military service was weakened in Canada during World War I. Migration to Mexico began in the 1920s, under new Privilegia from President Obregon in 1921. Seeking the wastelands of the interior, some 7,800 Mennonites were settled in Durango and Chihuahua colonies by 1950. All but a few hundred were from the Canadian prairie provinces.

Most of Professor Sawatzky's book details the manner in which these migrants adjusted to the barren highlands and the evolving life of post-Revolutionary Mexico. By 1950 their hardships seemed to have assured them of permanence. However, Mexican nationalists, especially in Chihuahua, attacked their aloofness from Mexican society, failure to upgrade educational ideas, and resistance to changes arising from the national revolution. Pressures to abro-

gate parts of the Privilegia have increased yearly. Attempts to force Mennonites into the national social security system alarmed them. In the mid-1950s a migration of 3,500 to British Honduras—with a new Privilegium—gave them another country and new challenges. It seems likely to Sawatzky, a Mennonite geographer at the University of Manitoba, that the bulk of the conservatives in Mexico will soon be moving out, probably to Paraguay and eastern Bolivia. Though their half-century struggle in Mexico seems to have left them with the old alternatives of adapting or moving, they have made a durable impact on the interior of northern Mexico. Pioneering agriculture in former grazing lands, they led Mexicans to adopt their methods. While modern Mexican practices have passed the Mennonites in many ways in recent years, it is unlikely that the Mennonite beginnings will be forgotten wherever they might go.

The account is difficult to follow, largely because it has no map to show the colony sites, which are described textually with precision but not clarity. The map for British Honduras does what one or two could do for Chihuahua and Durango: pinpoint locations so the goings and comings of the fragmented Mennonites might be more readily followed.

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ANDRÉ SAINT-LU. *Condition coloniale et conscience créole au Guatemala (1524-1821)*. (Publications de la Faculté des Lettres et Sciences humaines de Poitiers, Number 8.) Paris: Presses Universitaires de France. 1970. Pp. 219. 25 fr.

*Criollismo*, the theme of Professor Saint-Lu's work, is the expression by the so-called *criollos* (Americans of Spanish descent born in the New World) of a growing regional self-consciousness. In his case study of Guatemala, the author situates this feeling initially in the pride of conquest by the conquistadors, in their zeal in defending Guatemala from Spanish rivals or foreign buccaneers, and in their insistence on the right for themselves and their descendants to hold local political office, to receive income from Indian labor, and to enjoy various other privileges associated with the nobiliary class of Spain. Zealous in defending their privileges

against immigrant Spaniards, the *criollos* nurtured and expanded *criollismo* during the seventeenth century to include an esthetic appreciation of their native land and an admiration for the ancient Maya, both subjects expressed by the Creole historian, Francisco Fuentes y Guzman (1642?-1690?) in his history, *Recordacion Florida*.

The theme is less well sustained during the eighteenth century and the independence period, owing to the complexity of cultural influences and political ideas coming from Europe, but the author shows that the Creole class, better educated and wealthier, retained sufficient unity of feeling by a matured *criollismo* to accomplish a bloodless independence in 1821. Although the work clearly establishes the continuity of *criollismo* as a feeling shared by the Guatemalan *criollo* class during the colonial period, the theme could have been further illuminated by attention to Mayan passivism and limited miscegenation in Guatemala, for the class feeling on which *criollismo* rested was less solid in such racially mixed regions as El Salvador and south-central Mexico.

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RICHARD NEWBOLD ADAMS. *Crucifixion by Power: Essays on Guatemalan National Social Structure, 1944-1966*. With chapters by BRIAN MURPHY and BRYAN ROBERTS. Austin: University of Texas Press. 1970. Pp. xiv, 553. \$10.00.

This study, a combined effort by some of Richard Adams's graduate students and colleagues during 1963-65, is really an attempt (1) to explain how limited access to centers of power prevents many Guatemalans from improving their social and economic position and (2) to explore whether or not Guatemalans have the capacity to improve their situation. The work provisionally demonstrates that Guatemalan society is so restricted by its internal structure as well as by the strong influence of the United States that there is little likelihood that broad changes will occur.

Historians may find the theoretical disputation in the first chapter somewhat tedious. Adams has his own views and challenges the approach of several of his fellow anthropologists, but the chapter contains important sec-

tions on the structure, organization, and concentration of power. The bulk of the book consists of case studies by graduate students. Adams has analyzed their efforts and presented their finds, some of which challenge more traditional interpretations. Historians of Latin America or United States diplomacy might do well to take note of them. Diplomatic historians should consider that, the United Fruit Company notwithstanding, the United States really became deeply involved in Guatemala only in the period after 1941.

The overthrow of the controversial reform government of President Arbenz in 1954 led to a significant increase in U.S. industrial activity. Scores of American representatives of military, technical, and voluntary organizations also entered the country. This broadening of American activity placed increased strictures on the Guatemalans' ability to control many aspects of their own destiny.

Adams's study rightly shows that interests controlled by a minority of Guatemalans also inhibited diversification of power. One such interest group is the military. The officers liked the perquisites introduced by the United States, but they recognized that they did not want the United States to control them. The officers reacted by resolving internal differences, and, as the 1960s demonstrated, they achieved more say in the governance of their country.

There are several other chapters interesting to historians. These include insights into the Church's attempts to reassert itself, the role of interest groups, and the ecological and economic impact of cotton.

It is necessary to stress again that this study is only a preliminary one. No matter how small or willing the nation, a handful of investigators can only accomplish so much. But one has to begin somewhere, and for that reason Adams's look at the uses and abuses of power merits serious consideration.

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G. A. MELLANDER. *The United States in Panamanian Politics: The Intriguing Formative Years*. Danville, Ill.: Interstate Printers and Publishers. 1971. Pp. 215. \$7.95.

The subtitle of the volume, *The Intriguing Formative Years*, supplies the theme for this engaging study of the intimate United States involvement in Panamanian politics from the months just prior to her independence through the administration of her first president (1904-08). The author's many years of residence in Panama and his opportunity for thorough investigation of Panamanian source materials provide another perspective for subject matter that has heretofore been examined mainly in terms of United States foreign policy. His interpretation presents the United States as reacting to events rather than calculating a strategy. United States involvement in Panama's independence originated not in Washington but in Panama itself: Panamanian revolutionists, aided and abetted by non-isthmians, sought United States assistance. Although the United States was scarcely a reluctant partner it did not initiate the separatists' plans. Again, John Hay was not the architect of the 1903 treaty that transformed Panama into a protectorate of the United States, but Philippe Bunau-Varilla—although Hay did not hesitate to take full advantage of the opportunity presented. Dr. Mellander reminds us of a "soulfully painful" truth: not a single Panamanian was present during any of the treaty discussions. Bunau-Varilla virtually forced the treaty upon the hapless Panamanian Junta, which promised to ratify it five days before receiving it.

The author discerns two continuing threads during the period discussed. The Roosevelt administration became increasingly concerned that Panamanian internal politics might degenerate into an anarchy that could endanger the building of the canal and gravitated toward secret but active involvement in these politics. Notwithstanding, Washington became intimately involved in the internal affairs of Panama only after insistent and numerous pleas from the Panamanian Liberal party. The inordinate desire of the party for power led it into actions that seriously compromised the nation's sovereignty. "It may not be stretching a point too far to state that the United States, certainly not interested in becoming embroiled in local affairs, was the naive handmaiden of the Liberals. For they, preying on Washington's myopic

phobia of danger to the canal-building project, induced the United States to intervene in Panama's political arena" (p. 194).

MAURY BAKER

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ROBERT W. SHIRLEY. *The End of a Tradition: Culture Change and Development in the Município of Cunha, São Paulo, Brazil*. (Institute of Latin American Studies, Columbia University.) New York: Columbia University Press. 1971. Pp. xi, 304. \$10.00.

Cunha is perhaps the most thoroughly researched community of small-town Brazil. Shirley, an anthropologist, is the seventh scholar to work there, and his study uses Emílio Willems's *Cunha* (1947) as a point of departure. The new book, however, is not a restudy *stricto sensu*, but an examination of "disorganization of an old agrarian society . . . and the multifold effect which an industrial center has on its dependent zones" (p. ix). Thus the analytical framework is not the community as such, but the transformation of a semirural *município* by an expanding metropolis—São Paulo city—for which Cunha's main economic function in recent decades has been to supply foodstuffs and unskilled workers.

A major change since 1945 has been the tremendous increase in land values owing to Cunha's new satellite status. Consequently many peasants have lost their land (for want of legal title) but so have traditional *latifundiários*, and a rural bourgeoisie is rising. As the author demonstrates, social status in this urbanizing community is increasingly ambiguous, as the power of state institutions (more than federal ones) undercuts traditional authority.

This study contributes signally to a refutation of sociological dualism, a thesis explicit, for example, in Lambert's *Os dois Brasis*. Yet Shirley rejects an internal colonialism model. To the contrary he states, "The future of . . . rural Brazil lies in the continued development of its industrial cities" (p. 255). To me, however, it is not obvious that the beneficial aspects of Cunha's transformation would necessarily accrue to communities beyond the staging area of the metropolis. A holistic approach might lead to different conclusions and even raise doubts whether "other São Paulos" can arise.

The author's demonstration of the federal government's weakness relative to the state in Cunha—both fiscally and politically—is an intriguing finding, in view of the centralization of power after the 1964 “revolution.” São Paulo state is doubtless *sui generis*, but the historian is led to ask how important the central government could have been at the local level during the *Estado Novo* (1937–45), when São Paulo and Brazil were far less industrialized. Furthermore, if in Cunha allegiance to the state is still stronger than that to the nation (p. 108), we may ask whether economic development even now is strengthening state loyalties that will only be superseded by national ones later.

Students of modern Brazil should read this book for the issues it raises, directly and implicitly.

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FREDERICK M. NUNN. *Chilean Politics, 1920–1931: The Honorable Mission of the Armed Forces*. Albuquerque: University of New Mexico Press. 1970. Pp. x, 219. \$10.00.

This work escapes being a simple coup by coup account of military intervention yet is without pretentious conceptual capers. Chile may not have had “participatory democracy before the military came to power,” but, for Latin America, it has an enviable record of military subordination to civilian authority. However, the Honorable Mission's intrusion into politics was not unwarranted. The Honorable Mission's personnel, including Carlos Ibáñez del Campo, were sincerely reluctant to tamper with Chile's political apparatus. Economic debility, politi-

cal irresponsibility, and corruption created an irresistible urge among young officers to act. The consequences were beyond their capacity to control.

Ibáñez was initially supported by a broad spectrum of the nation. Communists and conservative elements were exceptions. The military men accepted socialism but rejected Marxism. If this seemed paradoxical, it did not bother them, and since the Chilean Socialist party was then nascent it was no problem. When the time came in 1931 for Ibáñez and his cohorts to stack their political arms, the same consensus that had approved their thrust to power opposed its continuance. The middle sectors had become disillusioned with the military regime. It should also have been noted that the young officers of 1924–31 were themselves of middle-sector antecedents. Their intra-service opponents were the senior officers of more aristocratic antecedents. This was indicated by the struggle within the army and the navy.

This book is no crystal ball in which the fate of other “problem oriented military regimes” is projected. Still, Chile's Honorable Mission was something more than a mere *cuartelazo*. True, it failed to achieve its ends, and that may be the lesson to be learned. The Honorable Mission and subsequent governments certainly prepared the way for the incumbent Chilean government. It would have been relevant if this had been dwelled on a bit by the author, but this work was well worth reading even without any prognostications.

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

Peter Loewenberg's "The Psychohistorical Origins of the Nazi Youth Cohort" (*AHR*, 76 [1971]: 1457-1502) fails in its attempt to profile an entire generation. Although the judgments expressed with regard to the emotional impact of a shared national experience are valid to a considerable extent, the many variables relating to home conditions, sibling rivalry, and the personality structure and genetic quality of individuals would have precluded anything like a scientifically confirmable result, even if scientific methodology rather than psychoanalytic and sociological theory had guided the author's work. But apart from such a self-evident and general critique, I deplore, as a historian, Loewenberg's failure to take into account cer-

tain basic political, psychological, and social considerations central to or touching upon his thesis.

Having described at some length conditions in wartime and early postwar Germany, Loewenberg suddenly brings in Austria. It seems he belatedly became aware that his analytical material and technique applied also to Germany's partner in defeat. Loewenberg concedes that the youth of all the belligerent nations suffered from absent parents, but he stresses that the absence of "German and Austrian parents" was coupled with extreme hunger and that when the German or Austrian father returned "he came in defeat and was unable to protect his family in the postwar period of unemployment and inflation. Not only was the nation defeated, but the whole political-social world was overturned. The Kaiser of Germany had fled, and the Kaiser of Austria had been deposed. . . . These national factors unique to Central Europe exacerbated the familial crisis of the absence of the parents and made of this wartime experience a generational crisis" (p. 1480). Eliciting the views of Paul Federn, Loewenberg tells us that "the Kaisers of Germany and Austria were deprived of land, throne, power, and the ability to offer a feeling of security. Thus a fatherless society was created that no longer stood in awe of the state" (p. 1489). Loewenberg, in general, lumps Austria together with Germany as sharers of a common Central European destiny, although he omits all mention of Hungary, which also lost a Kaiser (or king) and the war, as well as a far greater percentage of her ethnic territory than did either Germany or Austria.

But Hungary aside, Loewenberg made no attempt to analyze postwar events in Austria.

Had he done so, he would have discovered that, despite some obvious parallels, Austria experienced a substantially different pattern of development as compared with Germany and, most especially, with regard to the roles played by the Nazi movement in each country. Very instructive as to the differences between political developments in Germany and Austria are the following comments of Ernst von Salomon made in reference to his stay in Vienna in 1932: "The situation in Austria was indeed singular. . . . It has been said that the Weimar Republic collapsed in 1932 because of a multiplicity of political parties scrambling for shreds of power. . . . In Austria . . . since 1918, the division had been clear-cut, black and red [clerical and socialist]. There was no doubt that Vienna was on the brink of civil war." Having spoken to an Austrian Catholic about the impending *Anschluss*, Salomon "was in despair. He [the Austrian] would not see what was coming and I could not explain it to him. He didn't know National-Socialism, nobody in Austria did; the little [Nazi] groups which had up till then formed were scarcely noticeable and had certainly no effect on Austria's internal struggle for power. How could I, a Prussian, make it clear to Spann, an Austrian, that apart from the few Austrian National-Socialists no group in Austria, no matter how much it might welcome the *Anschluss*, could possibly welcome it in this form?" (Ernst von Salomon, *The Answers* [London, 1954], 93, 101).

Loewenberg presents through the words of Harold D. Lasswell the idea that Hitler's "imagery of cleanliness and pollution of the anal phase" became a source of ersatz parental security for the youth of Germany (pp. 1484-85). This is a highly eclectic way of bolstering his thesis because it underrates the traditionally anal orientation in German psychohistory and hence its function in the psyche of persons of all ages before, during, and after the period under discussion. Hitler's "anal" rages appear to have had a great deal in common with the emotional tone of Germany's other major prophets, as indicated by Luther's constipation and Marx's hemorrhoids. One is tempted even to suggest that but for Freud's auto-erotic fantasies, Loewenberg's own article could never have been penned.

What effect did alcoholism have upon the

Nazi movement? Konrad Heiden's eyewitness account of Hitler's abortive 1924 coup in Munich's Bürgerbräu Keller is instructive, especially with regard to olfactory detail. "With his storm troops at their posts, Hitler . . . stood amid the beer fumes in the crowded vestibule. . . . The crowd in the hall was not so docile. Hitler left the hall with his prisoners [Kahr, Lossow, and Seisser]. The mood grew menacing. 'Don't worry,' Göring shouted . . . 'we have the friendliest intentions, and anyway, you can be happy, you have your beer.' Meanwhile, in the adjoining room—a cold dismal place, full of beer fumes—Hitler spoke to his three prisoners in confused, jumbled snatches. [Hitler threatened them with his pistol.] They had but one choice: to fight by his side and conquer, or to die. Lossow and Seisser later testified that he staggered around the room half drunk, though he certainly had taken no alcohol." Drenched in beer fumes Hitler needed no oral intake to have been inebriated. In any case, he soon "roared at Graf: 'Get me a stein!' He wanted beer" (Konrad Heiden, *Der Fuehrer* [Cambridge, Mass., 1944], 186-88).

Despite the economic crisis of Germany, statistics for the year 1929-30 reveal that the average male over fifteen had consumed the staggering amount of 263 liters of beer (Reinhard Kraut, *Tatsachen. Alkoholnot auch heute noch* [Berlin, 1933], 3). "In contrast to other European and non-European countries," writes a contemporary researcher of the treatment of alcoholics, "Germany remains shamelessly indifferent to the problems of drunkenness. Indeed, it seems we place a premium upon drunkenness, when our laws make it a justification for lesser penalties and even of acquittal" (Hans Clausen, *Die Behandlung der Trunkenheit nach geltendem Reichsrecht . . .* [Coburg, 1928], 1). It seems to me that the huge amounts of alcohol consumed in the "capital of drunkenness," as Weimar Germany was so aptly named, should have been taken into account in any attempt to evaluate the actions of German youth. Imagine a future U.S. historian overlooking marijuana, heroin, and other drugs, as well as alcohol, in attempting to understand the young people of the current era.

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City University of New York

## MR. LOEWENBERG REPLIES:

Professor Stanley W. Page is correct in saying that my work focuses on Germany, deals less with Austria, and does not engage with the problems of Hungary. I never presumed to do a comparative study, although a series of studies of comparative civilian deprivations in World War I and their aftereffects would be most welcome. Page is mistaken when he equates the political effects of a lost war and a toppled regime in Hungary with the meaning of the demise of the *Kaiserreich* for Germany. The relationship of the Hungarian nation to Kaiser Franz Josef and his Habsburg heirs is not comparable to that of the German nation to the Hohenzollerns. See, for example, the memoirs of the Austro-German centralist, Arthur Count Polzer-Hoditz, of March 6, 1915, in which he proposes to the heir apparent, Archduke Charles, the shifting of the "center of gravity" of the Empire to Budapest "to gain the political force of Hungary for the Empire" (as quoted in Robert A. Kann, *The Multinational Empire: Nationalism and National Reform in the Habsburg Monarchy, 1848-1918* [New York, 1950], 1: 391 n.86). "The Hungarian problem" was, in the words of Oscar Jaszi, "the cornerstone of the whole system" (*The Dissolution of the Habsburg Monarchy* [Chicago, 1961], 450). The differences in attachments to the prewar system between Germany and Hungary are so numerous that the two cases defy facile comparison.

Other parts of Page's letter are very difficult to take seriously. I have the feeling that he is "putting us on" when he takes me to task for underrating "the traditionally anal orientation in German psychohistory." To talk this way is very bad psychology and impossible history. All humans have oral, anal, and genital as well as other drives and drive derivatives, defenses, and sublimations at all times. What is important is how these drives are handled, how and in what form these character structures and personality modes are activated. How are they adapted to the reality demands of a historical situation? And what evidence does Page present? Not a modal personality study nor an anthropological study such as Rodnick's, which found that shortly after World War II in Germany "among all classes, toilet training begins

when the infant is about five months old" (David Rodnick, *Postwar Germans: An Anthropologist's Account* [New Haven, 1948], 18). We get, instead, Luther's constipation, Marx's hemorrhoids, Freud's auto-erotic fantasies (whatever Page may mean by that), and Hitler's "anal rages." Page makes no attempt to move from the individual to the social level and from a static to a temporal historical process. The critical question in history as well as in psychology is: why now? Why this combination of events at this particular time?

Could Page possibly be serious in asking the "effect" of alcoholism upon the Nazi movement? Again, this reads like a bad joke. The massive and habitual use of alcohol, just as with the abuse of narcotics, is a defense against anxiety, a means of coping with inner stress and tension. The appropriate historical question is: what were people so anxious about that they drank so much?

Page's questions leave out the entire historical dimensions of actions and events that are time specific. To say that a nation is oral or anal will not take one far. All peoples have always been subject to phase specific developmental drives. But to work at showing a relationship between when a culture historically became sadistic due to a time specific regression to an early period of oral deprivation may begin to get us somewhere.

PETER LOEWENBERG  
University of California,  
Los Angeles

## TO THE EDITOR:

Alas. In his review of Carl Binger's *Thomas Jefferson: A Well-Tempered Mind* (*AHR*, 77 [1972]: 204-05), Mr. James L. Bugg, Jr. wrote that Jefferson's "well-balanced personality was a fortunate blend of masculine and feminine traits." Masculine traits accounted for Jefferson's "aggressive characteristics, his executive ability, the remarkable precision of his mind," while feminine traits "determined his esthetic and artistic temperament, his tenderness and concern for others [!], his sensitivity and shyness, and his love of solitude."

Now really, this is ridiculous. One need not be a "women's libber" to be pulled up short by

the sexual ascription of these "traits," most ludicrously the type-casting of females for tenderness, sensitivity, shyness, and (the most universal nonmale characteristic of all, I am sure) love of solitude.

If these are Mr. Binger's views, Mr. Bugg does his readers a disservice in not probing a historical work based upon such assumptions. If the views are Mr. Bugg's, then he has done a disservice not only to shy men and precision-minded women, but, indeed, to all.

FREDERICK H. SCHMIDT  
*College of William and Mary*

#### MR. BUGG REPLIES:

As I have already indicated in my review, the "views" to which Mr. Schmidt refers are those of the author. My assignment as the reviewer was to summarize, insofar as I could within a seven-hundred-word limitation, the principal theses of the two authors. Rather than devote the allocated space to a critique of the positions of the authors, with whom I have disagreements, I chose to state their respective positions, point to their common characteristics, and leave it to the reader to decide whether he wished to read the books. If so, he can make up his own mind on their validity. To have done otherwise would have been, in my judgment, a "disservice" to both the authors and to the reader.

I do not concur with the particular thesis of Mr. Binger, but to have developed rationally and fully the reasons for my disagreements would have required a longer review than I was requested to write unless, like Mr. Schmidt, I had chosen to indulge in such meaningless and unscholarly adjectives as "ridiculous" and ludicrous. To have adopted such a procedure might have been clever but hardly equitable or rational and would have represented, in my judgment, the real "disservice."

JAMES L. BUGG, JR.  
*Old Dominion University*

*The following letters have been received in connection with the publication of Jacques Barzun's article, "History: The Muse and Her Doctors," AHR, 77 (1972): 36-64.*

#### TO THE EDITOR:

Jacques Barzun's article does scant justice to recent developments in the field of psychohistory. Although he expresses solicitude for the methodological problems of psychohistory, he sheds very little light on this important subject. He has consulted the wrong sources. How he could consider David Donald, Stanley Elkins, and Harry Barnard psychohistorians is beyond my understanding. Their work neither reflects the methodology of psychohistory nor the progress that has been made in this discipline over the past few years. In order to understand the nature of psychohistory one must read, for example, the contributions of Peter Loewenberg, Robert G. L. Waite, John Demos, and Fred Weinstein. Contrary to Mr. Barzun's introductory remarks (pp. 36-39), psychohistory is a new discipline. While it is true that many methodological issues remain unresolved, his assertion that no "methodological procedure" is apparent in psychohistory is without foundation. Such an assertion rests on a cursory, selective, and jaundiced appraisal of the available literature.

Barzun maintains that the "technical jargon [of psychohistory] does not add to understanding" (p. 63). He contends that psychohistory merely restates in technical language what historians have always known. This objection is invalid. Concepts such as sublimation, reaction-formation, identification, the self, character defense, etc., have specialized meanings that incorporate new insights into personality functioning. It is time for historians to understand this.

Even though Barzun does raise some fundamental questions concerning psychohistory, problems related to the historian's choice of psychological theory, and his selection and evaluation of evidence, Barzun's treatment of these issues is fragmentary, superficial, and obscurantist. If we are to strive for simplicity, stability, and consensus in the writing of history, as Barzun suggests that we should, then we necessarily lose sight of important dimensions of human life that cannot be encompassed within this innocent philosophy of history. Instead of writing for a book-of-the-month-club audience, we should strive to communicate our insights,

in their full complexity, to whomever will listen. Life is full of ambivalence, complexity, and irresolution; unless we come to grips with this reality we fail in our essential task as historians.

JOHN J. FITZPATRICK  
University of California,  
Berkeley

#### TO THE EDITOR:

My long-standing concern with the bearings of modern psychology on the treatment of historical subjects led me to read Mr. Barzun's lengthy analysis twice over and with considerable care. Yet I am still not clear as to the cause or causes of the author's unhappiness. His objective seems to be to deflate the notion of "psycho-history." He does not like the term. Neither do I. More important, he denounces all efforts to establish it as "a new discipline," a "new genre." He ridicules the effort to apply "new methods rejuvenating history" and "to create new fields by using new methods." He obviously considers me the prime mover in this nefarious enterprise, despite the fact that I never contemplated any such effort. On the contrary, I would deplore any further fragmentation of history, and I have long preached the need for greater synthesis. I cannot, of course, speak for the many scholars, mostly younger men and women, who are now taking an active interest in the application of modern psychological concepts to the elucidation of historical characters and situations, that is, in "psycho-history." But I do not recall having ever heard any of them advance such extravagant notions as Mr. Barzun attributes to them.

Mr. Barzun, while constantly talking about "history," actually devotes most of his essay to biography, a form of literature in which, as we all know, the usefulness of the psychological approach has long since been recognized and demonstrated. His attention to really historical problems is almost completely confined to the modest attempt I made, some fifteen years ago, to determine the possible psychological and spiritual impact of a major human catastrophe such as the Black Death of the fourteenth century. He quickly comes to the startling conclusion that all I had to say had long been known

and that the use of psychoanalytic concepts amounted to little more than the substitution of a newfangled terminology for good old English words. Had I only known how easy the solution of the problem was, I would certainly have appealed to Mr. Barzun for help before spending time and effort on it. It may interest Mr. Barzun to know, if he is not already aware of it, that European scholars are still wrestling with such questions as that of the influence of the Black Death on the development of religious dissent and protest through Wyclif and Hus to Luther and the Reformation.

Mr. Barzun underlines and emphasizes what most of us have known for a long time, namely that from the beginnings of historiography among the Greeks to the present day there have been men with a keen intuitive understanding of the strengths and weaknesses of human nature. He makes great play with the fact that a major historian such as Karl Lamprecht urged in the early years of the century that more attention should be paid to psychology by historians. He might well have added that Eduard Bernheim, in his great *Lehrbuch der historischen Methode* (8th ed., 1908), discussed the importance of the psychological factor in history at considerable length.

These men, naturally, were talking in terms of the psychology of their day. They knew nothing of the Freudian doctrine and the variants that derived from it, that is, nothing of the theories that have revealed much of the unconscious and irrational forces of history. Mr. Barzun objects to the fact that the "psycho-historians" tend to put all their eggs into the Freudian basket, although I made a point in my address that historians need have no commitment to any particular brand of history, but should use whatever promised to help them in solving their problems. If the Freudian doctrine is still preferred, it is chiefly because it alone provides a coherent theory of personality development. It does not advance formulas or attempt conclusions, but it tries to show the wide variety of unconsciously determined reactions to external events and the many defense mechanisms to which human nature resorts.

It is generally recognized, I think, that over the past decades psychoanalysis has influenced

profoundly not only psychiatry, but the entire field of the arts and many of the social sciences, notably anthropology. In short, it has forced a revision of traditional views of man and society. The historians alone, among the social scientists, have until recently been loath to take into account unconscious and irrational forces. Many now recognize that the application of psychoanalytic (or related) concepts may throw light not only on individuals of historical importance, but on major mass movements and changing attitudes. Admittedly all these efforts are only in the beginning; admittedly they bristle with difficulties; admittedly they have as yet produced no resounding success. There is no question here of establishing a new discipline but simply of exploring a new approach to problems, many of which have long puzzled the historian. After all, there is no reason why, where the sources and the evidence permit, the student should not weigh the psychological as well as the economic, military, religious, or other components of a historical problem. It is regrettable, to put it mildly, that a scholar of Mr. Barzun's standing should devote a long essay to the effort to disparage the experiment and that he should becloud the issue by constantly confusing biography and history.

The most baffling part of Mr. Barzun's essay comes in the concluding paragraphs, where he seems to be prepared to eat his own words. "I would close," he writes, "by suggesting that no one should prejudge the results of psycho-history and its siblings. Nothing I have said or implied about them is such a prejudgment or goes to the matter of the potential usefulness of their projected gains." If these are indeed Mr. Barzun's real sentiments, the reader is entitled to ask why it was necessary to plow through twenty-eight pages of platitudes, insinuations, and charges. However, if Mr. Barzun is prepared to reverse himself, I for one will raise no objection.

WILLIAM L. LANGER  
Harvard University

#### TO THE EDITOR:

It is difficult to reason with those, like Professor Barzun, who presume to tell the rest of the

profession what is and what is not history. I will limit myself to two questions.

William James was a profound observer of human phenomena, including religious experience, habit, and many others. However, to tell us, as Barzun does, that James's work of 1890, *Principles of Psychology*, is "the classic of psychology perhaps most useful to the historian and biographer" (p. 51 n.31), is quite another matter. Are we to infer that all that has been developed in the clinical practice and theory of psychology in the last eight decades is of less value to the modern historian than the work of James? This may be so for Barzun, but it is not so for many of us who wish to live in the twentieth century. Novelty is certainly no criterion of value. It is equally faddish to cling only to the "classics." Some new things have been learned in human psychology since 1890, including of course the insights of Sigmund Freud, who developed the initial theories of psychoanalysis from 1895 to 1899, theories that William James treated with respect. "The future of psychology belongs to your work" James told Ernest Jones in 1909 (Ernest Jones, *The Life and Work of Sigmund Freud*, vol. 2: *Years of Maturity, 1901-1919* [New York, 1955], 57). Much has developed in both clinical technique and theory since those early decades. Historians are obviously free to neglect these developments. But I wonder whether Barzun would also use the anthropology or the linguistics of 1890 as his research models? Or, more to the point, would he select for his family a surgeon, a pediatrician, or a gynecologist whose thought had not developed in eighty-two years?

Barzun concludes his essay with the injunction, with which I concur, that "histories and biographies should always be rewritten. Each viewer remakes a past in keeping with his powers of search and vision, whose defects readily disclose themselves in his work" (p. 61). If this is so for history, why does Barzun not allow for the same relativism and evolution in psychology? Just as we are now in a different world, with its own psychological and historical perspectives, than were the men and women of 1890, so those who write history and psychology in the year 2050 will have their own position in history and accompanying unique perspectives on it. They will also have other psychological theories than we do. We cannot

know what kinds of synthetic history they will write. Why then is Barzun so concerned with the "permanence" of history and the "vogue" of psychology? The nature of both science and history is that the researcher commits himself to what appears to be the most probable hypothesis at the time. The test is whether the theory indeed leads him to new relationships

and data. If it does—and not even Barzun will deny that depth psychology has illuminated institutional life and personal biography with new richness, complexity, and insight—can one reasonably ask for more?

PETER LOEWENBERG  
*University of California,  
Los Angeles*

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## Recent Deaths

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GERAINT N. D. EVANS, associate professor of history at Richmond College of the City University of New York, died in an automobile accident on December 29, 1971, near Laguna Beach, California. Only thirty-six years old at the time of his death, Evans had already compiled a remarkable record as a student of the old British Empire and in the process had become a striking symbol of Anglo-American scholarship.

Born in Wales, educated at Trinity College, London, and at Cambridge University (B.A., M.A.), Geraint Evans early developed an interest in the history of North America and thus came to the United States to further his studies under Lawrence Henry Gipson at Lehigh (M.A.) and under Robin W. Winks at Yale, where he earned his third master's degree and his Ph.D. In the course of his studies he held fellowships from Great Britain, Canada, and the United States. His teaching career proved equally Anglo-American: Malvern College in England, McGill University in Canada, and Lafayette College, Southern Illinois University, Chapman College, and Richmond College in this country—thus spanning the nation from coast to coast. At each campus his restless enthusiasm, exacting standards, and uncompromising candor awed his students and colleagues but also earned their respect and in most cases their affection. In his first and only year at Southern Illinois the senior class voted him one of three outstanding teachers. At Chapman College in California, where he served as visiting professor in 1970-71, Evans so impressed his fellow historians that they sought to keep him as a permanent member of the staff. At Richmond College, his longest and last post, Evans specialized in American colonial and

revolutionary history and American Studies, served as deputy chairman of the division of social sciences, and was elected the first secretary of the college faculty. He also found time to participate in innumerable professional organizations, including the Canadian Association for American Studies (president, 1964-66), the American Studies Association (he was president of the New York City branch from 1969 to 1970), and the Columbia University Seminar on Early American History and Culture. He published several books and articles, perhaps the most significant being *Uncommon Obdurate: The Public Careers of J. F. W. DesBarres* (1969). He had recently begun work on a history of Canada, to be aimed at American readers, and an edition of a newly uncovered diary of an American loyalist.

With the death of Geraint Evans the profession lost an accomplished scholar, an inspiring teacher, a warm friend, and a young man of awesome energy.

ALDEN T. VAUGHAN  
*Columbia University*

GUSTAVE EDMUND VON GRUNEBaum, professor of Near Eastern history and director of the UCLA Near Eastern Center, died in Los Angeles on February 27, 1972, after a two-month illness. The wide-ranging interests, accomplishments, and intelligence of Professor von Grunebaum were matched by his human sympathy and understanding, and his departure leaves a gap that is impossible to fill.

Professor von Grunebaum was born in Vienna on September 1, 1909. He received his doctorate with honors from the University of Vienna in 1931, and then did postdoctoral work at the University of Berlin. To a child-



hood mastery of English and French he later added a command of Italian, Spanish, Latin, Greek, and classical culture, and a university-acquired knowledge of the Arabic, Persian, Turkish, and other Near Eastern languages and cultures. To his linguistic and literary knowledge he joined a broad understanding of human society that encompassed many of the usual academic disciplines, including history, anthropology, philosophy, and religion. The originality of his mind combined with indefatigable scholarship and an almost photographic memory to lay the foundation for an outstanding series of scholarly books and articles. His scholarly production was equaled by his teaching excellence, his human wit and warmth, and his unparalleled abilities as an organizer and administrator.

Professor von Grunebaum's first book, on early Arabic poetry, was published in German in 1937. Leaving Vienna when the Nazis came in 1938 he taught at the Asia Institute in New York as assistant professor from 1938 to 1942 and as chairman of the department of Arabic in 1942-43. From there he went to the University of Chicago, where he served as assistant professor of Arabic (1943-46), associate professor (1946-49), and professor (1949-57). He came to UCLA as professor of history and director of the newly-formed Near Eastern Center in the fall of 1957.

At UCLA Professor von Grunebaum carried out a multifaceted activity that was the envy of his colleagues. With the encouragement of the UCLA administration he built up a large Near Eastern Center that soon came to rank among the very best in the world. With his wide reading and frequent far-flung travels he came to meet or know about talented individuals in all parts of the world and in all stages of their careers. Free of prejudices regarding nationality, intellectual approach, university origin, or sex, he was able to recommend to the various departments that ultimately made employment decisions a multitalented group of individuals, the vast majority of whom came to and stayed at UCLA. For the UCLA history department he was also able to discover scholars of various nationalities outside the Near Eastern field, and his presence helped attract noted scholars to the department.

To the UCLA Near Eastern Center Professor

von Grunebaum was able to bring visiting professors and lecturers of distinction and an increasing number of talented students, both groups coming from many lands. Always ready to listen to the problems and advice of others he could reserve final decisions to himself, confident of the general validity of his own judgments. He created a center free of factions and personal animosities and characterized by a spirit of mutual cooperation, friendship, and respect. He sponsored a series of international conferences and created the Levi Della Vida Conference and award in Islamic Studies given biannually at UCLA, the papers of which he also edited (1970, 1971).

After a full day at the university, promptly answering a voluminous correspondence, teaching, seeing students and colleagues, and doing more than his share of committee work, Professor von Grunebaum turned at night and on weekends to the scholarly production that never let up in quantity or quality. Having already written *Medieval Islam* (1946), *Islam* (1955), and several other books before coming to UCLA, he went on to write *Modern Islam* (1962), *Classical Islam* (1970), and others after arriving here. Taking a total cultural approach to the civilization of medieval and modern Islam he was able to elucidate the interaction of various elements in Islamic society and to compare them with classical antiquity, medieval Byzantium and Europe, and the modern West. Always full of original ideas, his books, like his knowledge, transcend the usual academic disciplines and fall into no easily definable categories. While centering on his lifelong interest in medieval Islamic civilization, his writings sometimes ranged far afield into such topics as African literature and, in his last years, the history of the human imagination, on which he was organizing a conference in 1972. He was especially the master of the short idea-centered essay, and many of his books consist of such interrelated essays. In his last years, on the other hand, he was working on a full-scale survey of Islam as a religion. His works reflect his knowledge of all the subfields of Islamic studies and his rich imagination. Several of his books have been translated into various languages, and his scholarly reputation is worldwide. In addition to his own books and articles he edited a series of books based pri-

marily on the multidisciplinary international conferences he sponsored, of which *Unity and Variety in Muslim Civilization* (1955) and *The Dream and Human Societies* (1966) may be mentioned as representative examples.

Serving on the boards of numerous national and international organizations and journals, he was president of the American Research Center in Egypt since 1966, editor in chief of the *Bibliothek des Morgenlandes* (Switzerland), and coeditor of *Studia Islamica* since 1971. His academic honors, like his other activities, are too numerous to mention here, where it will only be noted that he was a fellow of the American Academy of Arts and Sciences and a member of the American Philosophical Society. He was Faculty Research Lecturer at UCLA, and he received honorary doctorates from the University of Frankfurt and Hebrew Union College as well as being elected to the Institut d'Egypte in Cairo. He was also one of the few non-Italian fellows of the Accademia Nazionale dei Lincei.

In personal relationships, after an initial shyness and reserve, he showed himself to be warm and friendly, ever willing to listen to personal or academic problems and to offer sympathy and advice. His sparkling wit and humor surfaced in the most casual, as well as the most serious, conversation, but was rarely exercised at the expense of others, however distant they might be. He liked to keep his own counsel about his personal and academic plans and activities and to make his own decisions, but he sought out and carefully considered the advice of those whose opinions he respected.

He was able to turn his full attention from one activity to another, from a complex and tiring international trip to a minor problem, from administration to teaching and scholarship, and from serious affairs to light banter, without missing a beat. Very rarely manifesting worry or anger, he scarcely discussed his own problems or achievements, while reacting with support and interest to those of others. An unfailingly interesting conversationalist and lecturer, his talk ranged from the mundane and particular to the most esoteric and abstract of subjects.

Professor von Grunebaum leaves behind his devoted and extraordinary wife Giselle, his two daughters, Claudia and Tessa, and his grieving

students and colleagues both at UCLA and throughout the scholarly world. He was a man of genius, warmth, optimism, multifaceted talents, indefatigable energy, and unparalleled achievements.

NIKKI R. KEDDIE  
*University of California,  
Los Angeles*

ALBERT DENNIS KIRWAN, scholar, university administrator, and former football coach, died suddenly on November 30, 1971. He was born in Louisville, Kentucky, on December 22, 1904, attended the public schools of that city, and in 1926 received the bachelor's degree at the University of Kentucky, where he was a star football player. Returning to Louisville as a teacher and a coach, he attained such outstanding success in the latter capacity that he was made head football coach at the University of Kentucky in 1938, a position he held for a half-dozen years.

An invitation, during World War II, to teach part time in the department of history of the University of Kentucky led him to a decision to abandon his career as coach and commit himself to historical scholarship. After receiving his M.A. in 1945 at the University of Louisville and his Ph.D. at Duke University in 1947, he returned once again to the University of Kentucky as associate professor of history and dean of men. Two years later he became dean of students, a position he held until 1955, when he resigned in order to devote full time to history.

His first book, *Revolt of the Rednecks*, had been published in 1951, and now, freed from administrative duties, he plunged enthusiastically into research and teaching. He edited two volumes, *Johnny Green of the Orphan Brigade* (1956) and *The Confederacy* (1959), while he worked on his prize-winning biography, *John J. Crittenden: The Struggle for the Union* (1962).

His reputation as a scholar and an administrator made him a logical choice for the position of dean of the graduate school, a position to which he was appointed in 1960. For six highly successful years he presided over the most marked period of improvement in quality and expansion of programs, enrollment, and support that has been known in graduate edu-

cation at the University of Kentucky. His return to full-time teaching in 1966 was short lived, for in 1968 he agreed to become acting president of the university. His administrative ability, his good judgment, his calmness, and his solid reputation for fairness were never more needed than during the months he filled this office, months when the institution was the subject of attack from within and without. In 1969, when a president was chosen and the acting presidency came to an end, the board of trustees paid tribute to Dr. Kirwan by designating him, retroactively, as the seventh president of the university.

Once again in the history department Dr. Kirwan looked forward to devoting the remainder of his active life to scholarship and teaching. He had published, with Thomas D. Clark, *The South Since Appomattox* (1967), and in the following year he was editor of *The Civilization of the Old South: Excerpts from the Writings of Clement Eaton*. His last writing to appear in print is a section on the Civil War in *Kentucky: A Pictorial History* (1971), edited by J. Winston Coleman, Jr. At the time of his death Professor Kirwan had completed research for a biography of Henry Clay.

No mere listing of publications and positions can reveal the services Professor Kirwan rendered to his profession and to his university, community, and state. He was the recipient of numerous honors and awards, including a Guggenheim fellowship, 1960-61; a Fulbright lectureship at the University of Vienna, 1966-67; and the Charles S. Sydnor Prize of the Southern Historical Association for his biography of Crittenden. On his own campus he won an alumni award for teaching and another for research, he was elected Hallam Professor of History (1967-69), and he was chosen by his colleagues in the College of Arts and Sciences as Distinguished Professor for the academic year 1967-68.

He was a diligent scholar, a gifted teacher, a wise administrator, and a considerate friend. All who knew him will long recall his warm personality, his kindness, and his deep enjoyment of life.

To perpetuate his memory, the Albert D. Kirwan Memorial Fund has been established at the University of Kentucky under the care of the University Development Office. A deci-

sion as to the nature of the memorial has not yet been reached.

JAMES F. HOPKINS  
*University of Kentucky*

MACY J. MARGOLIS, the coordinator of research services at the Boston Public Library, died suddenly on January 28, 1972, at the age of forty. A graduate of Boston University, from which he also received a master's degree in 1952, Mr. Margolis served in the United States Army as a Russian language specialist from 1952 to 1955. He then returned to the Boston Public Library, where he had begun working as a page in 1945. Mr. Margolis combined his career in the library's history department with advanced study, receiving a master's degree in library science from Simmons College in 1959. In 1967 he was promoted to the position of coordinator of research services, a position in which he gave invaluable service in plans being made for the development and strengthening of the collections of the research library services. Mr. Margolis is survived by his wife Esther and their three sons.

ROBERT L. MELKA died in a fishing accident on Lake Michigan on December 29, 1971. Dr. Melka was born in Chicago on July 1, 1932, and spent his early years in Minnesota. He pursued a deep interest in history and diplomacy with training at the College of St. Thomas, the Université de Grenoble, and Georgetown University. He received his M.A. from the latter in 1958. Dr. Melka interrupted his studies to serve with the U.S. Army in Germany. He returned to the United States to continue his academic career at the University of Minnesota, where he earned the Ph.D. degree in 1966. Dr. Melka taught at Wisconsin State University, Oshkosh, from 1965 to 1970. He joined the department of history at Hope College in September 1970. He had only begun to publish his extensive research on World War II at the time of his death.

The editors of *Societas* have established a national award in Dr. Melka's name. The department of history of Hope College has created a Robert L. Melka Memorial Fund.

HENRY BAMFORD PARKES died in New York on January 7, 1972, after a protracted illness. A

few months earlier he had retired from New York University, where he had been on the faculty since 1930. Born in Sheffield, England, on November 13, 1904, the son of a Methodist minister, Parkes attended Oxford on a scholarship and received his B.A. in 1927. In that year he came to the United States, where he completed his Ph.D. at the University of Michigan in 1929. His dissertation on Jonathan Edwards, published in 1930, testified to an interest in intellectual history and the special qualities of American evolution that would occupy him for the rest of his life.

Parkes's second major publication, however, represented something of a diversion. An opportunity to visit Mexico at a time when he was deeply interested in radicalism resulted in *A History of Mexico* (1938), which, despite criticisms for its leftist tone, was praised for its readability and accuracy. Although it has been revised only slightly, it remains the most satisfactory popular history of Mexico in English.

*Marxism, an Autopsy* (1939) signified for Parkes a partial renunciation of radical solutions as he offered a realistic appraisal of Marx that was regarded as seminal and distinctive. A textbook, *Recent America* (1941), received compliments for its soundness and its mordant and witty style.

In *The American Experience* (1947) Parkes emphasized the agrarian tradition as fostering freedom and equality, in contrast to the degrading effects of industrialization. A few historians who disagreed with his thesis acknowledged that his views were often original and penetrating. David M. Potter praised the 1955 revision as an outstanding interpretive work by a richly informed mind, a trenchant and most important book. Parkes's textbook, *The United States of America* (1953), was comprehensive but also primarily interpretive, favoring the Democrats, and it was widely used. Reinhold Niebuhr described it as "a masterpiece of scholarship and wisdom."

Always reaching for new meanings, Parkes turned to the history of Western thought and culture. Two books of his projected series, *Gods and Men* (1959) and *The Divine Order* (1969), became best sellers. His third volume on the seventeenth and eighteenth centuries was not completed at the time he was stricken. Parkes stressed the connection between free-

dom and creativity and the social significance of art. He held that mankind was happiest while developing and accepting myths or ideologies, and that it suffered chaos, war, and tyranny when consensus was undermined. His interpretations, bold but not smug, rested on enormous learning, and his style, always lively, was at its most stimulating in these final works.

Henry Parkes was always a popular teacher at New York University, whether he presented intellectual history, United States foreign policy, or the development of Latin America. He enjoyed lecturing to huge classes. After Parkinson's disease weakened his voice he offered only seminars. Here he also excelled, with his gentle and probing manner drawing the best out of his students. In the last course he gave the students accorded him in an evaluation the highest rating. A speed reader of prodigious skill, he supervised countless theses, caring little for pedantry but demanding that the writers show a grasp of the larger implications of their topics. He was exacting and yet infinitely patient.

As author of numerous articles, often on topical subjects, visiting lecturer in various institutions, and editor of a formidable list of books, Parkes also exerted wide influence. Among his achievements was his founding and direction of the American Civilization Program at New York University, in which he united his historical and literary interests. A deceptively casual administrator with an offhand manner who threw out his files at the end of the year, he nonetheless provided the essentials of his responsibility: a sound curriculum, high standards, rigorous examination, and placement. A shy and immensely attractive personality, he leaves many grateful friends and an impressive legacy as a historian.

JOHN EDWIN FAGG  
New York University

RAYMOND DE ROOVER, professor of history at Brooklyn College and the Graduate Center of the City University of New York, suffered a fatal cerebral hemorrhage at his home in Brooklyn on Tuesday morning, March 14, 1972. His sudden death brought to a premature close a career of outstanding merit in his chosen fields of economic history. He had concerned himself largely with the history of money, banking, and price controls, and with

industrial, commercial, and business enterprises and organizations, which he had carefully traced back to their medieval development.

De Roover was born on August 28, 1904, in Antwerp, Belgium, where he completed his early training at the École Supérieure de Commerce. He came to the United States and continued his studies at the Harvard University Graduate School of Business Administration and at the University of Chicago, where he received his Ph.D. in 1943. He began his teaching career at Wells College and later went on to Illinois College and Boston College. He held visiting professorships at Oberlin College and the University of California at Berkeley. In 1961 he came to Brooklyn as professor of history both in the college and in the Graduate Center of the City University of New York. At the latter he was in charge of a colloquium in medieval economic history and, at the time of his death, was directing several doctoral candidates.

Raymond de Roover was an unusually productive scholar. Among his most significant works were *The Medici Bank* (1948), *Money, Banking, and Credit in Mediaeval Bruges* (1948), and *The Rise and Decline of the Medici Bank, 1397-1494* (1963). In addition he contributed to numerous scholarly journals, books, and works of reference in America, Belgium, England, and France, and he served as advisory editor for *Studies in Medieval and Renaissance History*. He further served locally as president of the Medieval Club of New York and he participated in the Columbia University Seminars on the Middle Ages, the Renaissance, and Legal and Political Thought. He also participated in various international economic congresses, the most recent of which were the International Economic Congresses held at Munich in 1965 and at Leningrad in 1970.

Throughout the entire span of his academic career Raymond de Roover's zealous application to his studies won almost immediate recognition and continuing support in the form of grants and fellowships. Among these were a Fulbright fellowship and fellowships and grants from the Belgian Fonds National de la Recherche Scientifique, the Guggenheim Foundation, the Penrose Fund of the American Philosophical Society, the Social Science Re-

search Council, the City University of New York, and the National Foundation on the Arts and Humanities. The prizes and honors he received were numerous and varied. Beginning with the James Bowdoin Graduate Prize for his essay on "The Florentine Woolen Industry" in 1938, he went on to garner the Herbert Baxter Adams Prize awarded by the American Historical Association in 1948 for his *Medici Bank*, the Charles Homer Haskins Gold Medal, granted to him in 1950 by the Mediaeval Academy of America for his book *Money, Banking, and Credit in Mediaeval Bruges*, and the Robert Troup Paine Prize, given by the Harvard University Press, for his *Rise and Decline of the Medici Bank, 1397-1494*. Moreover, in fitting tribute to his contributions to scholarship he was elected honorary member of Phi Beta Kappa by the Wells College chapter and, in 1952, a foreign member of the Royal Flemish Academy of Sciences, Letters, and Fine Arts. In 1969, in what he considered his crowning achievement, he was knighted by royal decree of H. M. King Baudouin I of Belgium as Knight-Officer of the Order of the Crown. His most recent honor was that of election in 1970 as Fellow of the Mediaeval Academy of America. At every stage of his remarkably successful career Raymond de Roover never failed to acknowledge his indebtedness to his wife, Florence Edler, a scholar of merit in her own right, who survives him.

For his colleagues, friends, and students, to whom he endeared himself by the contagion of his ebullient spirit, his enthusiastic concern for scholarly excellence, and his friendly, open, and kindly nature, Raymond de Roover's sudden departure leaves a void that cannot soon be filled. Yet none of them can say that he is not the richer because Raymond de Roover was once nearby.

PEARL KIBRE

Graduate Center,  
City University of New York

When RICHARD HARRISON SHRYOCK expressed the wish nearly fifty years ago to study the history of American public health for his doctoral dissertation he was told firmly that the subject, though interesting, was not "history." When he died this winter, the history of medicine was a recognized academic discipline with university

departments and chairs, a national association, two journals, and other evidences of acceptance. In that development none played a larger part than he or made more distinguished and permanent contributions to both the content and the institutions of the subject.

Born in Philadelphia on March 29, 1893, Shryock attended Philadelphia Central High School, the Philadelphia School of Pedagogy, and the University of Pennsylvania School of Education, graduating in 1917. After war service in the Army Medical School he commenced graduate work at the University of Pennsylvania. He was John Bach McMaster's last assistant in 1919-20, but it was Edward P. Cheyney who encouraged his interest in the history of public health and social aspects of medicine. But Shryock was "in" American history, and, instead of public health, he wrote on Georgia and the federal union in 1350, for which he received his doctorate in 1924.

One of the sources for his dissertation was the letters of Dr. Richard D. Arnold of Savannah. These sustained Shryock's interest in medicine; he edited and published them in 1929 and in the next few years published several articles on medical history. In 1936 appeared *The Development of Modern Medicine*, a work of deep and original scholarship but so unconventional that its publisher hazarded only a small printing and the *American Historical Review* passed it over. Five years later, however, when the book had appeared in a German translation, the *Review* noticed it. Declaring it "high time to call attention to this notable work," the reviewer pronounced it "an invaluable contribution to social history, drawing together, correlating, and clarifying in masterly fashion a complicated and difficult body of material" (*AHR*, 46 [1940-41]: 605). Most of that material was European. In all his subsequent writing, most of it on American history, Shryock never failed to set his story against a broader, European background.

Shryock taught at Duke University from 1925 to 1938 and at Pennsylvania until 1949, when he became director of the Institute of the History of Medicine at the Johns Hopkins University. Retiring in 1958 he returned to Philadelphia as professor at the University of Pennsylvania and librarian of the American

Philosophical Society. In the latter position his influence was imaginative and liberating. In addition he served on many professional boards and committees—the National Historical Publications Commission, the National Portrait Gallery Commission, the Institute of Early American History, and the History of Medicine Study Section of the National Institutes of Health, among others. He was president of the History of Science Society and the American Association for the History of Medicine, and he received the principal award of each. He served as president of both the American and International Associations of University Professors. He retired from the University of Pennsylvania in 1963 and from the American Philosophical Society in 1965; it gave him a quiet pleasure to reflect that he was thrice an emeritus—of Johns Hopkins, Pennsylvania, and the Society.

All the while he was writing steadily. He was the author of *American Medical Research* (1947), *Cotton Mather, First Significant Figure in American Medicine* (1953), *The Unique Influence of The Johns Hopkins University on American Medicine* (1953), *Medicine and Society in America* (1960), as well as of scores of articles. The January 1968 issue of the *Journal of the History of Medicine* contains his bibliography to that date. He corrected the proofs of "The Medical Reputation of Benjamin Rush: Contrasts over Two Centuries" for the *Bulletin of the History of Medicine* (45 [1971]: 507-52) shortly before his death, which occurred while he was on vacation at Fort Lauderdale, Florida, on January 30, 1972.

As a historian of medicine and science, Shryock addressed himself to questions of their interrelations with politics, society, and economics. The subtitle of *The Development of Modern Medicine* was *An Interpretation of the Social and Scientific Factors Involved*. Knowing more medicine than historians or sociologists and having better historical training than any physician, Shryock saw the past in all its variety and complexity, illuminated contemporary problems with historical insights, and asked more questions than he answered. Tolerant by nature and skeptical by training, he constantly explored and probed. He had enough ideas to have founded several

"schools," but he suspected that all schools perpetuate half-truths; and, though he had many students, he had no "disciples."

He used statistics from the days of his earliest research, and in 1960 he organized and conducted a conference on the history of quantification. He sought the company of sociologists and psychologists, and he enriched his own work with understandings drawn from their disciplines. Though not a "revisionist," he held many unorthodox views: he examined every cliché, whether the influence of the frontier, the flowering of New England, or the benefits of federal union; he could write, for example, that "we should guard against the assumption that all major outcomes in the American past must have been for the best." The quality of Shryock's mind and the nature of his contribution are perhaps best revealed in the essays collected and published in *Medicine in America* (1966).

WHITFIELD J. BELL, JR.

*American Philosophical Society*

DALE ALAN SOMERS (1939-72), associate professor of urban history at Georgia State University, died of cancer in Houston, Texas, on March 27. He had begun to make his mark in historical scholarship with articles in the *Southwestern Historical Quarterly*, the *Journal of Popular Culture*, and *Louisiana History*. His first book, *The Rise of Sports in New Orleans, 1850-1900* (1972), was published on the eve of his death. Professor Somers was a very popular teacher who enjoyed great respect from his colleagues for his personal integrity and high standards of scholarship. As president of the local chapter of the American Associa-

tion of University Professors, he worked fearlessly for faculty rights and was known and appreciated by the faculty and administration for his humor, wit, and forthrightness. Born in Galveston, Texas, he attended Southwest Texas State College and Trinity University. In 1966 he received his Ph.D. from Tulane University. He taught at the University of Kentucky and East Texas State University before joining the faculty at Georgia State University in 1968.

Other members of the association who have died recently include: W. W. Bailey of Alexandria, Virginia; George A. Boyd of Middletown, New York; Eugene P. Chase of Hebron, Connecticut; M. D. Condon of Rolling Hills, California; M. Contopoulos of Jackson Heights, New York; L. Corning, Jr., of San Antonio, Texas; J. A. Fiorentino of Trenton, New Jersey; C. Warren Griffiths of Wilmington College in Wilmington, Ohio; Colonel Edward P. Hamilton of Milton, Massachusetts; E. H. Heintzen of Springfield, Illinois; E. S. Hoyt of Montreat, North Carolina; Donald R. McVeigh of Hyattsville, Maryland; Félix Martí-Ibáñez of New York, New York; Frank S. Meyer of Woodstock, New York; Ludwell L. Montague of Ware Neck, Virginia; Robert Cornelius Murray, former senior master at the Riverdale Country School, Riverdale, New York; Coen Gallatin Pierson of Illinois State University in Normal; Robert G. Ramsay of Orange, California; J. H. Rubenstein, an instructor at Kalamazoo Valley Community College in Kalamazoo, Michigan; Cortland V. Smith of Louisburg, North Carolina; and F. S. Wise of Bolivar, Missouri.

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## Association Notes

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Dr. Charlotte Alison Quinn has been appointed special assistant to the AHA's Committee on Women Historians for the year 1972-73. A specialist in African history, Dr. Quinn, author of *Mandingo Kingdoms of the Senegambia* (1972), received the Ph.D. from the University of California, Los Angeles, in 1967. In her new post Dr. Quinn succeeds Dr. Dorothy Rabin Ross, who has joined the department of history at Princeton as assistant professor.

Mrs. Janet Croly Hayman, assistant editor of the *AHA Newsletter* for the past year, has now been appointed as an assistant editor of the *AHR* as well.



## Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

BERLIN, CHARLES, editor. *Studies in Jewish Bibliography, History, and Literature in honor of I. Edward Kiev*. New York: Ktav Publishing House, Inc. 1971. Pp. 587. \$25.00.

SALO W. BARON, A Collection of Hebrew-Latin Aphorisms by a Christian Hebraist. ABRAHAM BERGER, Approaches to Rabbi Nachman and His Tales. CHARLES BERLIN, A Sixteenth-Century Hebrew Chronicle of the Ottoman Empire: The *Seder Eliyahu Zuta* of Elijah Capsali and Its Message. GEORGE L. BERLIN, The Jewish Labor Committee and American Immigration Policy in the 1930's. WILLIAM G. BRAUDE, Maimonides' Attitude to Midrash. ARTHUR A. CHIEL, Ezra Stiles and the Polish Rabbi. MARTIN A. COHEN, The Rebellions during the Reign of David: An Inquiry into Social Dynamics in Ancient Israel. PHILIP GOODMAN, Jewish Bookplate Literature: An Annotated Bibliography. ALFRED GOTTSCHALK, Ahad Ha-Am as Biblical Critic—A Profile. Y. KANTOR, translated and annotated by A. A. GREENBAUM, Some Notes and Conclusions about the Published Totals of the Soviet Census of January 15, 1959. JOSEPH GUTMANN, Jewish Ceremonial Art: A Basic Bibliography. JACOB KABAKOFF, S. B. Schwarzberg (1865–1929)—Hebrew Publisher and Bibliographer. NATHAN M. KAGANOFF, Supplement III: Judaica Americana Printed before 1851. GUIDO KISCH, An Innovator of Haggadah Illustration—Cyril Kutlik. MOSES MARK, A Bibliography of Hebrew Printing in Dyhernfurth, 1689–1718. ISIDORE S. MEYER, The Hebrew Exercises of Governor William Bradford. MICHAEL A. MEYER, Christian Influence on Early German Reform Judaism. LEON NEMOV, Studies in the History of the Early Karaite Liturgy: The Liturgy of Al-Qirqisāni. JACOB NEUSNER, Some Early Traditions concerning Yohanan ben Zakkai. SHLOMO NOBLE, The Jewish Woman in Medieval

Martyrology. HERBERT PARZEN, Observations on Maimonides' *Guide for the Perplexed*. MOSHE PERLMANN, "Talmudic Human Sacrifices": Egypt 1890. MOSHE PELLI, The Methodology Employed by the Hebrew Reformers in the First Reform Temple Controversy (1818–1819). ISAAC RABINOWITZ, A Rectification of the Date of Judah Messer Leon's Death. NAHUM M. SARNA, The Order of the Books. A. SCHEIBER, Bibliographisches aus der Genisa. MENAHEM SCHMELZER, Rashi's *Commentary on the Pentateuch and on the Five Scrolls*, Venice, Bomberg, 1538. EISIG SILBERSCHLAG, Contemporary Hebrew Literature: Source of Untapped Values. FRANK TALMAGE, David Kimhi and the Rationalist Tradition II: Literary Sources. CARL HERMANN VOSS, Letters from Stephen S. Wise to a friend and Colleague: Morton Mayer Berman. MYRON M. WEINSTEIN, A Putative Ceylon Rite. THEODORE WIENER, compiler, Addenda to Yaari's *Bibliography of the Passover Haggadah* from the Library of Congress Hebraica Collection. YOSEF HAYIM YERUSHALMI, *Privilegios del Poderoso Rey Karlo* (1740): A Neapolitan Call for the Return of the Jews, and Its Ladino Translation. HERBERT C. ZAFREN, Dyhernfurth and Shabtai Bass: A Typographic Profile. FRANK ZIMMERMANN, A Suggested Source for Some of the Substitute Names for YHWH.

JÜRGENSEN, KURT, and HANSEN, REIMER, editors. *Historisch-politische Streiflichter: Geschichtliche Beiträge zur Gegenwart*. Preface by KARL DIETRICH ERDMANN. Neumünster: Karl Wachholtz Verlag. 1971. Pp. 295. DM 29.80.

Einleitung: KURT JÜRGENSEN and REIMER HANSEN.

Zur Ideen- und Verfassungsgeschichte: TILMAN KOOPS, Gehorsam und Widerstandsrecht in der Theologie des orthodoxen Luthertums. KURT JÜRGENSEN, Zur Entstehungsgeschichte des belgischen Königreiches: Lamennais und Belgien. HANS PETER MENSING, Die Entwicklung der schwedischen Sozialpolitik im ausgehenden 19. Jahrhundert. DIETER PUST, Die ersten Flensburger Kommunalwahlen auf der Grundlage des "Normativs" von 1833. GERTRUD JUNG, Das schleswig-holsteinische Kommunalwahlrecht 1867–1914.

Zur Geschichte der Parteien und Verbände: HEINZ JOSEF VARAIN, Zur Geschichte und Theorie der Interessenverbände. GERHARD BEIER, Gruppenspezifische Sozialgeschichte am Beispiel der Buchdrucker-gewerkschaft. GERD SCHWIEGER, Der "Verrat" der deutschen Sozialdemokratie: Überlegungen zur Kriegspolitik der SPD im Ersten Weltkrieg. ERASMUS JONAS, Brüning und die Konservativen.

Zur innen- und aussenpolitischen Entwicklung in der Zeit der Weltkriege: ERICH-WOLFGANG HUBRICH, Zur amerikanischen Intervention in Europa 1914-1919: Aussenminister Robert Lansing und Präsident Woodrow Wilson im Spiegel der Lansing Papers. HELMUT GRIESER, Die Rapallo-Politik in sowjetischer Sicht: Zur Beurteilung der deutschen Aussenpolitik 1922-1932 in der zeitgenössischen sowjetischen Presse. JÜRGEN BLUNCK, Der Gedanke der Grossen Koalition in den Jahren 1923-1928. KARL-HEINZ HARBECK, Zur Geschichte der "Zeitschrift für Geopolitik" 1924-1944. HANS-JÜRGEN KRÜGER, Zum Verhältnis von Faschismus und Ständestaat. JENS PETERSEN, Italien in der aussenpolitischen Konzeption Hitlers. KARL-HEINZ MINUTH, Britische Balkanstrategie 1942/43: Betrachtungen zu Churchills "The Second World War." REIMER HANSEN, Die deutsche Kapitulation 1945.

Zur afro-asiatischen Geschichte: ERNST KOHL, Die christlichen Kirchen und die Rassenfrage in Südafrika. JOHANNES H. VOIGT, Nationalismus in der indischen Geschichtsschreibung. BERNHARD DAHM, Werdegang und politische Ideen Sukarnos.

*Papers Presented at the Second Annual New Jersey History Symposium Held December 5, 1970 at the State Museum, Trenton under the Auspices of the New Jersey Historical Commission.* Newark, N. J.: The New Jersey Historical Society. 1971. Pp. 80. \$3.00.

Political Party Systems of New Jersey in the Early National Period: CARL E. PRINCE, The Leadership of New Jersey's First Party System. HERBERT ERSHKOWITZ, The Origins of the Whig and Democratic Parties in New Jersey, 1820-1837. JAMES M. BANNER, JR., Comments.

The Historical Geography of New Jersey: PETER O. WACKER, New Jersey's Cultural Landscape before 1800. THEODORE W. KURY, Iron as a Factor in New Jersey Settlement. A. PHILIP MUNTZ, Comments.

## Other Books Received

Books listed were received by the *AHR* between February 1 and May 1, 1972. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

- ARBORE, VIRGIL, *et al.* *Atlas istoric*. Ed. by VASILICA NEAGU. Bucharest: Editura Didactică și Pedagogică. 1971. Pp. 197.
- BEN-SASSON, H. H., and ETTINGER, S. (eds.). *Jewish Society through the Ages*. New York: Schocken Books. 1971. Pp. 352. \$12.50.
- BESTERMAN, THEODORE. *History and Geography: A Bibliography of Bibliographies*. In 4 vols. The Besterman World Bibliographies. Totowa, N. J.: Rowman and Littlefield. 1972. Pp. 408; 409-802; 803-1196; 1197-1587. \$60.00 the set.
- BLACK, C. E., and HELMREICH, E. C. *Twentieth Century Europe: A History*. 4th ed.; New York: Alfred A. Knopf. 1972. Pp. xxiv, 1008, xxviii. \$15.95.
- BOULDING, KENNETH E., and MUKERJEE, TAPAN (eds.). *Economic Imperialism: A Book of Readings*. Ann Arbor: University of Michigan Press. 1972. Pp. xviii, 338. \$10.00.
- BRIGGS, ROBIN. *The Scientific Revolution of the Seventeenth Century*. Seminar Studies in History. New York: Harper and Row. 1969. Pp. x, 121. \$2.95.
- CARDWELL, D. S. L. *Turning Points in Western Technology: A Study of Technology, Science and History*. New York: Science History Publications. 1972. Pp. xi, 244. Cloth \$10.50, paper \$5.00.
- CHEIN, ISIDOR. *The Science of Behavior and the Image of Man*. New York: Basic Books. 1972. Pp. xii, 347. \$12.50.
- CLAYTON, G. D. *Britain and the Eastern Question: Missolonghi to Gallipoli*. The London History Studies, No. 8. [London:] University of London Press; distrib. by Rowman and Littlefield, Totowa, N. J. 1971. Pp. 256. \$3.00.
- CONSTANTINESCU, MIRON, and LIVEANU, VASILE. *Problems of History and of Social Theory*. Tr. by MARY LĂZĂRESCU. Bibliotheca Historica Romaniae, Studies, No. 30. Bucharest: Publishing House of the Academy of the Socialist Republic of Romania. 1970. Pp. 170. Lei 6.50.
- CORNEHLS, JAMES V. (ed. with an introd.). *Economic Development and Economic Growth*. New York Times Book. Chicago: Quadrangle Books. 1972. Pp. ix, 286. Cloth \$8.95, paper \$2.65.
- DANIELS ROBERT V. *Studying History: How and Why*. 2d ed.; Englewood Cliffs, N. J.: Prentice-Hall. 1972. Pp. viii, 120. \$1.95.
- DORN, GEORGETTE M. (comp.). *Latin America, Spain, and Portugal: An Annotated Bibliography of Paperback Books*. Hispanic Foundation Bibliographical Ser., No. 13. Washington: Library of Congress. 1971. Pp. 180. 75 cents.
- DRENNEN, D. A. *Karl Marx's Communist Manifesto: A Full Textual Explication*. Woodbury, N. Y.: Barron's Educational Ser. 1972. Pp. v, 202. \$1.95.
- ENGELS, FREDERICK. *The Origin of the Family, Private Property, and the State: In the Light of the Researches of Lewis H. Morgan*. With an introd. and notes by ELEANOR BURKE LEACOCK. New York: International Publishers. 1972. Pp. 285. \$7.50.
- EPSTEIN, FRITZ T., *et al.* (eds.). *The American Bibliography of Russian and East European Studies for 1966*. Russian and East European Ser., Russian and East European Institute, Indiana University, vol. 40. Bloomington: Indiana University Press for International Affairs Center. 1972. Pp. xxiii, 148. \$3.50.
- FLATHMAN, RICHARD E. *Political Obligation*. Studies in Political Theory. New York: Atheneum. 1972. Pp. xxvii, 334. \$12.95.
- German Polish Dialogue: Letters of the Polish and German Bishops and International Statements*. Bonn: Edition Atlantic-Forum. 1966. Pp. 127.
- GRIMM, HAROLD J. *The Reformation*. AHA Pamphlets 403. Washington: American Historical Association. 1972. Pp. 33. \$1.00.
- HACOHEN, JOSEPH, and the ANONYMOUS CORRECTOR. *The Vale of Tears* (Emek Habacha). Tr. and with a critical commentary by HARRY S. MAY. The Hague: Martinus Nijhoff. 1971. Pp. xiii, 146. 18 gls.
- HERMANN, WILLIAM. *The Holocaust: From a Survivor of Verdun*. New York: Harper and Row. 1972. Pp. xii, 141. \$5.95.

Humboldt, *Trabajos de*. Ser. Historica, No. 8. Bicentenario de Humboldt. Havana: Academia de Ciencias de Cuba, Museo Histórico de las Ciencias "Carlos J. Finlay." 1969. Pp. 31.

Humboldt, *Trabajos sobre*. In 2 pts. Ser. Historica, Nos. 9 and 10. Bicentenario de Humboldt. Havana: Academia de Ciencias de Cuba, Museo Histórico de las Ciencias "Carlos J. Finlay." 1969. Pp. 38; 31.

INNIS, HAROLD A. *Empire and Communications*. Rev. by MARY Q. INNIS. Foreword by MARSHALL McLuhan. [Toronto:] University of Toronto Press. 1972. Pp. xii, 184. \$2.75.

JORDAN, ROBERT S. (ed.). *Multinational Cooperation: Economic, Social, and Scientific Development*. New York: Oxford University Press. 1972. Pp. xiii, 392. \$4.95.

Keynes, John Maynard, *The Collected Writings of Vol. 2, The Economic Consequences of the Peace*. [New York:] St. Martin's Press for the Royal Economic Society. 1971. Pp. xxv, 191. \$9.50.

LAGO, MARY M. (ed., with an introd. and notes). *Imperfect Encounter: Letters of William Rothenstein and Rabindranath Tagore, 1911-1941*. Cambridge, Mass.: Harvard University Press. 1972. Pp. xiii, 402. \$15.00.

LANGER, WILLIAM L. (ed.). *Perspectives in Western Civilization: Essays from Horizon*. In 2 vols. New York: American Heritage; Harper and Row. 1972. Pp. 287; 286. \$3.25 each.

LIDDEL HART, B. H. *Why Don't We Learn from History?* Rev. ed.; New York: Hawthorn Books. 1971. Pp. 95. \$3.95.

LÓPEZ SÁNCHEZ, JOSÉ. *Humboldt y su época*. Ser. Historica, No. 13. Bicentenario de Humboldt. Havana: Academia de Ciencias de Cuba, Museo Histórico de las Ciencias "Carlos J. Finlay." 1970. Pp. 151.

LOUCA, ANOUAR. *Henry Dunant et les origines chevaleresques de la Croix-Rouge*. Geneva: Association Suisse-Arabe. 1971. Pp. 26.

LUZA, RADOMIR. *History of the International Socialist Youth Movement*. European Aspects: A Collection of Studies Relating to European Integration Published under the Auspices of the Council of Europe. Ser. C: Politics, No. 20. Leyden: A. W. Sijthoff. 1970. Pp. 336. 25 gls.

MALLIN, JAY (ed. and with an introd.). *Terror and Urban Guerrillas: A Study of Tactics and Documents*. Coral Gables, Fla.: University of Miami Press. 1971. Pp. 176. \$7.95.

MERLIN, MARK DAVID. *Man and Marijuana: Some Aspects of Their Ancient Relationship*. Rutherford, N. J.: Fairleigh Dickinson University Press. 1972. Pp. 120. \$8.00.

MEUVRET, JEAN. *Études d'histoire économique: Recueil d'articles*. Cahiers des Annales, 32. Paris: Librairie Armand Colin. 1971. Pp. 344. 35 fr.

MICHEL, HENRI. *La seconde guerre mondiale*. "Que sais-je?" No. 265. Paris: Presses Universitaires de France. 1971. Pp. 126.

NEIDER, CHARLES (ed. with an introd. and notes). *Antarctica: Authentic Accounts of Life and Exploration in the World's Highest, Driest, Windiest, Coldest and Most Remote Continent*. New York: Random House, 1972. Pp. x, 464. \$10.00.

NÚÑEZ JIMÉNEZ, ANTONIO. *Humboldt, espeleólogo precursor*. Ser. Historica, No. 11. Bicentenario de Humboldt. Havana: Academia de Ciencias de Cuba. 1969. Pp. 29.

NÚÑEZ JIMÉNEZ, ANTONIO, and CERMAK, JOSEPH. *Discursos*. Ser. Historica, No. 12. Bicentenario de Humboldt. Havana: Academia de Ciencias de Cuba. 1969. Pp. 12.

O'CONNOR, RAYMOND G. *Force & Diplomacy: Essays Military and Diplomatic*. Coral Gables, Fla.: University of Miami Press. 1972. Pp. 167. \$10.00.

PAYNE, ROBERT (ed. with an introd.). *The Unknown Karl Marx: Documents Concerning Karl Marx*. New York: New York University Press. 1971. Pp. viii, 337. \$8.75.

PESCH, OTTO HERMANN, O.P. *The God Question in Thomas Aquinas and Martin Luther*. Tr. by GOTTFRIED G. KRODEL. Facet Books. Historical Ser., 21 (Reformation). Philadelphia: Fortress Press. 1972. Pp. ix, 38. \$1.00.

PIEL, GERARD. *The Acceleration of History*. New York: Alfred A. Knopf. 1972. Pp. 369. \$8.95.

POULTON, HELEN J., with the assistance of MARGUERITE S. HOWLAND. *The Historian's Handbook: A Descriptive Guide to Reference Works*. Foreword by WILBUR S. SHEPPERSON. Norman: University of Oklahoma Press. 1972. Pp. xi, 304. \$4.95.

PROKOSCH, ERICH (tr., ed., and with an introd.). *Molla und Diplomat: Der Bericht des Ebû Sehil Nu'mân Efendi über die österreichisch-osmanische Grenzziehung nach dem Belgrader Frieden 1740/41*. Osmanische Geschichtsschreiber, No. 7. Graz: Verlag Styria. 1972. Pp. 250. Sch. 160.

ROBERTS, J. M. (ed.). *Europe in the 20th Century: Readings in 20th Century History*. Vol. 2, 1914-25. New York: Taplinger. 1971. Pp. 362. \$7.95.

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#### A GUIDE TO THE ABBREVIATION OF JOURNAL TITLES

GENERAL RULES: Proper names are spelled out with the exception of adjective forms and names of countries used to identify place of publication. Whenever possible articles, conjunctions, and prepositions are deleted. Exceptions are relatively short titles such as *Past & Present* or *The Americas*.

|                   |  |                    |  |
|-------------------|--|--------------------|--|
| <i>Abt</i>        | Abteilung  | <i>assyriol</i>    | assyriological, assyriologie, assyriology    |
| <i>acad</i>       | academia, academy  | <i>at</i>          | atti   |
| <i>accad</i>      | accademia  | <i>Atl</i>         | Atlantic                                     |
| <i>adm</i>        | administration, administrative   |                    |  |
| <i>aff</i>        | affairs  | <i>b</i>           | buch (compounds only)                        |
| <i>af-ric</i>     | africain, African  | <i>balt</i>        | baltisch                                     |
| <i>af-rik</i>     | afrikaanse   | <i>bayer</i>       | bayerisch                                    |
| <i>agric</i>      | agricultural, agriculture  | <i>Beitr</i>       | Beitrag, Beiträge                            |
| <i>AHR</i>        | <i>American Historical Review</i>  | <i>Ber</i>         | Bericht                                      |
| <i>aikakausk</i>  | aikakauskirja  | <i>bibl</i>        | bibliotek, bibliotheca, bibliothèque         |
| <i>Akad</i>       | Akademie   | <i>bibliogr</i>    | bibliografice, bibliographical, bibliography |
| <i>Ala</i>        | Alabama  | <i>bijd</i>        | bijdragen                                    |
| <i>Alas</i>       | Alaska   | <i>biog</i>        | biography                                    |
| <i>alemann</i>    | alemannisches  | <i>Bl</i>          | Blatt, Blätter                               |
| <i>alig</i>       | allgemein  | <i>bol</i>         | boletim, boletín                             |
| <i>Altertumsk</i> | Altertumskunde   | <i>boll</i>        | bollettino                                   |
| <i>alttest</i>    | alttestamentliche  | <i>brandenburg</i> | brandenburgisch                              |
| <i>Am</i>         | American   | <i>bras</i>        | brasileira                                   |
| <i>an</i>         | anales, Annalen, annales, annali, annals, annuaires, annual, annuarium       | <i>braunschw</i>   | braunschweigisch                             |
|                   | ancien, ancient  | <i>Braz</i>        | Brazilian                                    |
| <i>anc</i>        |  | <i>brem</i>        | bremisches                                   |
| <i>anthol</i>     | anthology  | <i>Brit</i>        | British                                      |
| <i>anthropol</i>  | anthropologie, anthropology  | <i>bull</i>        | bulletin                                     |
| <i>antiq</i>      | antiquarian, antiquarisch, antiquarischen, antiquité, antiquities, antiquity | <i>bus</i>         | business                                     |
|                   |  | <i>byz</i>         | byzantine                                    |
| <i>antol</i>      | antologia  |                    |  |
| <i>antropol</i>   | antropologiczny  | <i>cah</i>         | cahiers                                      |
| <i>anz</i>        | anzeiger   | <i>Calif</i>       | California                                   |
| <i>appenzell</i>  | appenzellische   | <i>Can</i>         | Canadian                                     |
| <i>arch</i>       | archiven, archives, archivio, archivo, archiv, Archivum                      | <i>Carib</i>       | Caribbean                                    |
| <i>archaeol</i>   | archaeolog, archaeology  | <i>cath</i>        | catholic                                     |
| <i>archäol</i>    | Archäologie, archäologische, archäologischer                                 | <i>cent</i>        | century                                      |
|                   | archeologie, archeology  | <i>cercet</i>      | cercetări                                    |
| <i>archeol</i>    |  | <i>českoslov</i>   | československy                               |
| <i>Ariz</i>       | Arizona  | <i>chron</i>       | chronicles, chronique                        |
| <i>Ark</i>        | Arkansas   | <i>circ</i>        | circle, circular                             |
| <i>ark</i>        | arkiv  | <i>civil</i>       | civilization                                 |
| <i>arq</i>        | arquivos   | <i>class</i>       | classica, classical, classique               |
| <i>arqueol</i>    | arqueológico   | <i>co</i>          | county                                       |
| <i>assoc</i>      | association  | <i>coll</i>        | college                                      |
|                   |  | <i>collect</i>     | collection, collections                      |



|                 |   |                  |  |
|-----------------|---|------------------|--|
| <i>Colo</i>     | Colorado  | <i>germ</i>      | germanistisch  |
| <i>com</i>      | comité, committee   | <i>Ges</i>       | Gesellschaft   |
| <i>comm</i>     | commerce  | <i>gesch</i>     | Geschichte, geschichtliche   |
| <i>comp</i>     | comparative   | <i>gos</i>       | gospodarczych  |
| <i>compil</i>   | compilation, compiled, compiler   | <i>gouv</i>      | government   |
| <i>concl</i>    | conclusion  | <i>grad</i>      | graduate   |
| <i>conf</i>     | conference  | <i>Grafsch</i>   | Grafschaft   |
| <i>cong</i>     | Congress  |                  |  |
| <i>Conn</i>     | Connecticut   | <i>h</i>         | hefte (compounds only)   |
| <i>contemp</i>  | contemporaine, contemporánea, contemporary                                    | <i>hamburg</i>   | hamburgisch  |
| <i>corp</i>     | corporation   | <i>hann</i>      | hannoversche   |
| <i>corr</i>     | correspondence  | <i>hell</i>      | hellenic, hellénique, hellenistic  |
| <i>c. r.</i>    | comptes rendus  | <i>helvet</i>    | helvetian  |
| <i>crit</i>     | critica, criticism  | <i>hess</i>      | hessisch   |
| <i>cuad</i>     | cuaderno  | <i>hisp</i>      | hispanic   |
| <i>cult</i>     | cultura, cultural, culture  | <i>hist</i>      | histoire, historiae, historiallinen, historical, historická, historický, histórico, historicum, historique, historisch, history, historyczne |
| <i>D. C.</i>    | District of Columbia  |                  | hohenzollernische  |
| <i>Del</i>      | Delaware  | <i>hohenzoll</i> | holsteinisch   |
| <i>demog</i>    | demografie, demographische, demography  | <i>holstein</i>  |  |
| <i>dept</i>     | department  |                  |  |
| <i>deux</i>     | deuxième  | <i>iaz</i>       | iazyka   |
| <i>dev</i>      | developing, development   | <i>Ida</i>       | Idaho  |
| <i>dig</i>      | digest  | <i>Ill</i>       | Illinois   |
| <i>dipl</i>     | diplomatic, diplomatique  | <i>illus</i>     | illustrated  |
| <i>doc</i>      | documentation, documents  | <i>ind</i>       | industrial, industry   |
| <i>dok</i>      | dokuments   | <i>Inda</i>      | Indiana  |
| <i>drev</i>     | drevnei   | <i>individ</i>   | individual   |
| <i>dsch</i>     | deutsche, deutschen, deutsches  | <i>inst</i>      | Institut, institute, institution, instituto  |
| <i>e</i>        | east, eastern   | <i>int</i>       | internacional, international, internationale   |
| <i>ec</i>       | economics, economique, economy  | <i>interdisc</i> | interdisciplinary  |
| <i>eccles</i>   | ecclesiastical  | <i>intern</i>    | internal   |
| <i>ecles</i>    | eclesiástico  | <i>introd</i>    | introduced, introduction   |
| <i>ed</i>       | edited, editor  | <i>ist</i>       | istorii, istorijski, istoriski   |
| <i>educ</i>     | education   | <i>istrusz</i>   | istruzione   |
| <i>EEH</i>      | <i>Explorations in Economic History</i>                                       | <i>ital</i>      | Italian, italiana, italienisch   |
| <i>EHR</i>      | <i>English Historical Review</i>  |                  |  |
| <i>ELH</i>      | <i>English Literary History</i>   | <i>j</i>         | journal  |
| <i>Eng</i>      | English   | <i>Jb</i>        | Jahrbuch, Jahrbücher   |
| <i>epig</i>     | epigraphik, epigraphy   | <i>JEGP</i>      | <i>Journal of English and Germanic Philology</i>   |
| <i>Epis</i>     | Episcopal   | <i>jugoslou</i>  | jugoslovenski  |
| <i>estud</i>    | estudios  | <i>jur</i>       | juridical, juridiceski, juridique  |
| <i>et</i>       | études  |                  |  |
| <i>ethnog</i>   | ethnographisch  | <i>kan</i>       | kanonistisch   |
| <i>ethnol</i>   | ethnological, ethnology   | <i>Kans</i>      | Kansas   |
| <i>etnol</i>    | etnologia   | <i>kath</i>      | katholik   |
| <i>Eur</i>      | Europe, European, européennes   | <i>Kd</i>        | Kunde  |
| <i>ev</i>       | evangelisch   | <i>Kl</i>        | Klasse   |
| <i>explor</i>   | explorations  | <i>Ky</i>        | Kentucky   |
| <i>fac</i>      | faculté, faculty  | <i>La</i>        | Louisiana  |
| <i>Fak</i>      | Fakult  | <i>Landesk</i>   | Landeskunde  |
| <i>fil</i>      | filosofia   | <i>lang</i>      | language   |
| <i>filol</i>    | filologia   | <i>lett</i>      | letter, letterário, lettre   |
| <i>Fla</i>      | Florida   | <i>lib</i>       | library  |
| <i>for</i>      | foreign   | <i>Lib Cong</i>  | U.S. Library of Congress   |
| <i>Forsch</i>   | Forschung, Forschungen  | <i>libr</i>      | librarian  |
| <i>fr</i>       | français, French  | <i>ling</i>      | linguistics, linguistique  |
| <i>francisc</i> | franciscanum  | <i>lit</i>       | literary, literatur, literature, literary  |
| <i>fränk</i>    | fränkische  | <i>lübeck</i>    | lübeckische  |
| <i>frankf</i>   | frankfurter   | <i>lüneburg</i>  | lüneburger   |
| <i>g</i>        | giornale  | <i>mag</i>       | magasin, magazine  |
| <i>Ga</i>       | Georgia   | <i>marit</i>     | maritime   |
| <i>gaz</i>      | gazette   | <i>Mass</i>      | Massachusetts  |
| <i>gen</i>      | general, général  | <i>Md</i>        | Maryland   |
| <i>geneal</i>   | genealogy   | <i>Me</i>        | Maine  |
| <i>geog</i>     | geográfico, geographic, geographical, géographique, geographischen, geography | <i>med</i>       | medieval, médiévale, medievals   |
| <i>Ger</i>      | German  | <i>meded</i>     | mededelingen   |
|                 |   | <i>Mediterr</i>  | Mediterranean  |
|                 |   | <i>mél</i>       | mélanges   |
|                 |   | <i>mém</i>       | mémoires, memorie  |

|                    |  |                  |   |
|--------------------|--|------------------|---|
| <i>mennonit</i>    | mennonitische                            | <i>prot</i>      | protestant, Protestantismus             |
| <i>Mex</i>         | Mexican                                  | <i>prov</i>      | providence, provinces                   |
| <i>Mich</i>        | Michigan                                 | <i>PSQ</i>       | <i>Political Science Quarterly</i>      |
| <i>mid</i>         | middle                                   | <i>psych</i>     | psychology                              |
| <i>midcont</i>     | midcontinental                           | <i>pt</i>        | part                                    |
| <i>mil</i>         | militaire, militarisch, military         | <i>pts</i>       | parts                                   |
| <i>Minn</i>        | Minnesota                                | <i>publ</i>      | publication, publishing                 |
| <i>misc</i>        | miscellany                               |                  |   |
| <i>Miss</i>        | Mississippi                              | <i>q</i>         | quaderni, quarterly                     |
| <i>Mitt</i>        | Mitteilung, Mitteilungen                 | <i>quel</i>      | quellen                                 |
| <i>Mo</i>          | Missouri                                 |                  |   |
| <i>mod</i>         | modern, moderna, moderne                 | <i>r</i>         | review, revista, revue, rivista         |
| <i>mond</i>        | mondiale                                 | <i>rass</i>      | rassegna                                |
| <i>Mont</i>        | Montana                                  | <i>Rdsch</i>     | Rundschau                               |
| <i>monum</i>       | monumenta                                | <i>rec</i>       | record                                  |
| <i>movim</i>       | movimento                                | <i>rech</i>      | recherches                              |
| <i>mt</i>          | mountain                                 | <i>regist</i>    | register                                |
| <i>mus</i>         | museum                                   | <i>relig</i>     | religion, religiöse, religious          |
|                    |  | <i>rend</i>      | rendiconti                              |
| <i>n</i>           | north, northern                          | <i>rep</i>       | report, reporter                        |
| <i>nac</i>         | nacional                                 | <i>res</i>       | research                                |
| <i>nass</i>        | nassauische                              | <i>rev</i>       | revolution                              |
| <i>nat</i>         | national                                 | <i>rhein</i>     | rheinisch                               |
| <i>nationalok</i>  | nationalökonomie, nationalökonomisk      | <i>R.I.</i>      | Rhode Island                            |
| <i>naz</i>         | nazionale                                | <i>ric</i>       | ricerche                                |
| <i>N.C.</i>        | North Carolina                           | <i>roc</i>       | roczniki                                |
| <i>N.D.</i>        | North Dakota                             | <i>roman</i>     | romanische                              |
| <i>ne</i>          | northeast                                | <i>roy</i>       | royal                                   |
| <i>Nebr</i>        | Nebraska                                 |                  |   |
| <i>neutest</i>     | neutestamentliche                        | <i>s</i>         | south, southern                         |
| <i>Nev</i>         | Nevada                                   | <i>S.C.</i>      | South Carolina                          |
| <i>newslett</i>    | newsletter                               | <i>Scand</i>     | Scandinavia, Scandinavian               |
| <i>N.H.</i>        | New Hampshire                            | <i>schles</i>    | schlesisch                              |
| <i>niedersächs</i> | niedersächsisch                          | <i>Schr</i>      | Schrift                                 |
| <i>N.J.</i>        | New Jersey                               | <i>schweiz</i>   | schweizerisch                           |
| <i>N.M.</i>        | New Mexico                               | <i>sci</i>       | science, scientific, scientist, scienze |
| <i>no</i>          | number                                   | <i>S.D.</i>      | South Dakota                            |
| <i>nos</i>         | numbers                                  | <i>se</i>        | southeast                               |
| <i>Nor</i>         | Norway                                   | <i>sec</i>       | sectio, section                         |
| <i>nord</i>        | nordisk                                  | <i>ser</i>       | série, series                           |
| <i>norm</i>        | normale                                  | <i>slaw</i>      | slawistik                               |
| <i>numis</i>       | numismatic, numismatique                 | <i>soc</i>       | social, society                         |
| <i>nw</i>          | northwest                                | <i>sociog</i>    | sociographiques                         |
| <i>N.Y.</i>        | New York                                 | <i>sociol</i>    | sociologia, sociological, sociology     |
|                    |  | <i>solothurn</i> | solothurnische                          |
| <i>oeslerr</i>     | oesterreichisch                          | <i>sozial</i>    | sozialistischen                         |
| <i>øk</i>          | økonomie                                 | <i>Soziol</i>    | Soziologie                              |
| <i>Okla</i>        | Oklahoma                                 | <i>spæl</i>      | spølecznuch                             |
| <i>Ore</i>         | Oregon                                   | <i>stat</i>      | statistical, statistics, Statistik      |
| <i>organ</i>       | organization                             | <i>stift</i>     | stiftung                                |
| <i>orient</i>      | oriental, orientalia, orientalistyczny   | <i>stor</i>      | storia, storici, storico                |
| <i>österr</i>      | österreichisch                           | <i>stud</i>      | studi, Studien, studies, studium        |
| <i>Osth</i>        | Osthefte                                 | <i>sup</i>       | superiore                               |
|                    |  | <i>suppl</i>     | supplement                              |
| <i>Pa</i>          | Pennsylvania                             | <i>sw</i>        | southwest                               |
| <i>Pac</i>         | Pacific                                  | <i>Sw</i>        | Sweden                                  |
| <i>pädagog</i>     | pädagogik, pädagogisch                   | <i>Swed</i>      | Swedish                                 |
| <i>paedagog</i>    | paedagogica                              | <i>symp</i>      | symposium                               |
| <i>pap</i>         | papers                                   |                  |   |
| <i>papyrol</i>     | papyrologie                              | <i>tech</i>      | technisch                               |
| <i>parl</i>        | parlementaire, parliament                | <i>technol</i>   | technology                              |
| <i>pfälz</i>       | pfälzische                               | <i>Tenn</i>      | Tennessee                               |
| <i>phil</i>        | philosophical, philosophique, philosophy | <i>test</i>      | testament, testamentum                  |
| <i>philol</i>      | philology                                | <i>Tex</i>       | Texas                                   |
| <i>photo</i>       | photograph                               | <i>theol</i>     | theological, theology                   |
| <i>pol</i>         | political, político, politics, Politik,  | <i>tids</i>      | tidskrift, tidskrift                    |
|                    | politique, politische                    | <i>tijd</i>      | tijdschrift                             |
| <i>port</i>        | portuguesa, portuguese                   | <i>tr</i>        | translated, translation, translator     |
| <i>pres</i>        | president, presidential                  | <i>trans</i>     | transactions                            |
| <i>Presb</i>       | Presbyterian                             | <i>trav</i>      | travail, travaux                        |
| <i>preuss</i>      | preussisch                               |                  |   |
| <i>probl</i>       | problems                                 | <i>u</i>         | und                                     |
| <i>proc</i>        | proceedings                              | <i>U</i>         | University                              |

|               |                              |                  |                                   |
|---------------|------------------------------|------------------|-----------------------------------|
| <i>Unter</i>  | Unterricht                   | <i>westf</i>     | westfälisch                       |
| <i>Va</i>     | Virginia                     | <i>wirtsch</i>   | Wirtschaft, wirtschaftlich        |
| <i>vaterl</i> | vaterlandisch                | <i>Wis</i>       | Wisconsin                         |
| <i>ver</i>    | Verein, vereinigung, Vereins | <i>wiss</i>      | Wissenschaft, wissenschaftlich    |
| <i>Verh</i>   | Verhandlungen                | <i>WMQ</i>       | <i>William and Mary Quarterly</i> |
| <i>Veröff</i> | Veröffentlichungen           | <i>württemb</i>  | württembergisch                   |
| <i>vesn</i>   | vesnik                       | <i>W. Va.</i>    | West Virginia                     |
| <i>vest</i>   | vestnik                      | <i>Wyo</i>       | Wyoming                           |
| <i>volksk</i> | volkskunde                   |                  |                                   |
| <i>vopr</i>   | voprosy                      | <i>yrbk</i>      | yearbook                          |
| <i>vrem</i>   | vremennuk                    |                  |                                   |
| <i>Vt</i>     | Vermont                      | <i>Z</i>         | Zeitschrift, Zeitschriften        |
|               |                              | <i>Zeitgesch</i> | Zeitgeschichte                    |
| <i>w</i>      | west, western                | <i>zgodov</i>    | zgodovinski                       |
| <i>Wash</i>   | Washington                   | <i>zhurn</i>     | zhurnal                           |

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# The American Historical Association

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Founded in 1884. Chartered by Congress in 1889

Office: 400 A Street, S.E., Washington, D.C. 20003

President: Thomas C. Cochran, *University of Pennsylvania*

Vice-President: Lynn White, jr., *University of California, Los Angeles*

Treasurer: Elmer Louis Kayser, *George Washington University*

Executive Secretary: Paul L. Ward

Assistant Executive Secretary: John J. Rumbarger

**MEMBERSHIP:** Persons interested in historical studies, whether professionally or otherwise, are invited to membership. The present membership is about 18,000. Members elect the officers by ballot.

**MEETINGS:** The Association's annual meeting takes place on December 28-30. The meeting in 1972 will be held in New Orleans. Many professional historical groups meet within or jointly with the Association at this time. The Pacific Coast Branch holds separate meetings on the Pacific Coast and publishes the *Pacific Historical Review*.

**PUBLICATIONS AND SERVICES:** The *American Historical Review* is published five times a year and sent to all members. It is available by subscription to institutions. The Association also publishes its *Annual Report*, the *AHA Newsletter*, a variety of pamphlets on historical subjects, and bibliographical and other volumes. To promote history and assist historians, the Association offers many other services. It also maintains close relations with international, specialized, state, and local historical societies through conferences and correspondence.

**PRIZES:** The *Herbert B. Adams Prize* of \$300 awarded annually for a first book in the field of European history. The *Troyer Steele Anderson Prize* awarded every ten years to the person whom the Council of the Association considers to have made the most outstanding contribution to the advancement of the purposes of the Association during the preceding ten years (next award, 1980). The *George Louis Beer Prize* of \$300 awarded annually for a first book

on any phase of European international history since 1895. The *Albert J. Beveridge Award* of \$5,000 given annually for the best book on the history of the United States, Canada, or Latin America. The *Albert B. Corey Prize*, sponsored jointly by the AHA and the Canadian Historical Association, of \$1,000 awarded biennially for the best book on the history of Canadian-American relations or the history of both countries. The *John H. Dunning Prize* of \$300 awarded in the even-numbered years for a book on any subject relating to American history. The *John K. Fairbank Prize in East Asian History* of \$500 awarded in the odd-numbered years beginning in 1969. The *Clarence H. Haring Prize* of \$500 awarded every five years to that Latin American who has published the most outstanding book in Latin American history during the preceding five years (next award, 1976). The *Robert Livingston Schuyler Prize* of \$500 awarded every five years for the best work in modern British and Commonwealth history (next award, 1976). The *Watumull Prize* awarded in the even-numbered years for a work on the history of India originally published in the United States; beginning this year it carries a cash award of \$1,000.

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**CORRESPONDENCE:** Inquiries should be addressed to the Executive Secretary at 400 A Street, S.E., Washington, D.C. 20003.

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# The American Historical Review

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Founded in 1895

The *AHR* is sent to all members of the American Historical Association; information concerning membership will be found on the preceding page. The *AHR* is also available to institutions by subscription. There are two categories of subscription:

CLASS I: *American Historical Review* only, United States, Canada, and Mexico \$25.00, foreign \$27.00.

CLASS II: *American Historical Review*, the *AHA Newsletter*, and the program of the annual meeting of the Association, United States, Canada, and Mexico \$30.00, foreign \$32.00.

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Correspondence regarding contributions to the *American Historical Review* and books for review should be sent to the Managing Editor, 400 A Street, S.E., Washington, D.C. 20003. Unsolicited book reviews or review articles are not accepted; a statement concerning reviewing policy will be found in the issue for December 1970, vol. 75, pp. 1889-91. Preliminary inquiries concerning articles are not necessary, though authors may find them useful. Attention is urged, however, to a statement concerning the kinds of articles the *AHR* ordinarily will and will not publish; it appears in the issue for October 1970, vol. 75, pp. 1577-80. The entire text, including footnotes, of manuscripts submitted for publication must be prepared in double-spaced typescript, with generous margins to allow for copyediting. Footnotes should be numbered consecutively throughout and should appear in a separate section at the end of the text. The editors of the *AHR* are the final arbiters of length, grammar, usage, and the laws of libel; articles will be edited to conform to *AHR* style in matters of punctuation, capitalization, and the like. The editors may suggest other changes in the interests of clarity and economy of expression, but such changes are never made without consultation with authors. There is no official style sheet for the *American Historical Review*, but a convenient general guide is *A Manual of Style*, published by the University of Chicago Press.



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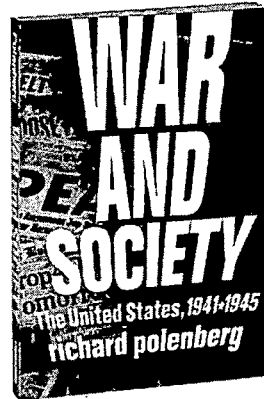
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## WAR AND SOCIETY

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Richard Polenberg, *Cornell University*

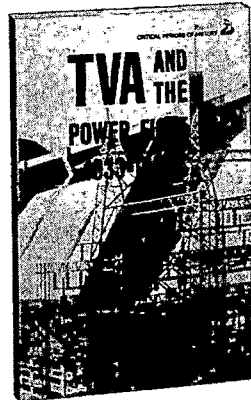
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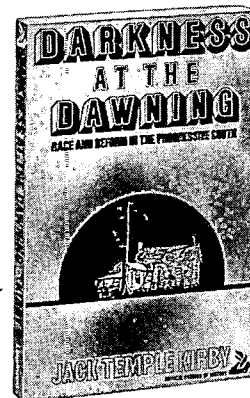
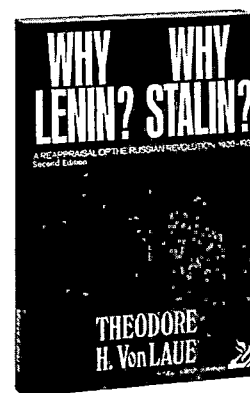
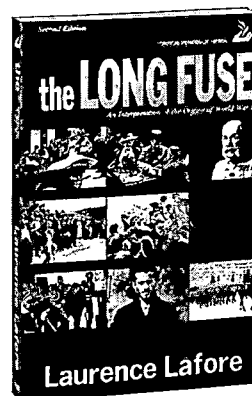
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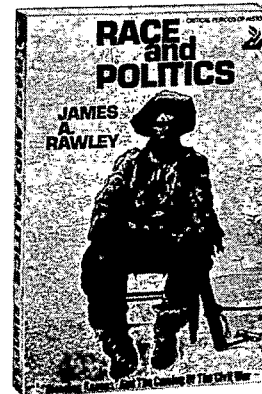
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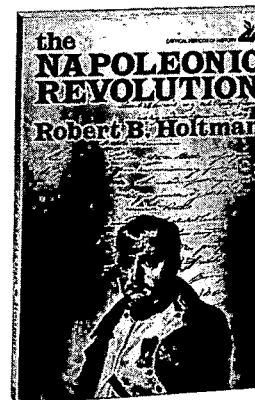
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Spectrum

on  
American  
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# For Fall/ 1972

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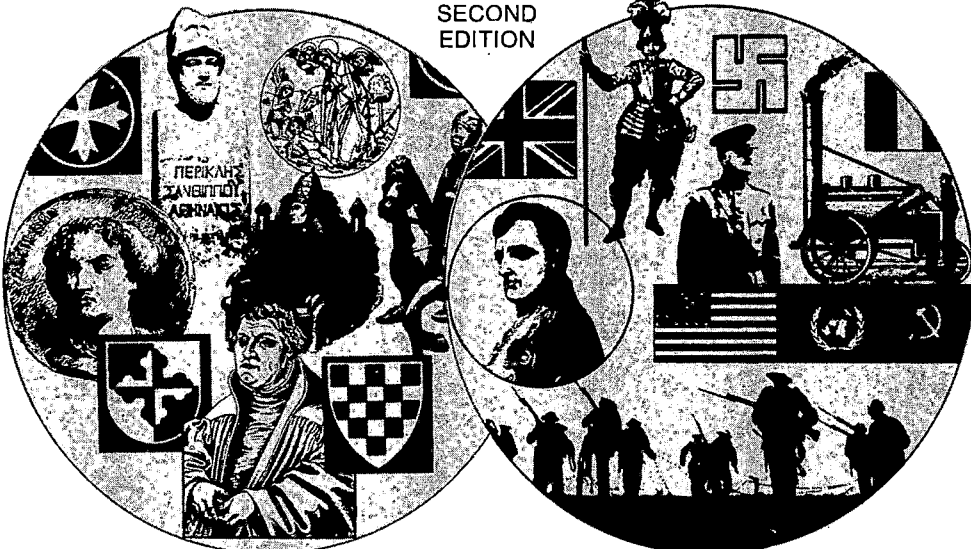
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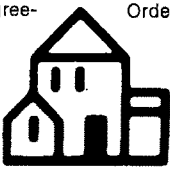
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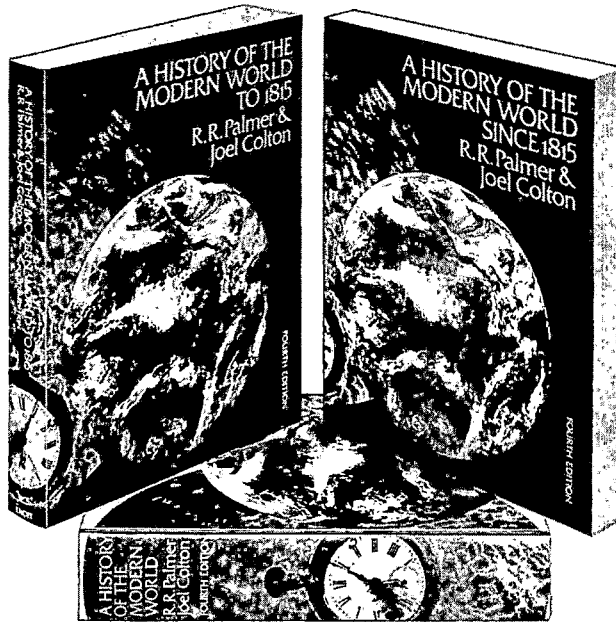
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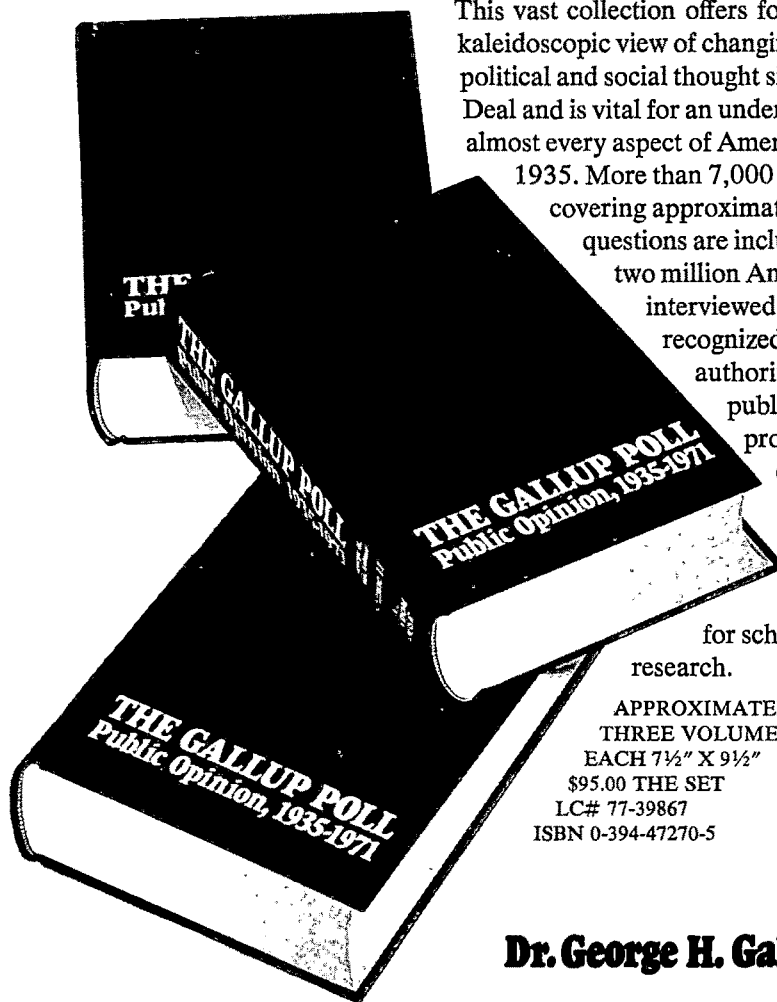
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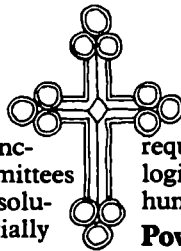
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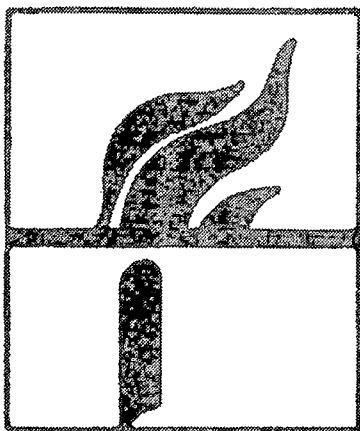
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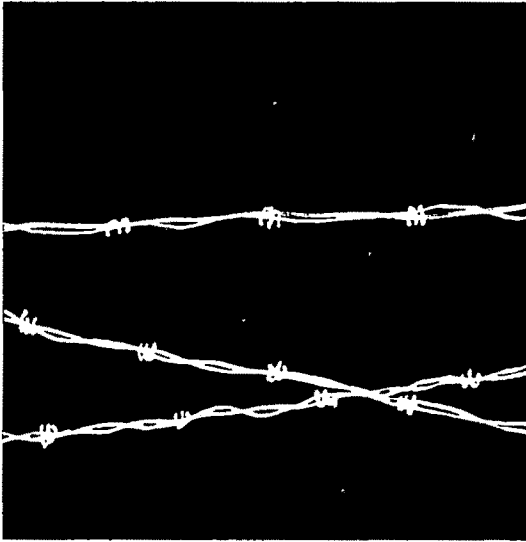
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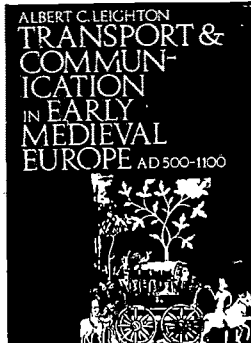
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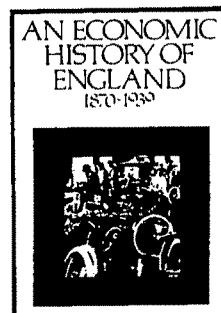
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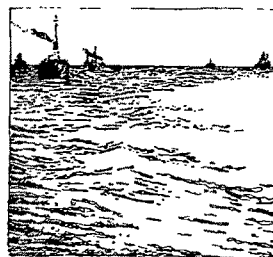
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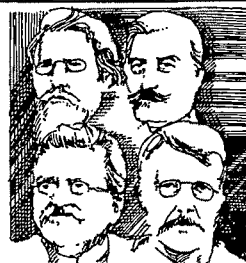
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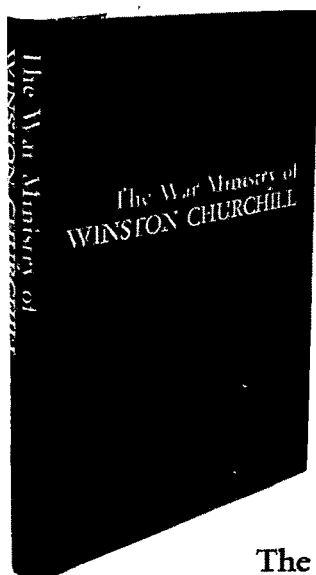
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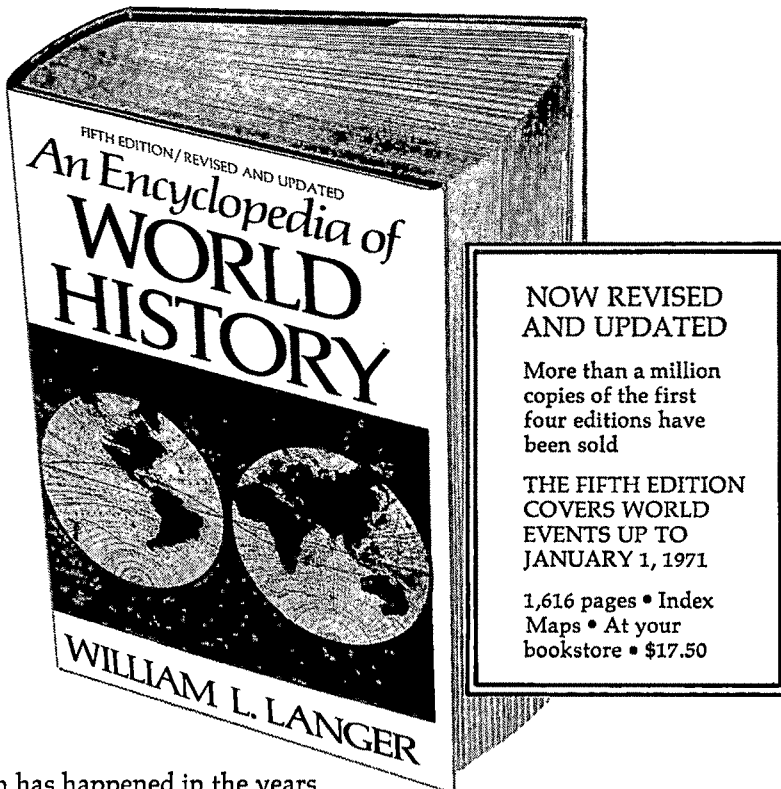
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At a recent meeting the Council of the Association voted to raise annual regular dues to \$25 for full professors and nonacademic members as a step toward reducing the deficit for the current fiscal year. The Council agreed that the increase should be put into effect starting with renewal notices sent out after the end of January 1973.

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# The American Historical Review

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## Martyrs, Myths, and the Massacre: The Background of St. Bartholomew

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DONALD R. KELLEY

HOW, FROM A DISTANCE of four hundred years, can we obtain a clear view of the Massacre of St. Bartholomew? The refractory powers of time always present difficulties, but in this case our vision is further distorted by a screen of false and conflicting evidence and by an endless stream of partisan debate. Perhaps the answer is that we should discard altogether the idea of describing some objective set of circumstances independent of ideological presuppositions and the passions aroused in witnesses and interpreters. Perhaps we should try rather to restore the event to its various contexts, conceptual as well as historical, and from a point of view that accommodates political and religious consciousness as well as social reality, that recognizes the mythical as well as the historical dimension. For it is upon some such symbolic level that the historical significance of events is to be found.

Over the past four centuries the Massacre of St. Bartholomew has presented many faces. It has been seen as a sensational explosion of violence fired by a half-century of mounting religious hatred; as a tragedy so shocking, according to one contemporary witness, that posterity would never believe it; as a turning point in a great world conflict; as a storm center of religious polemic and a seedbed of political theories; as a legend of gigantic proportions promoted by publicists and sanctioned by men of letters; as a puzzle and topic of debate for generations of historians; as an occasion for Catholic masses, Protestant lamentations, and historical conferences.<sup>1</sup> It was all these and more. It was also an archetypal occurrence that transcended its

This paper is an offshoot of two projects, a biography, *François Hotman, A Revolutionary's Ordeal*, forthcoming from Princeton University Press, and a study in progress of sixteenth-century propaganda. A shorter version of this paper was read at a meeting of the Newberry Library Renaissance Conference in May 1972 devoted to "The Massacre of St. Bartholomew in Historical Perspective."

<sup>1</sup>The most important source material is the anonymous pamphlet literature, which is fairly exhaustively listed in the following publications: Robert Lindsay and John Neu, *French Political Pamphlets, 1547-1648* (Madison, 1969); the *Catalogue de l'histoire de France*, 1 (Paris, 1845), which is a listing by shelf mark of the Bibliothèque Nationale (BN), principally the range Lb.<sup>33</sup> to Lb.<sup>38</sup>; the catalog of the Bibliothèque de l'Arsenal, unfortunately unpublished, in nine MS volumes, 8°H. 12868; and F.-A. Isambert, ed., *Recueil général des anciennes lois françaises*, 12 (Paris, 1822-33), numbered consecutively.



historical context—transcended it not only in the direction of the future (by endless debates over premeditation, guilt, and consequences) but also in the direction of the past (by seeming to symbolize, summarize, and confirm long-standing fears and anticipations and indeed to repeat earlier misfortunes on a grander scale). It was, in other words, an almost generic human experience that came as no surprise in the event, that followed a familiar pattern in its course, and that would be relived in various ways afterwards.

In order to suggest the basis and dimensions of this historical epiphenomenon, we must look beyond the conventional narrative and diplomatic sources that help to show (in Ranke's view) "what really happened." One of the most convenient as well as fashionable procedures is to choose some analyzable model. This seems to be all the more appropriate since the model in this instance does not have to be imported from any of the more structured social disciplines. On the contrary, it is suggested, indeed imposed upon us, by the historical context of the sixteenth-century wars of religion. Here is a clear case of history repeating itself, or at least of men seeing it as a repetition. For the witnesses, participants, and interpreters of the events of late summer 1572 knew what the phenomenon was practically before it happened: it was not a "tumult" or a "disorder" or the suppression of a "conspiracy" as various observers supposed; it was a massacre, by no means unexpected and not even the first in that generation. And they knew what part they might ultimately have to play: it was that highly stylized and stereotyped role called "martyrdom," the most exalted and yet in some ways the simplest form of sainthood. The fundamental psychological model for this phenomenon, then (again with a nominal bow to intellectual fashion), was what can only be called the martyr complex.<sup>2</sup>

The remaining problem is where to find reflections of this model, or in other words, how to gain access to this aspect of the Protestant conscience. The most obvious and direct source would seem to be that most human and down-to-earth variety of Protestant historiography, the martyrology. The tradition of latter-day martyrs was inherent in Protestant self-consciousness from the beginning—Luther's stand at Worms in 1521, so reminiscent of Hus's a century before, was potentially that of a martyr—but not for another generation did this tradition take a consciously literary form. The first fruits came within a few years and showed a pronounced family resemblance. Most significant were Jean Crespin's *The History of the Martyrs* and the first Latin version of John Foxe's *Acts and Monuments*, both appearing in 1554, in Geneva and Strasbourg respectively. Ludwig Rabe's *Stories of God's*

<sup>2</sup> A convenient bibliography on the question of martyrdom may be found in W. H. C. Frend, *Martyrdom and Persecution in the Early Church* (New York, 1967). In all the massive literature I find no useful studies from a psychohistorical or sociohistorical point of view. General Catholic treatments—of which the best is perhaps Henri Leclercq's in Fernand Cabrol and Henri Leclercq, eds., *Dictionnaire d'Archéologie chrétienne et de liturgie* (Paris, 1924–53), vol. 10, pt. 2—tend to be so rigidly legalistic and orthodox that Protestant experiences do not even come into the discussion.

*Chosen Witnesses and Martyrs*, published in Strasbourg in 1552, antedated these two but was limited to martyrs in antiquity until later, derivative editions. Also related were Heinrich Pantaleone's *History of the Martyrs*, Johann Sleidan's great history of the Reformation, published in Strasbourg in 1555 and soon translated into German, French, and English, and Matthias Flaccius Illyricus's *Catalog of the Witnesses of Truth*, published in Basel in 1556.<sup>3</sup>

Though independently conceived, these works were all products of the international Protestant community and had a collective, to some extent cooperative, character. The links between Geneva and Strasbourg, the two leading Calvinist centers, were particularly strong and were strengthened by the presence of the exiles from Marian England. Crespin was in close touch with Calvin's friend Sleidan, drew upon Sleidan's book, and in 1556 published a French translation of it in Geneva. For later versions of his own *History* Crespin drew also upon Foxe's concomitantly growing *Book of Martyrs*. Foxe himself had gone to Strasbourg in 1555 and then settled in Basel to continue his work, but he continued to receive materials from his friend Edmund Grindal, and no doubt indirectly from Sleidan. Foxe also made use of Flaccius Illyricus's *Catalog*, which was printed in 1556 by Foxe's own publisher, Johann Oporinus. In addition Foxe's work was continued by Pantaleone, who was also Sleidan's German translator. The relation of Rabe, a leading theologian in Strasbourg's Lutheran congregation, is not known, but in any case it seems clear that the first generation of martyrologists constituted something approaching a literary circle, a kind of Protestant pleiade of the exile circuit, which drew upon a common fund of experience, a common ideological commitment, a common historical perspective, and a common reliance upon what Foxe called the "miracle" of printing.<sup>4</sup>

In many respects the groundbreaking work was Crespin's, and for France, certainly, it provided the model. Crespin was himself an exile from the Netherlands and, along with his friend François Baudouin, had barely escaped from his native Artois with his skin. That was in 1545. Afterwards Crespin settled in Geneva, set up his printing press, and launched into his life's work. The *History of Martyrs* was based on a wide range of printed and unprinted sources and on several other independent works, including those of Foxe, Antoine de la Roche Chandieu, and François Hotman, the first historian of the Massacre of St. Bartholomew. Crespin also

<sup>3</sup> Ludwig Rabe, *Der heiligen aus erwählten Gottes Zeugen Bekennen und Martyren* (Strasbourg, 1552), with a treatment of the modern period in the expanded edition (Strasbourg, 1571); Jean Crespin, *Le Livre des martyrs* ([Geneva], 1554); John Foxe, *Commentarii rerum in ecclesia gestarum* (Strasbourg, 1554), dedicated to Christoph von Württemberg, Aug. 31, 1554; Heinrich Pantaleone, *Martyrum historia* (Basel, 1563); Johann Sleidan, *De Statu religionis et reipublicae, Carolo Quinto, Caesare, Commentarii* (Strasbourg, Sept. 1556); Matthias Flaccius Illyricus, *Catalogus testium veritatis* (Basel, 1556).

<sup>4</sup> For some of these complex interrelations, see G. Moreau, "Contribution à l'histoire du livre des martyrs," *Bulletin de la Société de l'histoire du protestantisme français*, 103 (1957): 173-99.

drew on the *Ecclesiastical History of the Reformed Churches of France*, long attributed to Theodore Beza. The *History of Martyrs* appeared in successively augmented and altered editions between 1554 and 1570 (the last published by Crespin) and thereafter was continued by Simon Goulart, the prolific polemicist who issued the most comprehensive edition in 1619, when he had succeeded Beza as head of the Genevan church. This book is the centerpiece, as far as France is concerned, of the modern martyrological canon.<sup>5</sup>

Perhaps the most fundamental theme of this whole genre was, in Crespin's words, "the conformity of the modern history of the martyrs with that of antiquity." This parallel was perhaps what Lefèvre d'Étaples had in mind when he began to compile his *Agonies of the Martyrs*, as did Rabe, who referred explicitly to the "proto-martyrs" of antiquity. "The memory of the first persecutions," as Chandieu put it, "is a school that teaches how to remain true to one's calling." Indeed the purpose of collecting these biographical accounts included all of the basic ingredients for the humanist prescription for history. They offered "consolation," as Crespin wrote in his first preface; they constituted a treasury of *exempla* for imitation and a kind of moral and anagogical "mirror" for Christians; and they were commemorative, preserving for posterity the "deeds and writings" of exemplary men of faith.<sup>6</sup>

Yet there is no doubt that these humanist commonplaces were transformed by their conscription into the service of militant Protestantism. The biographical form of martyrology resembled less the *de viris illustribus* of classical tradition than the *vitae sanctorum* of the medieval Church. In each biography Crespin wrote, it was his intention to describe the doctrine as well as the life of the martyr and above all his "happy ending"; and to

<sup>5</sup> Besides the invaluable edition by Daniel Benoit of Crespin and Simon Goulart, *Histoire des martyrs* (Toulouse, 1885-89), see Arthur Piaget and Gabriel Berthoud, *Notes sur le livre des martyrs de Jean Crespin* (Neuchâtel, 1930); C.-L. Frossard, *Le Livre des martyrs de Jean Crespin* (Paris, 1880); and more generally, Ferdinand Vander Haeghen et al., *Bibliographie des martyrologies protestants néerlandais*, 2 (La Haye, 1890); also the article on Crespin in Eugène Haag and Emile Haag, eds., *La France Protestante* (2d ed.; Paris, 1877-88); the notes to Calvin's correspondence in Calvin, *Opera quae supersunt omnia*, ed. J. W. Baum, Eduard Cunitz, et al. (Strasbourg, 1863-1900), vols. 38-39; and J.-F. Gilmont, "Une édition inconnue du martyrologe de Jean Crespin," *Bibliothèque d'Humanisme et Renaissance*, 30 (1969): 363-71. The latter article and that by Moreau, "Contribution à l'histoire du livre des martyrs" (p. 174), refer to the work of E. E. Halkin and his students, which is important though devoted mostly to the martyrs of Belgium and the Netherlands. On Rabe's book, see Robert Foncke, *Duitse Vlugschriften van de Tijd over het Proces en de Terechstelling van de Protestanten Frans en Nikolaas Thys te Mechelen* (Antwerp, 1937), 60-65; and William Haller, *Foxe's Book of Martyrs and the Elect Nation* (London, 1963), 55-73. The other related works are Antoine de la Roche Chandieu, *Histoire des persecutions et martyrs de l'église de Paris depuis l'an 1557* (Lyons, 1563), BN, Rés. Ln.<sup>25</sup>, 91; *Histoire ecclésiastique des églises réformées au Royaume de France*, ed. J. W. Baum and Eduard Cunitz (Paris, 1883); [François Hotman], *De Furoribus Gallicis* ("Edinburgh" [probably Basel], 1573); and [Hotman], *Gasparis Colinii Castelonii, magni quondam Franciae amiraliis vita* (hereafter *Vita Colinii*) (n.p., 1575).

<sup>6</sup> Crespin, *Histoire des martyrs*, vol. 1, prefaces of 1570 and 1554, *passim*; [Lefèvre d'Étaples], *Agones martyrium mensi ianuarii. Libro primo* (Paris, 1529) (no more published); Rabe, *Heiligen aus erwählten Gottes Zeugen Bekennen und Martyren* (1571), vol. 2; Chandieu, *Histoire des persecutions*, xiii.

suit this purpose Crespin did not hesitate to stretch the already flexible standards of sixteenth-century editorship, improving in various ways upon the texts of even original documents. Chandieu took pride in declaring his "fidelity" to truth and especially to his sources, but his partisanship was still more flagrant. The purpose of his book was both to afford "profit" to his brothers and to demonstrate the justice of his "Cause" to the "poor ignorant ones" outside of it. The book was quite literally a call to arms. "We are not in this world to rest," he declared, "but rather to fight."<sup>7</sup>

The most direct source of inspiration for these martyrologies was clearly the Protestant view of ecclesiastical history, as variously expressed by Luther, Melanchthon, and Calvin. "From all authors and histories," declared Flaccius Illyricus, "it is evident that our church . . . is truly ancient and takes its origin from the time of Christ and the apostles."<sup>8</sup> In the subsequent life of this true church the persecutions of the faithful constituted a major source of continuity, which according to Crespin paralleled the progress of Grace on earth and indeed represented its carnal counterpart. "The blood of martyrs is the seed of the church" was the invariably quoted motto of Tertullian, and its life-giving force was continuous over the centuries. Moreover, like Christian tradition in general and in sharp contrast to the national orientation of pagan historiography, the history of martyrs was truly universal in scope and included, as Crespin remarked, "all conditions, ages, sexes, and nations."<sup>9</sup> It differed from medieval historiography, too, especially from the saints' lives with which it had undeniable resemblances, in that it was concerned not with the idolatrous relics but with the spiritual legacy of the faithful—with their "words and deeds." Like the view of ecclesiastical tradition held by Melanchthon and Flaccius Illyricus, the view of the Crespin-Goulart martyrology was not merely "human" but doctrinal or confessional.

Between the first, archetypal age of Christian martyrs and the moderns came a time that seemed darker to Crespin than it did to many of the most critical of humanists (which may serve as a reminder, though surely not a revelation, that the myth of the "Dark Ages" was as much a creation of the Reformation as it was of the Renaissance). After the primitive church came the papal monarchy—"la monarchie papistique" is Crespin's phrase—and then, at a still lower level, a third age ridden with Scholasticism, canon law, relic worship, and other forms of idolatry.<sup>10</sup> Throughout this period there was a tradition of "pure religion," but it was tenuous and took the form only of scattered "witnesses to the truth"—"precursors" is the word modern

<sup>7</sup> Chandieu, *Histoire des persecutions*, xxviii.

<sup>8</sup> Flaccius Illyricus, *Catalogus testium veritatis*, preface.

<sup>9</sup> Crespin, *Histoire des martyrs*, 1:1; Chandieu, *Histoire des persecutions*, lxii. See also Hans Campenhausen, *Die Idee der Martyrium in der alten Kirche* (Göttingen, 1964), and the references there to Augustine, Luther, and Calvin.

<sup>10</sup> Crespin, *Histoire des martyrs*, 1: 41; see also Heinrich Bullinger, *Origines errorum* (Zurich, 1549).

historians prefer for this still no less mythical phenomenon. Finally came the revival of true doctrine begun by Luther, which brought with it, according to Crespin, a renaissance of martyrdom as well.

SO MUCH FOR MARTYROLOGY as a model. The original, martyrdom itself, is more difficult to define. A legal concept, a value judgment, a psychological condition, a social role, a weapon of propaganda—again it was all these, and more. The idea, if not the terminology, long antedated Christianity. As early as the fifth century, for example, Athenians killed in war were assured of deification; and this notion of dying in battle for a just cause has always been associated with that of martyrdom.<sup>11</sup> But it was Christianity that adopted the Greek term *martys* to this general behavior pattern. Although the original signification was lost in the Latin West, Protestant martyrologists quite consciously, in the spirit of Biblical humanism, restored it and identified the martyrly condition with one “testifying” to the faith.<sup>12</sup> It was not the fact of death, in other words, but the inner attitude that admitted one to the pantheon of martyrs and so, though theologically this could not be explicit, to the certainty of salvation as well as earthly immortality. The vital importance of the act of testifying in early Protestantism is further highlighted by the counterconcept—“Nicodemitism” was the term coined by Calvin in 1543—referring to the concealment of one’s faith, one of the most heinous of all sins.

Martyrdom was a highly conventional as well as highly painful process: it was a form of mimesis—*imitatio Christi* with a vengeance. And to follow Christ, “captain of the martyrs,” as Crespin put it, entailed a heavy weight of ritual, rhetoric, etiquette, and symbolism, as reflected in interrogations, confessions of faith, execution scenes, crowd reactions, and contemporary graphic representations. The stereotyped character of the role is clearly evident in the martyrologies—not only in the “ten marks of the martyr,” which Crespin established for purposes of identification and as the Protestant equivalent of canonization, but also in the unarticulated categories, including the status of the accused and the types of punishment, which modern scholars have extracted and applied in an almost quantitative fashion.<sup>13</sup> These categories were confirmed, and in some cases created, by the more or less predictable reactions of established authority, which in France

<sup>11</sup> Ernst H. Kantorowicz, “*Pro Patria, Mori* in Medieval Political Thought,” *AHR*, 56 (1950-51): 472; included also in his *Selected Studies* (New York, 1965), 308.

<sup>12</sup> See H. A. M. Hoppenbrouwers, *Recherches sur la terminologie du martyre de Tertullien à Lactance* (Nijmegen, 1961); and Carlo Ginzburg, *Il Nicodemismo* (Turin, 1970).

<sup>13</sup> Crespin, *Histoire des martyrs*, preface of 1570. A modern example is Piaget and Berthoud, *Notes sur le livre des martyrs*, drawing upon Vander Haeghen, *Bibliographie des martyrologies*. Such attempts deserve to be followed up by more statistically grounded studies, not only along the lines suggested by Geoffrey Nuttall, “The English Martyrs, 1535-1680,” *Journal of Ecclesiastical History*, 22 (1971): 191-97, but also, and indeed at the same time, along those of Lacey Baldwin Smith, “English Treason Trials and Confessions in the Sixteenth Century,” *Journal of the History of Ideas*, 15 (1954): 471-98.

was determined to uproot all heresy. The result was to intensify the impression of Protestants that they were recapitulating the experiences of early Christians—confronting the same style of interrogation, the same inseparable charges of blasphemy and sedition, and the same sorts of repression and punishments. It may be added that they also anticipated the same kind of vengeance, that is, the decline and fall of the persecuting state; for such was the judgment often expressed by Protestants about the fate of France in the later sixteenth century.<sup>14</sup>

It is not going too far to suggest that the coherence of the modern tradition of martyrs was created as much by external pressures as by the sharing of a common ideology and a common "Cause."<sup>15</sup> Catholic attempts to discredit individual martyrs only provided further publicity. Stories about last-minute recantations were always being circulated, remarked Erasmus, rejecting one such rumor about Louis Berquin, the first notable French martyr (who indeed, against Erasmus' advice, had chosen to stand upon his conscience).<sup>16</sup> Catholics were always as ready to attribute a "Virgin, intercede for me" to a man about to be killed as Protestants were to find a martyrly defiance; what counted most, however, was the fact of execution. The Catholic solution was to create a kind of countermythology about heretics and, ultimately, an "antimartyrological" tradition to undo the work of Crespin and his fellow authors.<sup>17</sup> In ideological wars, it seems, demonology has often been the response to hagiography.

This dialogue was also pursued on the official level. From the mid-1520s Protestants were plagued by a wide range of repressive legislation that constituted a kind of mirror image of their own propaganda and was an almost continuous effort to regulate their behavior on every level. The legislation was not always consistent (alternating between prescribing banishment and forbidding emigration, for example, and between controlling the printing press and abolishing it altogether), but its tone was unmistakable. Except for intermittent periods of crisis or compromise, it reflected an almost totalitarian, off-with-their-heads attitude toward heresy and disobedience that was hardly less intense than the fanaticism of the Huguenots themselves. The royal ordinances are filled with repetitive orders banning weapons, expelling vagabonds and such dangerous elements, forbidding Protestants the right to inheritance, officeholding, and burial, and prescribing death for the printing, selling, or even possession of heretical or seditious literature and for the convening of "illicit assemblies." Again and again government policy was declared to be "the extirpation and extermination" of

<sup>14</sup> See, for example, Chandieu, *Histoire des persecutions*, lxxv.

<sup>15</sup> "La CAUSE," i.e. of the Huguenots, became a central theme of debate between Protestants and Catholics immediately following St. Bartholomew; see especially Pierre Charpentier, *Epistola ad Franciscum Portum* (n.p., 1572); Franciscus Portus, *Responsio* (n.p., 1573); and Jean de Montluc, *Oratio* . . . (Paris, 1573).

<sup>16</sup> Erasmus, *Opus epistolarum Des. Erasmi Roterdami*, ed. P. S. Allen and H. M. Allen, 8 (Oxford, 1934), no. 2188.

<sup>17</sup> Jacques Severt, *L'Anti-Martyrologie* . . . (Lyons, 1622).

heresy.<sup>18</sup> The most spectacular expression of official disfavor, of course, was the unending series of executions staged as public exhibits at strategic spots, especially market places like the Place de Grève and the Place Maubert in the university section of Paris.<sup>19</sup> It was this campaign, more than anything else, that gave French Protestantism its persecution complex and Protestant propaganda its paranoid style.

From the time that the winds of Lutheran doctrine first reached France, bracing to some, to others bringing an odor of heresy and sedition, the production of martyrs became a familiar feature of religious opposition. In many cases, of course, the pattern of early Christianity was consciously and pridefully followed. From the execution of the self-styled hermit Jean Vallière on August 8, 1523, the honor roll of this special echelon of the elect grew and was carefully preserved by religious chroniclers and martyrologists. From 1534—the notorious “year of the placards,” when heretical posters were distributed in the streets of Paris and even affixed to the king’s chamber door in the castle of Amboise—the official reaction became more intense, and within a few months the list of martyrs increased considerably. It was at this time, Crespin reported, that the practice of cutting off the tongues of heretics before execution was initiated, although this symbolic act was notably unsuccessful in silencing French Protestantism in general.<sup>20</sup>

It was the “miracle” of printing that gave Protestantism its voice, but this same miracle also produced one most unattractive offshoot—the institution of preventive censorship. Book burning was by no means a Catholic monopoly, as Luther’s sensational act of throwing the corpus of canon law into the fire demonstrates, but it was specifically Protestant literature that furnished most of the fuel at the beginning. Bearing witness in the embarrassingly public form of print often entailed equally drastic retaliation by established authority; and condemning and destroying books may well be regarded as one of the preliminaries of martyrdom itself, since more than one printer and propagandist followed his books into the flames. So it was with Berquin, so it would be with Étienne Dolet (that unwilling “martyr of the Renaissance” who did in fact recant),<sup>21</sup> and so it would have been with Calvin and Beza if they had returned to France.

One crucial factor in the ideological polarization of society was the alienation of the younger generation. A year before the affair of the placards there was an indication of this alienation in an edict that sought to bring greater discipline into the university of Paris. Specifically the order banned “the impudent books of the heretics” and called for interrogation of younger students if such books were found in their possession. The edict

<sup>18</sup> Isambert, *Recueil*, vol. 12: no. 128 (June 10, 1525), no. 211 (Jan. 29, 1534), no. 367 (July 23, 1543), no. 382 (July 24, 1557).

<sup>19</sup> See John Viénot, *Promenades à travers le Paris des martyrs, 1523–59* (Paris, 1913).

<sup>20</sup> Crespin, *Histoire des martyrs*, 1: 297.

<sup>21</sup> R. C. Christie, *Etienne Dolet, The Martyr of the Renaissance: 1508–1546* (London, 1899).

also frowned upon long whiskers (*prolixa barba*) worn by masters, and it was later derided by Beza, a student in these years, as the "edict of the beards."<sup>22</sup> In the course of the reaction to the placards (in which university persons were deeply implicated), Francis I issued a special warning to the university community about the danger of heresy. "I pray that you . . . be especially solicitous of the youth," he told the faculty in the spring of 1535, "and see that they are well instructed and indoctrinated [*indoctriner*] so that they do not fall into the evil and forbidden opinions."<sup>23</sup>

This incipient conflict of generations affected the family as well as education. One striking but not unusual example is that of François Hotman, who was converted to the "new opinions" at precisely the time that his father was taking his place on that special tribunal of the Parlement of Paris, the *Chambre ardente*, which took over the official campaign of suppression in 1547. The conflict was so intense that the next spring the younger Hotman made his decision—after a crisis very much like that of his friend Beza a few months later—to break ties with his home and go into exile; and he made quite clear the significance of the martyr complex in his choice. "My father ended his career by oppressing more than a thousand martyrs," he later told Melanchthon. "As long as I was with him he tried to keep me forcibly from impiety, but God kept me for His church, and here I intend to spend the rest of my life." He denounced the way of the Nicodemite and chose Calvin, quite literally, as his new "father." "He [Hotman] abandoned the hope of a fine inheritance," Calvin later told Heinrich Bullinger, "in order to fight for Christ."<sup>24</sup> A contemporary scholar has suggested that the martyr is a kind of religious adventurer,<sup>25</sup> and it was in some such spirit that Hotman, like many another young man in these years, committed this act of rebellion.

In the spring of 1549 Hotman left Geneva (in the company of Beza, who had just decided not to go into the printing business with Crespin) to take a position in the Calvinist academy of Lausanne. Even here Hotman was not out of reach of the royal campaign of persecution. Less than four years later five students from Lausanne, on their way to join congregations in southern France, were imprisoned by the Catholic authorities of Lyons and became a *cause* more *célèbre* than that of the placards. Despite Calvin's efforts and despite pleas from various Swiss cities these young men were condemned in 1553 and, one by one, were burned at the same stake. Crespin

<sup>22</sup> Beza to Maclou Popon, May 7, 1542, in Beza, *Correspondance*, ed. Fernand Aubert and Henri Meylan (Paris, 1960-), 1: 43.

<sup>23</sup> Francis I, quoted in C. E. Bulaeus, *Historia Universitatis Parisiensis* (Paris, 1673), 6: 252-53.

<sup>24</sup> François Hotman to Melanchthon, May 24, 1556, BN, MS, Collection Dupuy, vol. 797, fol. 212v. The record of the elder Hotman's activities has been published by Nathaniel Weiss, *La Chambre ardente* (Paris, 1889); the son's conversion and flight may be followed through his correspondence with Calvin, published in the *Opera*. See Calvin to Bullinger, Nov. 25, 1549, *Opera*, vol. 44, no. 1324.

<sup>25</sup> Michael Walzer, *The Revolution of the Saints* (London, 1965), 9. This study contains many interesting suggestions concerning the psychology of martyrdom.



publicized the fate of these latter-day "martyrs of Lyons" by printing their prison correspondence with Calvin in the first edition of his *History*, which appeared the following year (and which provided Sleidan with his account).<sup>26</sup> Whether these letters were authentic or assembled in some way by Crespin himself, they provided most effective Protestant propaganda and took their place in the martyrological tradition.

THE CREATION OF MARTYRS *seriatim* was alarming enough, especially when it was the result of institutional persecution, but far worse was the creation of martyrs *en masse*. According to a famous distinction of the Huguenot historian Agrippa d'Aubigne, massacre victims, though their names often went unrecorded, constituted a second type of martyr, and these were still more disturbing to the Protestant conscience.<sup>27</sup> The first sensational episode was the notorious persecution of the Waldensians in Mérimole, which Crespin called "as memorable as anything within the memory of man" and in fact thought worthy of a separate volume.<sup>28</sup> In 1545 a veritable campaign of extermination was waged in which twenty-two villages were destroyed and hundreds of persons killed; others were put to flight, and many went to Geneva, as was becoming increasingly common. This episode, together with the work of the *Chambre ardente*, haunted Protestants with the prospect of martyrdom or exile, which represented a kind of political martyrdom. Further incidents and the repressive legislation of the 1550s served to magnify such fears among the Huguenots, as they were beginning to be called, as well as to bring civil war closer.

Such fears played a part, too, in that complex set of uprisings referred to as the Conspiracy of Amboise, which such Protestants as Hotman, Beza, and Crespin looked upon as in effect the opening phase of the wars of religion. The conspiracy, brewing already in the fall of 1559 after the death of King Henry II, aimed at breaking the power of the Guise family by gaining possession of the young King Francis II. It was further stirred up by the famous trial of Anne du Bourg, who was executed at the end of that year.<sup>29</sup> In his responses to interrogators and in his last words, widely publicized by Crespin and others, du Bourg provided a *locus classicus* not only for Huguenot political propaganda but also for the tradition of martyrs.

The conspiracy itself was a fiasco. Hotman, one of the conspirators as well as the historian of this attempt to overthrow the Guise "tyranny," described the terrible fate of the captured "rebels." Some of them were hanged from the parapets of the castle of Amboise and others decapitated, and Hotman quoted in particular the words of his old friend the Sieur de Villemongis:

<sup>26</sup> Crespin, *Histoire des martyrs*, 2: 595.

<sup>27</sup> Agrippa d'Aubigné, *Histoire universelle* (Paris, 1616-20), ed. Alphonse de Ruble (Paris, 1886-1909), 1: 227.

<sup>28</sup> Crespin, *Histoire des martyrs*, 2: 381; and Crespin, *Histoire memorable de la persecution et saccagement du peuple de Merindole et Cabrieres . . .* ([Geneva], 1556); see also Isambert, *Recueil*, vol. 12, no. 316 (Nov. 8, 1540).

<sup>29</sup> Crespin, *Histoire des martyrs*, 2: 675.

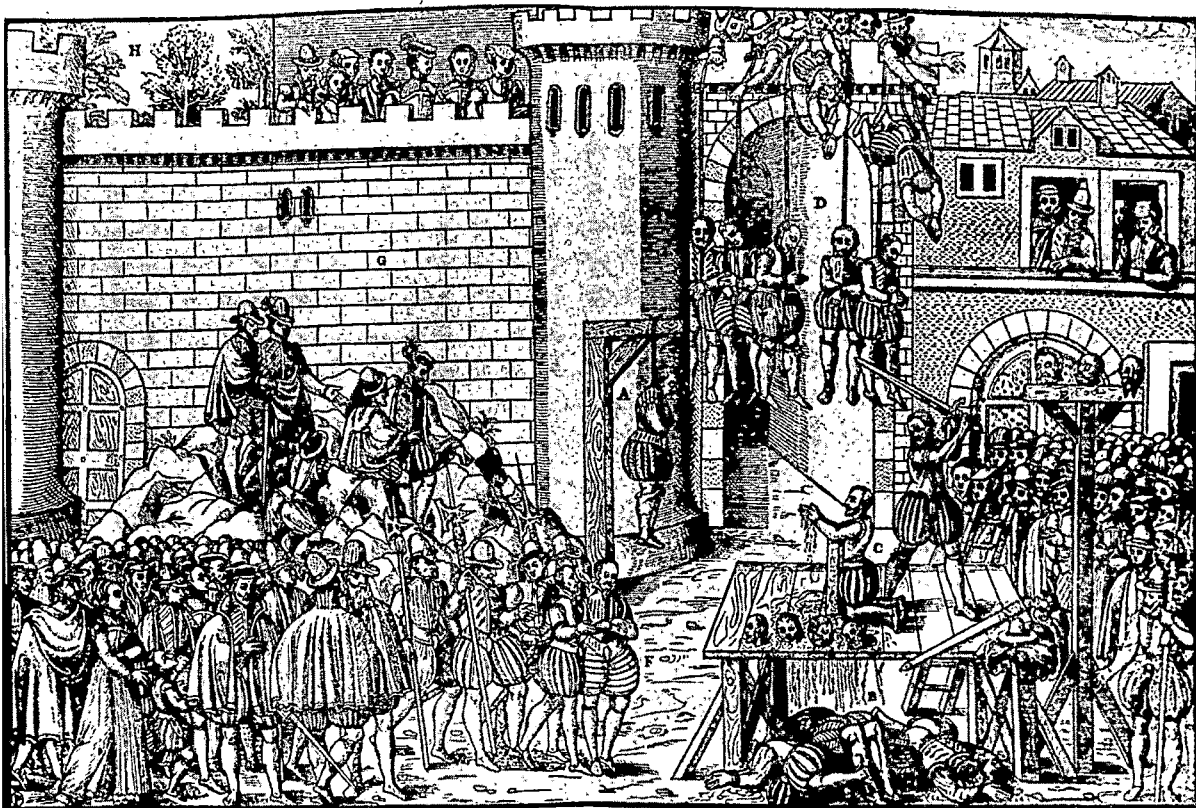


Fig. 1. Execution of conspirators at Amboise, 1560. Contemporary engraving. Photograph from Jean Héritier, *Catherine de Medici*, tr. Charlotte Haldane (New York, 1963), facing p. 208.

"Having dipped his hands in the blood of his headless companions, he lifted them as high as possible to the heavens and cried, 'Here is the blood of your children, O Lord; for this you shall be avenged.'"<sup>30</sup> This scene, depicted in an often-reproduced contemporary engraving (see fig. 1), was another to treasure in that martyrological canon that Hotman and Crespin were in the process of compiling.

The villain of this piece, of course, was the cardinal of Lorraine. He was denounced a few weeks later by Hotman as the "Tiger of France" in a pamphlet that was in effect the *J'accuse* of the religious wars. The cardinal, supported by his brother, the duke of Guise, was the true conspirator, Hotman charged, the one responsible for spilling the blood of so many innocents. "If Caesar was killed trying to gain the sceptre justly," he asked, "can we permit you to live, who pretend to it unjustly?" It must be added that Hotman himself was responsible for the creation of at least one more martyr, for during the summer the printer of his *Tiger* was seized by the police, who were apparently chasing a murderer at the time. The printer, a certain Martin Lhommet, was executed a month later in the Place Maubert near the

<sup>30</sup> [Hotman], *L'Histoire du tumulte d'Amboyse* (Strasbourg, 1560), BN, Lb.<sup>32</sup>.15; see also [Hotman], *Vita Colinii*, 33.

university. Only three copies of Hotman's book seem to have survived: one in Strasbourg, one in Switzerland, and the other in Paris, which was to be discovered in the nineteenth century, reprinted, and made the center of an extensive controversy over the "freedom of the press."<sup>31</sup>

The next two years were a time of indecision, wavering duplicity, and futile attempts at compromise. Persecutions continued, especially against the Waldensians, and a recent historian has spoken with justice of "the impossible toleration of the Colloquy of Poissy" in the fall of 1561.<sup>32</sup> Few hoped to prevent war; it was only a question of time, and the time came the following spring. The spark was provided by the tragic confrontation at Vassy between a force led by the duke of Guise and a Huguenot congregation—the "Sarajevo of the religious wars," as it has been called.<sup>33</sup> As Guise and his men approached, perhaps looking for trouble, they heard singing from the Protestant church—"I am afflicted and ready to die" is one of the verses from the psalm—and were outraged at this violation. Whichever side cast the first stone, the result was seventy-four Huguenots dead or dying and a political situation out of control. "If you will forgive a snap judgment," wrote one observer, "this is the beginning of a tragedy that we shall all be playing."<sup>34</sup> No Protestant hesitated to regard the affair as a full-fledged massacre. So within a few weeks Hotman and other observers represented it, and so later it would be interpreted by Huguenot historians, again including Hotman as well as Crespin.

The blood of these martyrs rapidly nourished the seeds of civil war. Barely a month later Huguenot forces gathered at Orleans and prepared for a major conflict. Hotman was among them and made the connection most explicitly. "In this event," he remarked of the affair of Vassy, "our leaders see the signal for a general massacre being prepared by our enemies in all parts of the kingdom."<sup>35</sup> Underlying this view one may see not only a kind of conspiracy theory of history, which was to become increasingly common, but also a doctrine of political expediency, which was to become a central feature of Huguenot propaganda and self-justification.<sup>36</sup> In either case the Massacre of Vassy was transformed into a myth—not a "nonevent" but a kind of "hyper-event," which served at once as an excuse for resist-

<sup>31</sup> [Hotman], *Epistre envoyée au tigre de la France* (Strasbourg, 1560), ed. Charles Reade, *Le Tigre de 1560* (Paris, 1875), and the offprints among Reade's papers located in Paris, Bibliothèque de la Société de l'Histoire du Protestantisme Français, MS 816, vol. 4; see also *Calendar of State Papers, Venetian*, 8: 234.

<sup>32</sup> Alain Dufour, "L'impossible tolérance au Colloque de Poissy," *Musées de Geneve*, n.s. 4 (1963): 8–11.

<sup>33</sup> H. O. Evennett, *The Cardinal of Lorraine and the Council of Trent* (Cambridge, 1930), 21.

<sup>34</sup> Etienne Pasquier, *Les Lettres* (Paris, 1617), bk. 4, no. 15; other accounts of the massacre are in Crespin and Goulart, *Histoire des martyrs*, 2: 209; and *Memoires de Condé* (London, 1743–45), 3: 111.

<sup>35</sup> Hotman to Bonifacius Amerbach, Apr. 12, 1562, Universitätsbibliothek, Basel, MS G. II. 19, fol. 148r.

<sup>36</sup> See, e.g., *Advertissement sur la fausseté de plusieurs mensonges semez par les rebelles* (Paris, 1562), in Lindsay and Neu, *French Political Pamphlets*, no. 258, which is a Catholic response to the *Histoire comprenant en brief ce qui est advenu depuis le partement des sieurs de Guise* . . .

ance, as a means of shifting all guilt to the Guise party, and as a most satisfactory explanation for the coming of the wars of religion.

There were other "massacres" over the next decade, of course, but Vassy remained the archetype; and for the rest of the century Huguenots operated upon the assumptions generated—or rather confirmed—by this tragedy. The Massacre at Vassy fitted in perfectly with the belief, based upon countless statements of official policy, that the government was literally intent upon the "extermination" of those of the religion. The theme recurred again and again over the next decade. The most famous illustration is the meeting in 1565 between Catherine de' Medici and the duke of Alba. Although their conversations were generally inconclusive, they immediately provoked suspicions among Huguenots about a universal Catholic conspiracy, turning upon a Paris-Madrid axis linked in turn with the Council of Trent, whose canons and decrees had been published only the year before. This meeting between Catherine and the duke of Alba has always been associated with St. Bartholomew seven years later, but it should be understood that the legend surrounding it came earlier and was in fact part of the general fear of that Florentine woman, Catherine de' Medici. Hotman later published a letter by her, supposedly written in 1569 and intercepted; and although it is undoubtedly spurious, it sounded quite convincing to her Protestant critics. "To restore the crown of France," she is supposed to have written, "there is no better way than to kill all the Huguenots."<sup>37</sup>

It was at this same time, that is, during the third war, that Admiral Gaspard de Coligny emerged as the leader of the Huguenots and as a figure of international influence. Protestants began looking to him as their savior, and indeed a number of cities and congregations placed themselves under his protection. In September he was officially condemned by the Parlement and deprived of his offices. According to this *arrêt*, which was printed in eight languages and widely distributed, Coligny was "giltie of traison, distourber and breaker of peace, ennemy of repos, and tranquillitie of the commonwealth: the Captain, author, and ringleder of the rebellion, conspiracie, and faction that hath bin made against the King and his State." It is very interesting to note that when Coligny perished in the Massacre of St. Bartholomew three years later, this same charge was resurrected and in fact the *arrêt* itself republished.<sup>38</sup>

(Orleans, 1562), BN, Lb.<sup>33</sup> 48; see also *Bref discours et veritable des principales coniurations de ceux de la maison de Guyse . . .* (Paris, 1565), in Lindsay and Neu, *French Political Pamphlets*, no. 407, one of the many pamphlets carrying on the attack inaugurated by Hotman's *Tigre*.

<sup>37</sup> Quoted in [Hotman], *Vita Colinii*, 57. On "le desseing de Bayonne" see, e.g., the Huguenot pamphlets collected as *Les Requests, Protestations, remonstrances et advertissements, faits par Monseigneur le Prince de Condé . . .* (Orleans, 1567), BN, Lb.<sup>33</sup> 206; and *Discours au vray des conseils et moyens qu'on a tenus pour exterminer la pure doctrine . . .* (Heidelberg, 1568), BN, Lb.<sup>33</sup> 195.

<sup>38</sup> *Arrest de la Court de Parlement contre Gaspart de Colligny, qui fut admiral de France, mis en huict langues, a sçavoir, François, Latin, Italien, Espagnol, Allemand, Flament, Anglois et Escoçois* (Paris, 1569), republished (Paris, 1574), in Lindsay and Neu, *French Political Pamphlets*, nos. 652, 653, 754.

In these same years there was a growing tendency among Protestants to attach these fears and threats to malicious foreign, especially Italian, influence. Among other suspected imports during the early stages of the religious wars came "Papemanía" and the practice of assassination—murder "Italian style," as Hotman's friend Henri Estienne called it—which was another source of martyrs.<sup>39</sup> Nor was the overall apprehension in any way allayed by specific pacification agreements, which on the contrary were suspected of being duplicitous arrangements made to trick Huguenots into letting down their guard. This was especially the case with the "limping and uneasy peace" of Saint-Germain of 1570, which Huguenots would later condemn as an evil trick and, according to one of Hotman's imaginative friends, as the diseased offspring of Catherine and her alleged consort, that devil of a cardinal (of Lorraine), who was accumulating his own share of "legends."<sup>40</sup>

All of these proliferating and intertwining anti-Catholic legends tended, as legends will do, to group themselves together around a more visible and concrete symbol that could serve as the center of a grander construct. The symbol that finally emerged to take this sovereign position was the figure, or at least the public image, of Catherine's countryman Machiavelli, who came to represent a kind of devilish counterpart to the Protestant martyr ideal. Machiavelli could not assume this posthumous role, however, until there was a crime worthy of his evil genius; and such a "crime" was indeed provided on August 24, 1572. Taken together, this massacre, the Protestant conspiracy theory, and the patterns of martyrdom created a mythology of monumental proportions.

It is in such a conceptual and emotional context, it seems to me, that the events surrounding and succeeding St. Bartholomew's Day 1572 must be understood; and so must the leading actors in the drama. The Huguenot chief Coligny was the very prototype of the Protestant saint, as represented in the official, and indeed hagiographical, account written by Hotman.<sup>41</sup> Coligny's days, even his meals, were filled with prayers, sermons, and psalm singing, and he was most solicitous about spreading the word through education and missionary work as well as by personal example. Yet he never ceased being a fighter, and in the name of these very ideals he had been playing a most dangerous political game, purportedly at the expense of that

<sup>39</sup> Henri Estienne, *Apologie pour Hérodote*, ed. Paul Ristelhuber (Paris, 1879), 1: 353; and *La Papemanie de France* (n.p., 1567), Arsenal, 8°H. 12774, vol. 1; see also Rabelais, *Pantagruel*, bk. 4, ch. 48.

<sup>40</sup> Anonymous poem, "La Paix Valois," in *Le Reveille-matin des François* ("Edinburgh" [Basel?], 1574); see also *Legende de Charles Cardinal de Lorraine*, vol. 6 of *Memoires de Condé*. In general, see my "Murd'rous Machiavel in France," *Political Science Quarterly*, 85 (1970): 545-59; and Salvo Mastellone, *Venalità e Machiavellismo in Francia (1572-1610)* (Florence, 1972).

<sup>41</sup> [Hotman], *Vita Colinii*, 130; see also Crespín and Goulart, *Histoire des martyrs*, 3: 663.

Tridentine "conspiracy," which by now had taken on the appearance of a Madrid-Rome axis. Indeed he was doing a bit of conspiring himself, though apparently not without royal permission, and had committed himself to supporting William of Orange's projected invasion of the Netherlands. But in July Coligny's credit had fallen off sharply when a force of Huguenot troops, sent to Mons to relieve William's lieutenant Louis of Nassau, was ambushed and slaughtered by the Spanish.

Meanwhile in Paris tensions were mounting. The scheduled wedding of Henry of Navarre and Catherine's daughter Marguerite, a kind of nuptial prefiguring of the conciliatory policy later adopted by the Politiques, was a hopeful sign; but it had to be postponed because of the death of Henry's mother, Jeanne d'Albret, in June. Stories immediately began to circulate, and to be believed by Huguenots such as Hotman, that she had been poisoned by the enemy. In such a heated atmosphere the wedding was set for August 18. Alarming rumors continued to fly, and in July Charles IX issued another in a long line of ordinances banning weapons and expelling vagabonds and *mauvais garçons* within twenty-four hours. Just before the wedding, it seems, Jean de Montluc, a moderate Catholic friend of the Huguenots and a diplomatic agent for Catherine, warned one of Coligny's men to get out of the city. This Coligny himself could not do, and he took up residence in a house belonging, ominously enough, to the family of Anne du Bourg. "I would rather be with you than at court," he wrote to his young and pregnant wife a few hours after the ceremony, "but I must set public advantage above private pleasure."<sup>42</sup>

The first act of the tragedy came on Friday of the same week. On that morning of August 22, as he was returning from court, Coligny was shot and seriously wounded by a certain Maurevert, called the "king's killer" because of a more successful attempt made against one of Coligny's lieutenants a few years before. The next two days Coligny spent in his quarters, attended by friends and his physician, Ambrose Paré, and visited by embarrassed members of the royal family. Coligny spoke at length and in saintly tones, providing political advice for the king, a confession of faith for his followers, and forgiveness for his would-be assassin, if not for the duke of Guise, whom he held responsible. Already, according to Hotman's secondhand account, Coligny was beginning to sound like a martyr. A royal guard was given to him but was placed under the command of an old enemy. The ban on arms was still in effect, but it did not apply to the king's men, who were rattling their weapons ostentatiously in the streets. So, as

<sup>42</sup> Quoted in [Hotman], *Vita Colinii*, 105, but differing significantly from the extant original (which has been published in the *Bulletin de la Société de l'histoire du protestantisme Français*, 1 [1852]: 369) by adding a statement to the effect that the admiral would take care not to offend his enemies (presumably to offset the charges of hostility and militancy); see also *Ordonnance du Roy portant injonction a tous ses subjects de vivre en amitié les uns avec les autres* (Paris, 1572), Arsenal, 8°H. 12778, vol. 3.

Huguenots looked back on the situation, the trap had been set; indeed the events of that weekend all seemed to fall into one terrible pattern.

The second and successful attempt on the admiral early in the morning of August 24 set off the mythopoeic process. "I am ready for death," the admiral had said before the assassin struck, and soon the "Matins of Paris" began. "Away with him, cut of his head and handes,/ And send them as a present to the Pope," the duke of Guise has been represented as saying. The words are Christopher Marlowe's,<sup>43</sup> but the deed itself has been documented and has taken its place alongside of many other atrocity stories; babies dropped from windows, bodies stripped and thrown into the Seine, some to be seen for days afterwards, a few as far as Rouen, according to a Huguenot song. As usual Paris set the fashion for the provinces, and the attacks signaled by Coligny's murder, carried out by laborers as well as by gentlemen, spread to a dozen or so towns throughout France. Many Protestants fled into the countryside. "I am sure that the wild beasts are kinder than those in human form," Hotman remarked to a friend. Later he reported that "Huguenot-hunting"—*la chasse des huguenots*—was becoming a popular sport.<sup>44</sup>

In the international Protestant community the reactions included shock and outrage but little real surprise. The inhabitants of La Rochelle had been warning Coligny of some such plot for two months, Hotman had heard, and throughout France men had been saying that Coligny was deceived at court.<sup>45</sup> "What an atrocity!" was Beza's reaction to the admiral's death. "How many times have I predicted this! How many times did I warn him about it!"<sup>46</sup> Streams of refugees carried the news in greater detail, but there seems to be little doubt about the underlying cause of the massacres. The plot was "undoubtedly general and the work of the Council of Trent," declared the Council of Geneva, which indeed feared that it would extend into their own territory.<sup>47</sup> During the fall one French agent confessed under torture that the invasion would be launched from Savoy, and though the attack did not materialize, the fear continued for the rest of the century. Hotman, then exiled in Geneva, shared and broadcast this apprehension about what he called "the Tridentine web and popish alliance called the Holy League."<sup>48</sup>

<sup>43</sup> Christopher Marlowe, *The Massacre at Paris*, based on Hotman's well-known *De Furoribus Gallicis*; see also Paul Kocher, "François Hotman and Marlowe's *The Massacre at Paris*," *PMLA*, 56 (1941): 349–68.

<sup>44</sup> Hotman to Rodolphe Gualter, Nov. 5, 1573, Zentralbibliothek, Zurich, MS F. 39, fol. 214v; the same phrase is used by Nicolas Pithou, *Histoire ecclesiastique de l'église pretendue reformée de la ville de Troyes*, published by Charles Recordon as *Le Protestantisme en Champagne* (Paris, 1863), 144.

<sup>45</sup> Hotman to William of Hesse, Oct. 6, 1572, published in Ludwig Ehinger, *Franz Hotmann, Beiträge zur vaterländischen Geschichte*, vol. 14 (Basel, 1896), no. 24.

<sup>46</sup> Beza to Thomas Tilius, Sept. 10, 1572, published in *Bulletin de la Société de l'histoire du protestantisme français*, 6 (1858): 16.

<sup>47</sup> Registres du Conseil, Sept. 4, 1572, Archives d'état, Geneva, vol. 67, fol. 201r.

<sup>48</sup> Hotman to Bullinger, Oct. 25, 1572, Zentralbibliothek, Zurich, MS S. 127, fol. 95r.

One conspicuous sign of the meta-historical impact of the massacre was the extremism of the reactions to it from all quarters. The Huguenots, of course, screamed bloody murder. The leaders were all "assassins," Hotman proclaimed, "the likes of whom no age has tolerated."<sup>49</sup> And this specifically included the king, by whose "express command" the deed was admittedly done. On the other side Catholics, arguing that the attacks were designed to crush a Huguenot "conspiracy" arising from the "Theodor-Bezian infection," concluded that it was simply retribution. "There is no gallows, cross, or torture severe enough to punish the crime of a traitor or rebel," one royalist wrote.<sup>50</sup> Others celebrated the event as providential. So François de Belleforest, later one of Hotman's chief critics, declared it to be no less than a miracle; and he found this judgment to be confirmed by the appearance of the great nova (the first in modern history) of November 1572. "I know the heretics will laugh and tax me with superstition," he added.<sup>51</sup> In fact the "heretics" were not laughing and had an explanation of the phenomenon hardly less "superstitious": to them it was the Star of Bethlehem returned, and it signified the salvation to come.

Quantitative estimates were likewise inflated. The massacre was so enormous, said one commentator, "that I doubt if posterity will ever believe it."<sup>52</sup> And in fact posterity has not believed the figures given by contemporary critics. At first Beza himself cried that over 300,000 of his brothers had been killed.<sup>53</sup> Later estimates were commonly placed at 100,000 deaths and revised downwards to 50,000. A decade later a certain "N. Froumentau" published what purported to be a statistical survey of the costs, social as well as economic, of the religious wars, and his figures offer evidence of the depth of contemporary feeling if not of exact totals. By that date, 1583, Froumentau estimated that about 765,000 persons had perished in the wars; 76,010 were civilian casualties, and of these 36,000 could be classified as "massacred"; 4,500 bodies had passed Paris on the Seine, while 6,000 had been carried by the Loire. In addition he estimated that 12,000 women and girls had been raped, and he went on to remark that, since this sort of thing so often went unreported, the total was probably twice as great. If the war continued, he remarked, the total would indeed be 100,000 instead of only 36,000.<sup>54</sup>

<sup>49</sup> Hotman to Abraham Sulzer, Oct. 3, 1572, *ibid.*, fol. 47r. See also *Declaration du roy de la cause et occasion de la mort de l'admiral* (Paris, 1572), in Lindsay and Neu, *French Political Pamphlets*, no. 729.

<sup>50</sup> Artus Desire, *La Singerie des Huguenots* . . . (Paris, 1574), 22; and *Discours de la mort et execution de Gabriel comte de Montgomery* (Paris, 1574), fol. 2v, in Lindsay and Neu, *French Political Pamphlets*, no. 790.

<sup>51</sup> François de Belleforest, *Discours sur l'heur des presages advenus de nostre temps signifiantz la felicité de regne de nostre Roy Charles* (Paris, Nov. 18, 1572), fol. 10r.

<sup>52</sup> *Resolution claire et facile sur la question . . . de la prise d'armes par les inférieurs* (Reims, 1577), 97, in BN, Lb.<sup>34</sup>. 103.

<sup>53</sup> Beza to Bullinger, Sept. 1, 1572, Zentralbibliothek, Zurich, A. 44, p. 679, cited by P. F. Geisendorf, *Théodore de Bèze* (Geneva, 1949), 306.

<sup>54</sup> The book in question is the fascinating *Secret des finances de France* (n.p., 1581), dedicated to Henry III, by N. Froumentau (no doubt a pseudonym), a friend of Hotman and author also



Whoever he was, Froumentau was a Huguenot and had nothing but contempt for the "Machiavellistes" who had so ravaged France. Yet, though his statistics are inflated by partisanship, it should be added that he did make some effort to indicate his sources (*preuves*) and often omitted provincial figures for lack of evidence, so that his conclusions are relatively far closer to reality than those of most chroniclers.<sup>55</sup> While it would be an error to take his figures at face value (as certain nineteenth-century scholars have done), it would be equally mistaken to deny this pioneering work a significant place in the history of statistics, which like other social sciences emerged at least in part from (and has never quite disassociated itself from) ideological conflict. In any case a great effort went into the making of the book, and it testifies again to the enormous intellectual impact of the wars of religion and of St. Bartholomew in particular.

In the field of political thought, of course, the impact of St. Bartholomew was even more spectacular. The massacre immediately took its place as the pivotal event in the martyrological tradition and became a central force in the flood of propaganda that poured from Protestant presses at a greater than average rate in succeeding years. It was at this point that the enlarged community of martyrs began to take on a more special political significance because of its association with the opposition party led by Henry of Navarre, who succeeded Coligny as the leader of the Huguenots. The point was made best by Beza in his *Right of Magistrates*, a tract so radical that he was refused permission to publish it in Geneva. "And this I conclude," he wrote, "that we must honor as martyrs not only those who have conquered without resistance, and by patience only, against tyrants who have persecuted the truth, but those also who, authorized by law and by competent authorities, devoted their strength to the defense of the true religion."<sup>56</sup> Here is expressed the complete politicization not only of the Calvinist cause

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of the anonymous *Miroir des François* . . . (Paris, 1581), *Le Cabinet du roy* . . . (Paris, 1581), and, if only in part, of the *Reveille-matin*, which is one work that estimates the massacred of St. Bartholomew as 100,000 (p. 78). The first book includes a dialogue between "Provence" and "Le Politique" (who figures also in the *Reveille-matin* and the *Miroir des François*) and a listing, province by province, of the taxes, charges, and expenditures, especially military, over the previous thirty-one years. The second book discusses the social costs, again province by province (pp. 378-79). The categories of the dead are ecclesiastics, nobles (Catholics and Huguenots distinguished), soldiers (again Catholics and Huguenots), those executed (many for lese majesty), Huguenots "massacred," foreigners, houses destroyed, villages burned and razed, and *filles violees*. Froumentau's *Preuves* (pp. 401-09) include records of the *Chambre des Comptes* and *contrôles de la gendarmerie* as well as "chronicles and memoirs," but he adds that no "proof" is needed to describe the horror of the massacres.

<sup>55</sup> Froumentau, *Le Secret des finances*, 418. The work has not received its due in the history either of economic thought or of statistics; the judgment of Fernand Faure, "France," in John Koren, ed., *The History of Statistics* (New York, 1918), 236-37, contradicting such credulous nineteenth-century opinions as that of Henri Baudrillart, is probably excessively severe and certainly unhistorical.

<sup>56</sup> Beza, *Du Droit des magistrats* (Geneva, 1574), critical edition by R. M. Kingdon (Geneva, 1970), 67, and translated in Julian Franklin, ed., *Constitutionalism and Resistance in the Sixteenth Century* (New York, 1969), 135.

but also of the tradition of martyrs—an unmistakable shift from passive to active resistance.

Another conspicuous by-product of St. Bartholomew was the mythology, or demonology, associated with Machiavelli, whose ideas of political behavior were widely regarded as the cause both of the massacre itself and of the political and social degeneration of France in general over the previous ten years and more. Much of this material, especially the *Reveille-Matin* and Hotman's *French Fury*, consisted of highly colored accounts of the event itself or of extravagant elegies of the "hero-martyr" Coligny,<sup>57</sup> but there was also a growing quantity of polemic and theorizing about the problem of war guilt (which has always played so fundamental a role in historical thinking) and about ideas of political resistance, constitutional government, sovereignty, and the structure of society in general. Not only the work of the Monarchomachs but also Jean Bodin's *Republic*, which may be taken as a response to the work of the Monarchomachs, grappled quite directly with the problems that the Massacre of St. Bartholomew defined in a way that made them impossible any longer to ignore.<sup>58</sup>

In more concretely historical terms the effect of the massacre was traumatic. Anticipated for a decade, after its coming it haunted an entire generation of Protestants, who suspected that it might be revived at any time and who were prepared, ideologically if not emotionally, to take their place among the future martyrs. Hotman, for example, having been sent into permanent exile, lived out his remaining eighteen years in almost perpetual fear that the fate he had just barely missed in 1572, and that had claimed so many of his friends, including Petrus Ramus, would overtake him. The tone of his correspondence is consistently that of a victim and even martyr. At one point he was convinced that the pope had hired a man to assassinate him, and in his last years he became obsessed with the idea of dying in battle for his Cause—one of the established "marks of martyrdom." Only the thought

<sup>57</sup> See, for example, *Epicedia illustri heroï Caspari Colignio . . . Beato Christi martyri, variis linguis a doctis piisq. poetis decantata* (n.p., 1572), in Lindsay and Neu, *French Political Pamphlets*, no. 725, of which Beza's own presentation copy is in the possession of Yale University Library. Professor Samuel Kinser of Northern Illinois University points out a most interesting example of the hero-martyr dichotomy in d'Aubigné's account of Coligny's death. Following the account of Jacques-Auguste de Thou, *Historiarum sui temporis opera* (Frankfurt, 1614-21), 1: 993, and of [Hotman], *Vita Colinii* and, with some modifications [Hotman], *De Furoribus Gallicis*, d'Aubigné first represented the admiral, on the point of death, as adopting a hero's stance and lamenting the fact that he was about to be killed at the hands not of a "cavalier" but only of a common servant. But a few years later, in the second edition (1626), d'Aubigné changed the picture so that Coligny appeared as a passive victim, "on his knees beside his bed" and making only the martyrly comment, "My friends, save yourselves." See *Histoire universelle*, 3: 313.

<sup>58</sup> For discussions of this large subject, see Ralph Giesey, "The Monarchomach Triumvirs: Hotman, Beza and Mornay," *Bibliothèque d'Humanisme et Renaissance*, 32 (1970): 41-56; and J. H. M. Salmon, "Bodin and the Monarchomachs," in a forthcoming volume based upon the international conference on Jean Bodin held in Munich in 1970. It has recently been argued that the monarchomach author of the *Vindiciae contra tyrannos* (n.p., 1578) was not Philippe du Plessis Mornay but rather Johan Junius de Jonge: see Derek Visser, "Junius: The Author of the *Vindiciae contra Tyrannos*?" *Tijdschrift voor Geschiedenis*, 84 (1971): 510-25.

of his children, he said, restrained him.<sup>59</sup> In many ways his attitude was typical of the French exile community of his generation, whose politics, perspective, and very lives had been shaped by the massacre.

Situated as it was at the center of such swirling emotions, revolutionary implications, festering resentments, and indeterminate intellectual repercussions, the Massacre of St. Bartholomew became a legend almost before it happened, and it grew with the telling and with the passage of time. As it furnished a target and motive for endless polemic, so it furnished a target and motive for scholarly debate—a classic “problem” for historians, though ultimately insoluble, at least in the guild-oriented, legend-prone, history-transcending terms of sixteenth-century propaganda, from which we have hardly yet escaped.<sup>60</sup> “The blood of martyrs is the seed of the church”: it has also been fuel for the labors of generations of historians, and by now more ink than blood has flowed as a result of the events of that weekend in Paris four hundred years ago. Catholic masses have ceased, Protestant lamentations have been muted, but the historical discussion continues: such is one of the forms, it seems, that myths take in our time.

<sup>59</sup> Hotman to Simon Grynaeus, Nov. 27, 1586, in B. F. Hummel, ed., *Celebrium virorum . . . epistolae ineditae* (Nuremberg, 1777), 81; and Hotman to Daniel Tossanus, Feb. 26, 1588, in *Hotomanorum epistolae* (Amsterdam, 1700), no. 162, citing *Aeneid*, bk. 2, line 353.

<sup>60</sup> Herbert Butterfield, “Lord Acton and the Massacre of St. Bartholomew,” in his *Man on His Past* (Cambridge, 1955), 171–201, is a useful survey.

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“The Sense of an Impending Clash”:  
English Working-Class Unrest before the First World War

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STANDISH MEACHAM

HISTORIANS WHO SET out to understand early twentieth-century England must sooner or later come to terms with George Dangerfield's *Strange Death of Liberal England*.<sup>1</sup> An elaborately extended metaphor, the book describes a “liberal” England that is part Gladstone and part Rupert Brooke, apparently ageless, yet doomed because unable to practice any longer the deceiving homilies it has preached for over half a century: individualism and economic servitude; self-help and deference; democracy and class-consciousness. Its death signals the birth of a new and very different England, one of strikes, suffragettes, and turmoil over Ireland, a nation as inclined to balk at compromise as was its predecessor to indulge in it.

Dangerfield appears ready to welcome this new, post-liberal England, insisting as he does that the conflicts of 1911–14, far from signs of decadence, were evidence of rebirth. Yet his dazzling impressionism is not wholly convincing. To describe Georgian England as casting off the traditions and institutions of the past implies a break with the Victorian age that had by no means come by 1914. It fails to account for the traumas of the ensuing fifty years, heralded by Strachey's *Eminent Victorians* and marked out by Versailles and Suez, in which the English have had to face down a far from moribund nineteenth century.

A second implication appears more plausible. At the root of Dangerfield's thesis lies his belief that Georgian unrest was all of a piece. Suffragettes, Orangemen, and syndicalists are joined together in one pattern to describe his “new” England. Most historians have agreed. They speak of a “mood” that, according to their predilections, may or may not signal the end of Victorian liberalism, but that almost invariably combines labor unrest, feminist militancy, and Irish insurrection into one general prewar cataclysm. Henry Pelling has now challenged that notion. In a chapter in his recent *Popular Politics and Society in Late Victorian*

<sup>1</sup> George Dangerfield, *The Strange Death of Liberal England* (New York, 1961; 1st ed., 1935).

*Britain*,<sup>2</sup> he chides historians generally and Dangerfield particularly for a readiness to see a connection where none exists. Pelling is a no-nonsense historian. For him one hard fact is worth a dozen impressionistic hypotheses. He takes actions more seriously than words. His judgments are commensurately careful and refined. He refuses to see labor unrest as part of a general protest against nineteenth-century liberalism or parliamentarianism, concluding simply that it "was only coincidental with the acute phases of the Irish and women's suffrage questions. It had its own independent and sufficient causes": primarily a low unemployment rate, which encouraged union growth, and the introduction of national insurance, which threatened union organization. "In any case, it owed little to feelings of disappointment with parliamentary institutions or existing political parties."<sup>3</sup>

Correct or not, Pelling's cold douche should compel scholars to re-think their conclusions about this complicated period. Their compulsion to do so will be fortified by their conviction that a historian's particular task is the tracing and untangling of connections. No era of English history presents a more bizarre and challenging puzzle than this one. If Dangerfield has imposed an over-elaborate design upon the facts, he has given us facts too compelling to put aside. These accounts of English men and women up in arms, dismayed and occasionally furious without knowing quite why, strike us with an immediacy—I deliberately avoid "relevancy"—born of our own more recent experiences, making us all the more anxious to understand the fiercely articulated aspirations and frustrations of another period in time.

To do so we shall begin, as Pelling does, with the labor unrest and, again as he does, with some facts. His statistics show that union membership during the years 1911-14 expanded markedly, with 1911 and 1913 experiencing the greatest percentage of growth (22 per cent and 21 per cent respectively).<sup>4</sup> Total membership in all unions rose by over 60 per cent from 1910 to 1914; membership in the so-called "new unions"—dockers, seamen, and general laborers—increased by over 300 per cent. The spread of strike activity in the 1911-14 period was just as marked. From an average of 480 strikes per year for the years 1907 to 1910, the figures leap upward: 872 in 1911, 834 in 1912, 1459 in 1913, and 972 in 1914 (a figure that would have been much higher had not England gone to war in August). Figures for the total number of working days lost are more erratic, though they are generally higher from 1911 to 1914 than from 1907 to 1910.<sup>5</sup>

<sup>2</sup> Henry Pelling, "The Labour Unrest, 1911-1914," in his *Popular Politics and Society in Late Victorian Britain* (London, 1968), 147-64.

<sup>3</sup> *Ibid.*, 164.

<sup>4</sup> These and subsequent figures from the table in *ibid.*, 149.

<sup>5</sup> Figures in thousands: 2,150 (1907); 10,790 (1908); 2,690 (1909); 9,870 (1910); 18,169 (1911);

These are clearly years of intense labor activity and unrest. By linking them to earlier periods of trade-union growth Pelling hopes to temper the impulse to treat the events of 1911-14 as unique. Although he credits syndicalism with an appeal among younger trade unionists and with some measured success within the railway unions, he will have nothing to do with the notion—suggested by Elie Halévy and subsequently elaborated by Dangerfield—that the age was one of syndicalist revolt. His caution is justified. Halévy himself notes that “during those very years in which revolutionary syndicalism was so vocal, cooperation between the trade unions and the Government became closer than before.”<sup>6</sup> Dangerfield contents himself with calling syndicalism “a convenient expression for a new energy.”<sup>7</sup> The expression was Continental in accent, if not in intent. Tom Mann, who was not embarrassed to call himself a revolutionary, preached “control of industry by ‘syndicates’ or Unions of workers, in the interest of the entire community.” Revolutionary in aim, the syndicalist movement urged the abolition of the wage system; revolutionary in method, it spurned long-term contracts between workers and masters. Syndicalists talked of an end to parliamentary government. “We shall have no need to plead with the parliamentarians to be good enough to reduce hours as the workers have been doing for fully twenty years without result. We shall be able to do this ourselves, and there will be no power on earth to stop us so long as we do not fall foul of economic principles.”<sup>8</sup>

The pronouncement borrowed its theories and tone from Europe. But, as E. H. Phelps Brown remarks, the British strikers really had very little in common with those on the Continent who seized town halls.<sup>9</sup> Lacking a revolutionary vocabulary of their own, English workers used syndicalists’ words to demand not *syndicates* but industrial unions and a more militant anticapitalism. Union members called for a minimum wage and for the Triple Alliance; at the same time they refused to forsake independent political action through the Labour party.<sup>10</sup> They listened to Tom Mann’s rhetoric but did not subscribe to his doctrine. No doubt, as

40,890 (1912—the year of the national coal strike); 9,800 (1913); 9,880 (1914). These figures suggest comparisons with the findings of Edward Shorter and Charles Tilly. They conclude that as organizational techniques increase, along with union membership, a strike turns from a test of endurance into a show of strength. Strikes are shorter but more frequent. Statistics for England in this period reflect the same general trend. See “The Shape of Strikes in France,” *Comparative Studies in Society and History*, 13 (1971): 67.

<sup>6</sup> Elie Halévy, *The Rule of Democracy*, 2 (London, 1952): 479.

<sup>7</sup> Dangerfield, *Strange Death*, 233.

<sup>8</sup> Tom Mann, *Memoirs* (London, 1967; 1st ed., 1923), 263, 206.

<sup>9</sup> E. H. Phelps Brown, *The Growth of British Industrial Relations* (London, 1965), 336. Peter Stearns, in *Revolutionary Syndicalism and French Labor* (New Brunswick, 1971), argues convincingly that syndicalism made far less impact upon the French working class than had been previously supposed.

<sup>10</sup> At the annual TUC meeting in 1912, delegates voted 1,693,000 to 48,000 for a resolution affirming support for independent political action and the “centralization of social and industrial questions in the hands of the Government and local authorities.” Speeches during the debate left no doubt that this was a test for or against syndicalism. B. C. Roberts, *The Trades Union Congress* (London, 1958), 253.

Pelling points out, the language of syndicalism appealed to those among the working class who had no sympathy for bureaucratic socialism of the Fabian stripe. But the response that appeal in turn engendered looked less like revolution than like consolidation for the purpose of more assertive and successful negotiation.<sup>11</sup>

To say that the syndicalists made little real headway is not to say that the working class remained immune from the violent fever Dangerfield claimed to have diagnosed. Pelling, not surprisingly, makes little of such notions. He is seconded by others, for example B. C. Roberts, who remarks in his history of the Trades Union Congress that though the militant spirit of the workers influenced debates, "the weight of tradition, vested interest, and the common sense of the delegates proved too great to be easily pushed aside. Strong sentiments were expressed, but no revolutionary policy was adopted, and no new faith embraced."<sup>12</sup> Phelps Brown voices surprise that England and Europe did not experience more violence, in view of the sharp check on earning power the working class sustained during the prewar years.<sup>13</sup>

The surprise of those alive then, however, was at the extent and bitterness of the disorders. Sir George Askwith, dispatched in the summer of 1911 to mediate at Hull, reported the remark of a town councilor who had experienced Paris during the Commune "and had never seen anything like this: . . . he had not known there were such people in Hull—women with hair streaming and half nude, reeling through the streets, smashing and destroying."<sup>14</sup> Those with a sense of the past might have derived consolation from the memory that working-class history in the nineteenth century was marked by violence: Captain Swing, plug plots, Sheffield outrages, Bloody Sunday. Yet the disturbances of 1911 and after seemed at the time, and contemporary descriptions make them seem now, to possess a quality of their own. Something—the ever-increasing numbers involved, the restless, never-ceasing pattern of agitations—fused them into an expression of mass dissatisfaction and mass uncertainty that was unprecedented and therefore alarming.

Nothing quite equaled the summer of 1911. Phelps Brown chronicles the events in a chapter appropriately entitled "Strife": The seamen's

<sup>11</sup> See Alan Bullock's remarks regarding a 1914 speech by Ernest Bevin in favor of federation. "It underlines the preoccupation with the practical problem of organization—unity to give 'not so much a power to attack as a power to negotiate . . . the most valuable thing we can have'—as distinct from those who put in the forefront the syndicalist argument in favor of trade-union unity, the general strike as a means to the revolutionary seizure of power." *The Life and Times of Ernest Bevin* (London, 1960), 1:41. Stearns suggests that syndicalism, as a doctrine, could appeal only to those who—unlike almost all trade unionists—were alienated from the industrial system. "In fact," he notes with regard to France, "syndicalism was largely irrelevant precisely because the workers capable of protest, led in fact by the most skilled, accepted the industrial system." *Revolutionary Syndicalism*, 106.

<sup>12</sup> Roberts, *Trades Union Congress*, 247.

<sup>13</sup> Phelps Brown, *British Industrial Relations*, 336.

<sup>14</sup> Lord Askwith, *Industrial Problems and Disputes* (London, 1920), 150.

strike a week before the coronation in June; a railwaymen's strike—pressed by the rank and file upon the leadership—in August. Rioters shot dead by troops in Liverpool; cavalry and infantry in the streets of London; the plundering of a train in Wales; Jews driven from their shops in Ebbw Vale.<sup>15</sup> And in another part of the forest, Agadir, suffragettes in the Albert Hall, and Carson prophesying Ulster's armed rebellion. The crisis subsided, but not the mood. Ben Tillett, the dockers' leader, called for class war in his annual report of 1912.

The class war is the most brutal of wars and the most pitiless. The lesson is that, in future strikes, the strikers must protect against the use of arms, with arms; protect against shooting, with shooting; protest against violence, with violence. . . . The other lesson is that Parliament is a farce and a sham, the rich man's Duma, the employer's Tammany, the Thieves' kitchen and the working man's despot. . . . Capitalism is capitalism as a tiger is a tiger; and both are savage and pitiless towards the weak.<sup>16</sup>

Tillett was stung by the defeat his union had suffered when it failed to achieve a national stoppage in 1912. But his angry words bespeak a deeper and more widespread discontent. Evidence of it was the apparently endless, uncontrollable, predominantly unofficial series of strikes in 1913 and 1914—937 in the last six months before the outbreak of war. Unions pressed ahead with recruitment and amalgamation, the better to negotiate, but uncertain what they were negotiating for. Men and women reacted more often than not from instinct. The Triple Alliance between railwaymen, miners, and transport workers is a case in point. P. S. Bagwell, historian of the railwaymen, makes it clear that the leadership of the National Union of Railwaymen had no intention of exacerbating the class struggle "by using every strike in the coal mining, dock, and railway industries as an occasion to draft reinforcements to the strikers from the million-and-a-half trade unionists covered by the agreement." Yet the rank and file expressed the belief that—in their words—the alliance would "encourage the growth of greater solidarity and a vast improvement in the social conditions of the workers, and be a powerful lever in the course of working-class emancipation."<sup>17</sup> As Phelps Brown remarks, the agreement laid the members "under no obligation to strike together, or indeed to do anything save to consult one another before proceeding with a major issue. . . . Nonetheless it was understood to have pledged all to back the cause of each. Nothing could have been less thought out." How to explain such thoughtlessness? The generally cautious Phelps Brown begins to sound like Dangerfield.

<sup>15</sup> Phelps Brown, *British Industrial Relations*, 321–23.

<sup>16</sup> From the Dock, Wharf, Riverside and General Workers' Union Annual Report, quoted in Bullock, *Ernest Bevin*, 1:35.

<sup>17</sup> P. S. Bagwell, *The Railwaymen* (London, 1963), 307.



That it should ever have been adopted can be explained only by the amount of unemployment among railwaymen and the transport workers that the coal strike had caused in 1912, prompting the thought that if all must be in it together in practice they might as well go in with a will; and by the sense of an impending general clash, a civil war between capital and labour, that was strong at that time of so many clashes.<sup>18</sup>

"The sense of an impending clash" is the sense of the evidence. It would appear to call for further accounting than that provided by Pelling. We can talk of trade-union growth and development, but we need to talk of something more—the sense that sniffed that clash between capital and labor. And as we know more of that sense, we shall be better prepared to judge if it shared in that more general mood of which so many historians have felt compelled to speak.

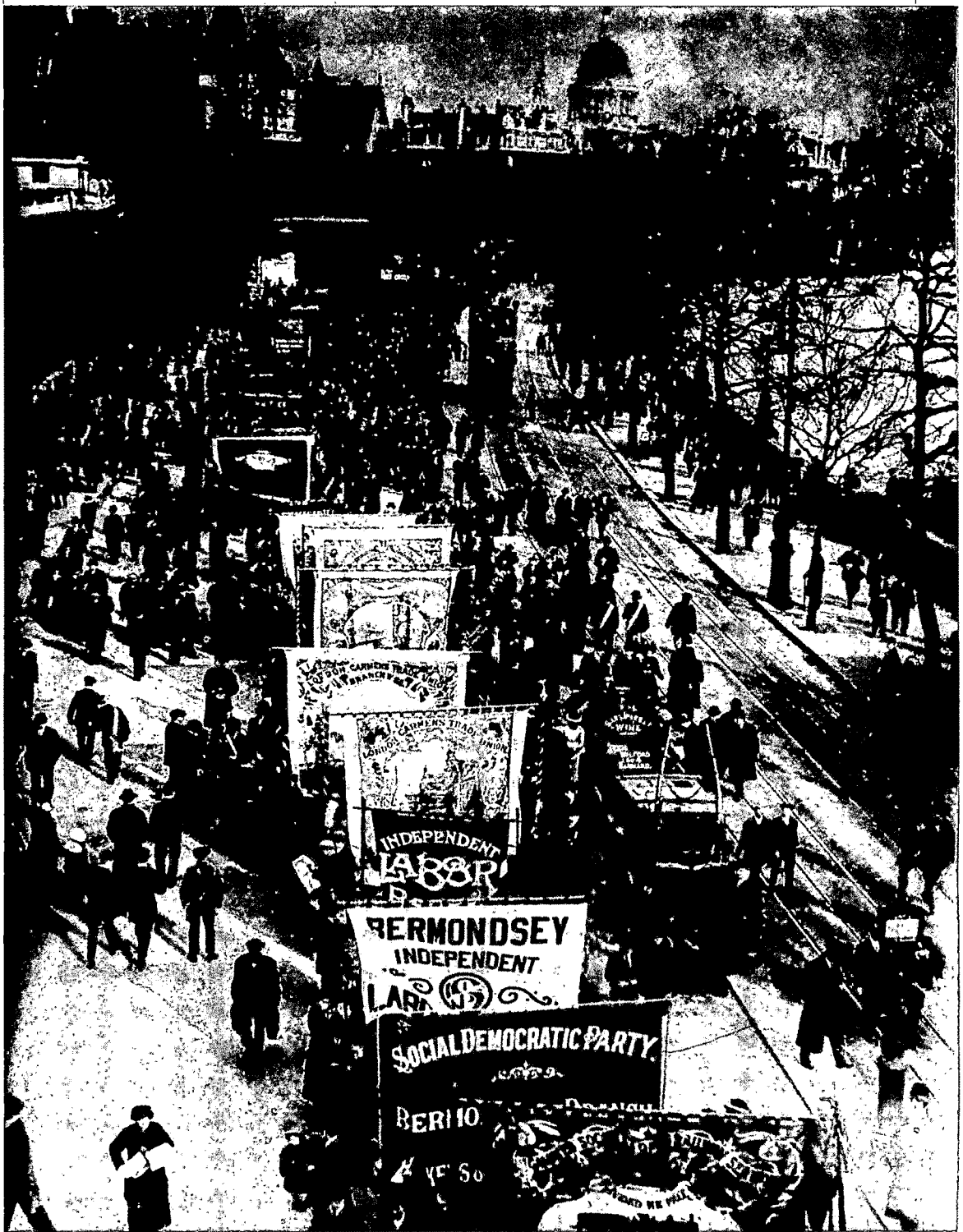
THE SENSE MAY well have sprung from economic circumstance. The general increase in real wages enjoyed by members of the English working class during the second half of the nineteenth century had induced them to take a constantly improving standard of living for granted. As working-class incomes rose,<sup>19</sup> a significant drop in the cost of living and the importation of large quantities of cheap food combined to provide a decent life for at least some of those families upon whose labor England's industrial revolution had been built. Very suddenly, around 1900, the pattern changed. Real wages declined, production rates faltered, and Britain's balance of trade began to run against her. Prices rose steadily from the late 1890s until 1901 and again from 1907 to 1913. Total national consumption rose only three per cent faster than did population; consumption of food and clothing remained almost constant: a meager enough record under any circumstances but paltry in the extreme when compared with that of the previous thirty years. By 1914 real wages managed to catch up with the cost of living.<sup>20</sup> But the race was uneven and especially exasperating for the general worker, who, if he bothered himself at all with statistics, took little consolation from the fact that the worst was over, the worst having left him exhausted and a good deal poorer than he thought he ought to be.

Askwith, the Liberal party's peripatetic labor negotiator, did not hesitate to ascribe the workers' discontented mood to economic distress.

<sup>18</sup> Phelps Brown, *British Industrial Relations*, 330.

<sup>19</sup> Leone Levi, *Wages and Earnings of the Working Class* (London, 1885), 55. Levi estimates that from 1851 to 1881 working-class incomes rose 59 per cent. He estimates that incomes of the middle to upper classes had decreased by 30 per cent, and those of the lower middle class had increased by 37 per cent, during the same period.

<sup>20</sup> Phelps Brown, *British Industrial Relations*, 17. See pp. 13 ff. for a full and clear discussion of these factors. Using a 100 per cent figure as a base for 1880, Arthur L. Bowley estimated in 1920 that real wages increased to a figure of 132 per cent by 1900 but hovered between 132 and 134 per cent from 1900 to 1913. See *The Change in the Distribution of the National Income, 1880-1913* (Oxford, 1920), 18.



Trade-union demonstration at the Embankment. London, ca. 1902.  
Photograph: Gernsheim Collection, University of Texas.

Trade had been improving, but employers thought too much of making up for some lean years in the past, and of making money, without sufficient regard to the importance of considering the position of their work-people at a time of improvement of trade. Prices had been rising, but no sufficient increase of wages and certainly no general increase, had followed the rise.<sup>21</sup>

The argument has continued to convince most historians, though few have troubled to discover the extent to which strikes were a conscious reaction to increased living costs. Strikes that workers may have considered aggressive—demands for more take-home pay—may in fact have been unconsciously defensive reactions to a decline in real wages, of which the workers themselves were only dimly aware. Pelling, who does not put much stock in the rising-cost-of-living argument, quotes K. G. J. C. Knowles to the effect that strikes more often occur during deflationary periods, when employees are faced with the tangible problem of a wage cut.<sup>22</sup> Pelling suggests that the low unemployment figures for the years 1911 to 1914 (never higher than 3.3 per cent) explain the increased pressure of working-class demands. The masters needed the men, and the men took advantage of that need to press their claims. Pelling supports this thesis by pointing out that the two most recent periods of intensive union growth and agitation—1888–91 and 1896–1901—were also periods of full employment. Of course one can agree with Pelling without dismissing as irrelevant the factor of a decline in real wages. One can say simply that the related factors of a decline in real wages and the low rate of unemployment gave workers both their grievance and their power to articulate it.

As their demands accorded with their economic discontents, they refused any longer to countenance the nineteenth-century shibboleth that pegged wages to profits. "They held that the payment of a wage determined not by supply and demand but by human needs and common decency should be a first charge upon their industry."<sup>23</sup> To this extent they were pressing a new claim, one that Dangerfield's intuitive analysis describes well when he contrasts what he terms strikes about wages with strikes about money.

<sup>21</sup> Askwith, *Industrial Problems and Disputes*, 175.

<sup>22</sup> Pelling, "Labour Unrest," 150. See K. G. J. C. Knowles, *Strikes: A Study in Industrial Conflict* (Oxford, 1952), 223: "At the level of strikes, at any rate, Trade Unions seem to have reacted to the need for resisting a money wage decrease much more strongly than to the need for achieving a money wage increase to keep pace with the cost of living." Evidence that the workers were aware of an increasing cost of living appears in the *Annual Report* of the General Federation of Trades Unions for 1911, p. 6: "The case for increased wages was never stronger than it is today; trade is booming, profits are increasing, and wealth is accumulating, but wages remain almost stationary. Between 1900 and 1908 nominal wages have risen only one per cent; in London food prices have risen during the same period by nine per cent while profits have risen by twelve and a half per cent." Quoted in Alan Fox, *A History of the National Union of Boot and Shoe Operatives* (Oxford, 1958), 341.

<sup>23</sup> Phelps Brown, *British Industrial Relations*, 313.

While a strike about wages demands either a definite rise or a restoration of a definite cut, a strike about money comes from a sense of injustice. It is not specific, but incoherent and ominous. It is a voice in the wilderness, crying for recognition, for solidarity, for power. Its echoes are innumerable.<sup>24</sup>

In some industries workers continued to strike for wages; in others, as Dangerfield suggests, they struck for money. Employers understood even less than workers just what was at stake. And since they settled strikes with wage increases, they failed, naturally enough, to understand why the workers remained unsatisfied.<sup>25</sup>

There is a chance, then, that Phelps Brown's "sense of an impending clash" was rooted in something broader, if more ill-defined, than economic circumstances alone. Recognition, solidarity, and power—Dangerfield's ominous echoes—were union goals in the years 1911 to 1914 as they had been since the middle of the nineteenth century. But just as economic circumstances during those years heightened the workers' sense of economic injustice, social conditions induced them to proclaim in a more pronounced way their identity as members of a separate working class.

The twenty or thirty years before the First World War in England witnessed increasing class division. Enmity grew between employers and workmen. No longer did clever, ambitious artisans rise into the ranks of the entrepreneurial middle class, as they had with some regularity in the beginning and even the middle periods of the century. Nor could workers any longer persuade themselves of an identity of interest between capital and labor.<sup>26</sup> Factory masters gave way to directors, whose interest in their employees' welfare often amounted to indifference. Resentful of the boycotts and sympathy strikes of the "new" unionists after 1890, management obeyed the letter of the new Factory Acts, paid what the government forced it to pay in liability insurance claims, and threw itself with a will into the formation of a British Employers' Defense Union to counter union activity. Employers may have spoken with more wisdom than they knew when they observed, during negotiations with the Engineers in 1908, that demands such as the minimum wage were transforming "commercial problems into class problems."<sup>27</sup> If such was the

<sup>24</sup> Dangerfield, *Strange Death*, 249.

<sup>25</sup> See, in this connection, Phelps Brown, *British Industrial Relations*, 321. "What the men wanted [in 1913] was often hard to tell; to their employers it seemed the upheaval was simply revolutionary; but post by post and trade by trade, settlements were reached by raising wages."

<sup>26</sup> Among others, the miners had for a time argued this point in order to persuade the owners to curb outputs. "The miner would be rescued from the mire of poverty, obviously caused by too much coal on the market, and the coal-owners too would prosper exceedingly. What a striking proof it seemed to be of the identity of interest of Capital and Labour. If only the coal-owners and themselves could get together and agree to restrict production, the Law of Supply and Demand would do the rest." But once the miners began to win wage demands, they were quick to drop this argument. R. Page Arnot, *The Miners* (London, 1949), 1:125, 127.

<sup>27</sup> H. A. Clegg, Alan Fox, and A. F. Thompson, *A History of British Trade Unions, Since 1889* (Oxford, 1964), 1: 433. The period 1900-07 did see genuine attempts on the part of both labor and management to make the system of collective bargaining work. But the general picture remains one of increasing animosity. See *ibid.*, 362.

case, their actions as well as their employees' demands had made it so.

Sharper class distinctions manifested themselves outside the factory as well. The middle classes now spoke King's English, or at least tried very hard to; the working class did not. Middle-class parents had an average of four children; working-class parents, an average of six. In matters of pay, dress, drink—and drunkenness—health, life, and death, the classes appeared to be as far apart as they were in the 1840s and in some cases were further apart. The result was a heightened sense of class-consciousness, along with the decline and fall of that ideal which mid-Victorians had hoped might make of England one sturdy phalanx of bourgeois aspirations and sentiments. All classes now felt threatened and went on the defensive. The upper class foresaw ruin in Lloyd George's mad tax schemes; the middle class, in labor unrest and foreign competition; the working class, in a plutocracy that permitted everyone else to make money at the laborer's expense. They were at each other's throats, until one begins to suspect that only George Askwith, tirelessly and infinitely patient, could command attention and respect in every quarter.

That the working class listened to Askwith was a tribute to his personality, not his profession. He was a lawyer, at a time when laboring men had little use for the law. Their widespread mistrust of the law furthered their sense of a beleaguered "we" forced to take measures against an alien "they." Throughout the nineteenth century unions and workers had struggled for security and recognition within a hostile legal environment. Each advance appeared to produce a new set of obstacles. The Taff Vale decision of 1901, which held unions liable for damages incurred by individual members during a strike, and the Osborne judgment of 1909, which declared that unions could not levy dues for political purposes, were only the two most obnoxious in a long series of legal setbacks suffered in the years after 1890. *Temperton v. Russell* (1893) ruled against boycotts. *Trollope v. London Building Trades Federation* (1895) declared union officers who published blacklists of non-union firms and free laborers to be guilty of conspiracy. *Lyons v. Wilkins* (1899) pronounced against picketing, if it was determined to be "picketing to persuade"—an interpretation which inclined one judge to remark that "you cannot make a strike effective without doing more than what is lawful."<sup>28</sup> *Charnock v. Court* (1899) found two union men guilty of watching and besetting after attempting to persuade strikebreakers in Halifax to return to Ireland and offering to pay their fare.<sup>29</sup> *Quinn v. Leathen* (1901) overturned an earlier decision favoring the Boiler-makers' attempt to establish a closed shop. All the opinions showed the

<sup>28</sup> *Ibid.*, 309. These decisions are treated in detail on pp. 308-11.

<sup>29</sup> "The effect was to render actionable any communication even of the most peaceable kind, except by letter, between strikers and men whom the employer was seeking to engage." *Ibid.*, 309.

courts in a mood to do everything in their power to curb unions and to prohibit strikes.

Hostile judgments were not the sole source of working-class disaffection for the law. Pelling takes Goldsmith's saw—"laws grind the poor and rich men rule the law"—as his text for a chapter in which he convincingly demonstrates that in many areas—debt, divorce, compensation, the selection of juries—the law conspired in explicit ways against the wage earner and the propertyless.<sup>30</sup> And with increasingly efficient law enforcement agencies, he points out, this law was increasingly brought to bear upon the resentful working class. What workers found most repellent was the obvious double standard. The Law Lords condemned the unions' boycotts and blacklists but sanctioned them when instigated by a ship-owners' cartel.<sup>31</sup> The Osborne judgment placed obstacles in the path of union officers, but not in the way of railway company directors, who wished to sit in Parliament. Tom Mann was arrested for incitement to mutiny; Sir Edward Carson sat unmolested in the House of Commons. If Ulster was to be allowed to arm, asked J. H. Thomas, why should the unions not put their funds to use as well in preparation for the revolution? So bitter were workers at their treatment by the law that when it came time to draft a bill restoring the status quo ante Taff Vale, the unions insisted that they be allowed to remain immune from the law. They would not accept from the Liberals an act that appeared to leave their fate in the hands of the judiciary.

Resentment of the law not unnaturally grew, in the minds of some, into a resentment of parliamentary politics. G. D. H. Cole and Raymond Postgate oversimplify when they suggest, in *The Common People*, that the workers in these years deserted the Labour party for direct action.<sup>32</sup> Once more Pelling provides the cautionary counter-evidence, balancing the party's electoral losses against its quite substantial organizational growth.<sup>33</sup> Yet some of the workers clearly felt a growing exasperation and impatience, a conviction that a Parliament in which, for example, over one-tenth of the members were railway directors really cared very little for the working class. John Hood, an outspoken employee of the Cambrian Railway, was fired after he testified before a parliamentary committee. Parliament admonished the railway but did nothing to get Hood his job back.<sup>34</sup> Mingled with the resentment was confusion as to

<sup>30</sup> Pelling, "Trade Unions, Workers and the Law," in *Popular Politics*, 62-81.

<sup>31</sup> Roberts, *Trades Union Congress*, 173, n. 1.

<sup>32</sup> G. D. H. Cole and Raymond Postgate, *The Common People* (London, 1938), 460.

<sup>33</sup> Pelling, "Labour and the Downfall of Liberalism," in *Popular Politics*, 117. Affiliated trade-union membership grew from 904,496 in 1906 to 1,572,391 in 1914. Affiliated trade councils and local branches of the Labour party rose in number during the same period from 73 to 177. The number of elected Labour party representatives on local governing bodies increased from 56 in 1907 to 184 in 1913. Pelling's source for these figures is the Labour party Annual Reports.

<sup>34</sup> Bagwell, *The Railwaymen*, 160-67.

the role the Labour party should play and the sort of legislation it should attempt to introduce. The election victory of 1906—even more the series of welfare measures passed in the ensuing years—appear to have taken many Labour politicians by surprise. Their campaign in 1906 had been fought on an essentially old-fashioned, radical platform: recognition of trade unions, elected school boards, free trade. Churchill and Lloyd George threw them off balance. Dished by the Liberal whizbangs, they wondered just what it was they had been elected to accomplish, and whether they were to continue to take their cue from the Liberal leadership. If that was the case, they asked themselves, how did they differ from those self-effacing Lib-Labs they had been sent to replace?

The uncertainty and frustration resulted in many cases in a general revulsion against Parliament, an institution, like the law, apparently designed by a ruling class for its own particular ends. "What is this great Parliament to whom we have entrusted our liberties composed of," asked George Baker, a member of the Miners' National Executive, during the coal strike of 1912.

It consists of six hundred and seventy men, six hundred and thirty who were capitalists and landowners, and it will be the death knell to the liberties of this movement if we hand them over to a body of this character, therefore I say we cannot hope to get much from those who represent those great interests of the country. I say that we must hold the sovereign power in our own hands.<sup>35</sup>

Parliament provided valets to assist members into their dinner jackets, Fred Jowett complained, but not with clerks to help them draft legislation. "The Statute Book has been strewn by generations of lawyer members of Parliament with pitfalls for the unwary plain man, and unless you can afford to pay for expert assistance you must flounder as best you can."<sup>36</sup> Jowett stood with Keir Hardie and Philip Snowden in denouncing the whole game of parliamentary politics. "All this jiggery-pokery of Party Government," he told the ILP in 1909, "played like a game for ascendancy and power, is no use to us."<sup>37</sup> The ways of Parliament were not the ways of the working class. Jowett proclaimed the conviction of many men and women, whose disaffection fed their class-consciousness, and that consciousness in turn helped fuel their "sense of an impending clash."

A NOT UNREASONABLE hypothesis, then, would suggest that economic realities and a heightened sense of class combined to produce the severe labor unrest that occurred in England before the First World War. It

<sup>35</sup> R. Page Arnot, *The South Wales Miners* (London, 1967), 305-06.

<sup>36</sup> From the *Clarion*, July 24, 1908, quoted in Fenner Brockway, *Socialism Over Sixty Years, The Life of Jowett of Bradford* (London, 1946), 73.

<sup>37</sup> Brockway, *Jowett*, 104.

would leave unresolved, however, the further question of whether that unrest was in any way connected with concurrent agitations over Ireland and women's rights. An answer may lie in the working man's alienation from the law and from Parliament. A glance at the history of the period is enough to confirm that suffragettes and Ulsterites shared the same disaffection. Both groups considered themselves pushed by circumstances to a desperation that turned those stately institutions into irrelevant stumbling blocks. Indeed almost all that the Victorians had claimed to cherish and had been content to take for granted was in at least some measure called into question by Edwardian and Georgian facts of life. The Boers and the German navy threatened Britain's defense establishment and, worse, her very security. International competition menaced free trade. Property confronted the land tax and employers' liability. Family felt the sting of a ruthless cost of living as well as the blows of feminist warriors. Bloomsbury considered self-discipline and duty and proclaimed them repression and self-deceit. Wilde transformed the serious business of life into a quest for muffins and cucumber sandwiches. All this sent shock after shock right through society. The more stable the institution or value, the more traumatic the challenge when it came.

Working men and women found themselves, like everyone else, struggling to come to terms with a social system and a set of values that they knew were changing fast. Not only, for example, did their class-consciousness increase ("we" against "they"); the structure of the working class itself was undergoing considerable readjustment. Victorian England tended to divide the working class into two categories—artisans and laborers—a division the working class itself accepted along with everyone else, since it appeared to conform to the facts. The artisans were the men with a skill or a trade. They earned a wage that allowed them to live decently and, if there were not too many young children, in some degree of comfort. The laborers had only their strength to sell. They were the hewers and drawers, and in many industrial towns over forty per cent of their number lived in stony poverty. The line between the two castes was wide, wider than that which separated the skilled artisans from the clerks and small shopkeepers of the lower middle class. John Burns contrasted the "old" unionists (the skilled) with the "new" (the unskilled) as they appeared at the Trades Union Congress of 1890.

Physically the "old" unionists were much bigger men than the "new," and that, no doubt, is due to the greater intensity of toil during the last twenty or thirty years . . . the "old" delegates differed from the "new" not only physically but in dress. A great number of them looked like respectable city gentlemen; wore very good coats, large watch-chains and high hats—and in many cases were of such splendid build and proportions that they presented an aldermanic, not to say a magisterial dignity.



Amongst the "new" delegates not a single one wore a tall hat. They looked workmen. They were workmen. They were not such sticklers for formality or Court procedures, but were guided more by common sense.<sup>38</sup>

At past congresses the "old" men had ruled absolute. Indeed until very recent years there had been no "new" men to raise a fuss. At the very meeting Burns describes, however, the "new" men overthrew the "old." They forced through a program of socialist demands and effected the resignation of that champion of the "old," Henry Broadhurst.

Remarks of this nature argue in favor of the theory of a labor aristocracy. One equates the "old" with the "aristocrats," better paid, with a history of steady employment in a skilled industry, and content with political Lib-Labbery. This profile conforms generally to that delineated by Eric Hobsbawm in his influential article on the subject. Using as criteria "prospects of social security," "conditions of work," "relations with social strata above and below him," "general conditions of living," "prospects of future advancement," and—"incomparably the most important"—"the level and regularity of the workers' earnings,"<sup>39</sup> he has argued that one can distinguish an aristocracy that until the beginning of the twentieth century lived closer to a lower-middle than to a working-class life. Pelling challenges the argument in *Popular Politics*.<sup>40</sup> Hobsbawm, as a Marxist, finds the theory of a labor aristocracy a comforting one. It stands, says Pelling, as a convenient explanation of "the signs of comparative affluence in the working class, and also the presence of non-revolutionary sentiments among the workers." Pelling faults Hobsbawm for defining the aristocrats almost exclusively in terms of their wages and therefore in terms of the nature of their employment.

The wages of the individual worker do not readily provide us with an index of his relative affluence, which must depend upon the size of his family, the earnings, if any, of his wife and children, the ability of his wife as a housekeeper, and his and her financial self-discipline, foresight, intelligence, and temper.<sup>41</sup>

He contends as well that to use the concept of an aristocracy to explain working-class political quiescence flies in the face of facts which suggest that, on the contrary, it was in most cases the aristocrats—engineers like Burns, Mann, and J. L. Mahon—who were the most militant.

How can one sort all this out, in the face of accounts such as Burns's? Certainly working men themselves recognized distinctions within their own ranks, based upon all the criteria Hobsbawm lists and tempered by the circumstances Pelling cites. Further, as the two historians acknowledge, the distinctions were beginning to blur with increasing rapidity

<sup>38</sup> Quoted in Raymond Postgate, *The Builders' History* (London, 1923), 343.

<sup>39</sup> Eric Hobsbawm, "The Labour Aristocracy in Nineteenth-Century Britain," in his *Labouring Men* (London, 1964), 273.

<sup>40</sup> Pelling, "The Concept of the Labour Aristocracy," in *Popular Politics*, 38.

<sup>41</sup> *Ibid.*, 52.

at the beginning of the twentieth century. Hobsbawm, the Marxist, suggests that imperialist capitalism created an "alternative hierarchy" of civil servants, clerks, and teachers that alienated the labor aristocracy from the lower middle class, of which it had been able to feel itself a part.<sup>42</sup> Hobsbawm seems to imply—although he nowhere asserts—that the result was a growing willingness on the part of labor aristocrats to identify with the interests of their less affluent brothers.<sup>43</sup>

Both Hobsbawm and Pelling discuss a second and more readily substantiated reason why the working class was growing from two classes into one: technology was making it difficult to distinguish with any assurance the skilled from the unskilled. As Pelling puts it, "The process of change gradually invaded separate crafts one after the other, reducing them all, or nearly all, to a situation in which the skill of the worker was something that could readily be 'picked up'."<sup>44</sup> Hobsbawm maintains that "the competition of machinery and the threat of down-grading" compelled some of the most "aristocratic" unions to affiliate with the Labour Representation Committee.<sup>45</sup> Burns spoke of this threat, from the point of view of the "new" and the "old," in his account of the Liverpool Congress.

There has been a lot of cant talked about the "new" and the "old" trades unionism. The difference between them, if any, is entirely due to the fact that the "old" see that labour-saving machinery is reducing the previously skilled to the level of unskilled labour, and they must, in their own interests, be less exclusive than hitherto. The "new" believe that distinctions of labour must disappear and that class prejudices that have disintegrated the Labour movement in the past must be abolished.<sup>46</sup>

The continuing introduction of new technology brought with it a breakdown of the apprenticeship system and of the implication that a man once taught his craft need never relearn it. It meant that the "unskilled," when put to work at a machine, became to that extent "skilled" and therefore indispensable. It brought the use of new materials—concrete, for example—with which the older skilled workmen were wholly unfamiliar. It dictated the substitution of cooperative for individual effort—vividly illustrated in Charles Booth's description of a locomotive

<sup>42</sup> The number of clerks in London rose from 90,000 in 1881 to 172,000 in 1911. Paul Thompson, *Socialists, Liberals, and Labour: The Struggle for London, 1885-1914* (London, 1967), 14. Between 1880 and 1914 the class of occupied people receiving an "intermediate income" more than doubled, whereas the increase in the occupied population as a whole was 40 per cent. Clegg, Fox, and Thompson, *History of Trade Unions*, 1: 473.

<sup>43</sup> G. D. H. Cole, however, reads the evidence a different way in his *Studies in Class Structure* (London, 1955), 39. He believes the increased number of clerks blurred the distinctions among classes. "The upshot was a much greater differentiation within the classes of employed persons, and a blurring of the lines of division between wage- and salary-earners, and therewith between 'workers' and the 'lower middle class.'"

<sup>44</sup> Pelling, "Concept of the Labour Aristocracy," 45-46.

<sup>45</sup> Hobsbawm, "Labour Aristocracy," 289.

<sup>46</sup> Postgate, *Builders' History*, 343-44.

factory.<sup>47</sup> And it plagued artisans accustomed to working at their own pace with the hated time and motion studies of scientific management.<sup>48</sup>

Standardization, subdivision, and mechanization disrupted working-class patterns and habits by drawing the skilled and unskilled together. The experience was bound to upset those caught up in it. And its effects manifested themselves in the strikes the workers felt increasingly compelled to call. More often than not, status was at stake, along with wages. As the unskilled worker acquired a position of some importance and security within his class, he became the more anxious to insure that black legs did not wrest it from him. Dangerfield estimates that during 1910 only 20 per cent of the strikes concerned themselves with wages; the rest arose over conditions of labor and refusal to work with non-unionists.<sup>49</sup> The dockers' major claim in 1912 was the compulsory union ticket. The rank-and-file railwaymen considered recognition a more vital goal than wage conciliation; their leaders joined the Triple Alliance from a conviction that management was systematically harassing their membership.

Status meant more than keeping the black legs at bay. There were as well the questions of the new unionists' relationship with their more "aristocratic" brothers and, among those brothers, of the need to adjust to the breakdown of specialization by craft. Unions had to redefine work patterns that had been drawn under far different conditions than those existing by 1900. Booth, writing in the nineties of the need for union regulation between workman and workman, cited

the cases of the artisan or mechanic and the assistant labourer, when the latter is forbidden to use the skilled man's tools; in the definition, or attempted definition of the spheres of work of such trades as shipwrights and ships' carpenters; of mason and bricklayers where they come together, or of plumbers and fitters.<sup>50</sup>

Postgate, in his *Builders' History*, takes the plumbers as an instance in point.

They were unable to grapple with the difficulties involved in the new processes; their only remedy was to claim that whatever had before been done by plumbers should still be done by them, though iron or china had taken the place of lead. In 1903 they recorded, without dissatisfaction, that they had longstanding and

<sup>47</sup> Charles Booth, *Labour and Life of the People of London* (3d ed.; London, 1891), 9: 227.

<sup>48</sup> See Phelps Brown, *British Industrial Relations*, 92-98; Eric Hobsbawm, "Customs, Wages, and Work-load in Nineteenth Century Industry," in *Labouring Men*, 344-70.

<sup>49</sup> Dangerfield, *Strange Death*, 247. Phelps Brown argues that there was a decline in the usefulness of boards of conciliation and arbitration, since they were designed primarily to resolve questions of wages, not status. As status began to divide labor and management after 1900, Phelps Brown maintains that these boards, which depended on some common ground between the two sides in order to succeed, ceased to function effectively. *British Industrial Relations*, 143-44, 340. Knowles, however, counters with the fact that "between 1910 and 1913 the yearly average number of strikes was over twice as high as it had been between 1904 and 1909; but the number of cases settled by conciliatory methods was almost three times as high." Knowles, *Strikes*, 66 n.1.

<sup>50</sup> Booth, *London*, 9: 252-53.

bitter consequent quarrels with no less than five other crafts—whitesmiths, hot-water fitters, zinc workers and glaziers.<sup>51</sup>

Amidst these confusions over status and jurisdictions, as Phelps Brown points out, working men found it more and more difficult to determine the issues at stake or to take them to their union leadership for solution.<sup>52</sup> Hence the more than usual number of wildcat strikes during the period. The rank and file grew impatient with leadership too often insensitive to particular local difficulties and distinctions. They resented trade-union bureaucracies (here again the matter of status was an issue) that sent out agents from comfortable London headquarters to investigate conflicts whose bitterness they could not share and to propose conciliatory solutions the workers would not accept. Workers refused to sacrifice local momentum while awaiting a decision from London; South Wales miners disparaged centralized conciliation as "class collaboration."<sup>53</sup> Too often, on the other hand, rigid regulations combined with a lack of ready talent to prevent the growth of any sort of flexible negotiating system at the local level. The resulting conflicts were regularly expressed at annual meetings of the Trades Union Congress. In 1911 the cautious strike policy of the Parliamentary Committee, which blamed violence on the police and army but spoke out strongly in favor of discipline and centralization, met with defeat at the hands of the delegates. They passed instead a resolution declaring that "no effort shall be spared by the forces of organized labour to arouse and maintain the discontent of underpaid workers with their conditions, and to quicken and assist their determination to use all possible means to win for themselves a living wage."<sup>54</sup> They attacked as well a proposal supported by the Labour leadership, which called for a thirty-day notice prior to the commencement of a strike.

Uncertain as to what its place should be within the working-class hierarchy, the rapidly expanding rank and file picked a fight with its leaders over what their place had been. At stake was not so much union policy and procedure as a redefinition on the part of the working class of its own social structure. The problem was complicated by the continued prevalence of marked and galling contrasts between working-class families and neighborhoods, which neither technology nor fulminations of the Trades Union Congress had abolished. Some workers lived comfortably; one-third

<sup>51</sup> Postgate, *Builders' History*, 378–79.

<sup>52</sup> Phelps Brown, *British Industrial Relations*, 231.

<sup>53</sup> Page Arnot, *The Miners*, 322. See also his account of the difficulties encountered by the union leadership in settling the Cambrian strike of 1911. *The Miners*, 2: 71.

<sup>54</sup> From the *Annual Report*, 1911, quoted in Roberts, *Trades Union Congress*, 241.

<sup>55</sup> Of those members of the working class in the 1890s whose weekly income was less than thirty shillings, D. J. Oddy has this to say: "The individuals in these groups ate only six pounds of bread per week, and the whole diet provided less than 2000 kilo-calories a day. On the whole, what they ate was very similar to the diet of the worst group [needlewomen in London] examined by Dr. Edward Smith in 1863, except that sugar consumption was double that of the 1860's." D. J. Oddy, "Working Class Diets," *Economic History Review*, 23 (1970): 320.

lived below the poverty line.<sup>55</sup> A growing number sat on city councils; a majority still did not have the vote.<sup>56</sup> A handful won places in secondary schools; twelve per cent of the Middlesbrough working class could neither read nor write.<sup>57</sup> The distinctions and the jealousies engendered by "rising expectations" did nothing to simplify the job of social redefinition.

That task was but one of several workers faced as they tried to make twentieth-century sense of nineteenth-century institutions. Another was the acceptance of the changing circumstances of welfare and charity and of changing attitudes toward social service. The years 1890 to 1914 witnessed the climax of that struggle Asa Briggs has described, between the theories of the social-service state and the welfare state. Victorian England had been a social-service state, in which the upper and middle classes, through their own or the government's agency, extended helping hands to the deserving poor. Modern England is a welfare state, in which the government offers "all citizens without distinction of status or class . . . the best standards available in relation to a certain agreed range of social services."<sup>58</sup> Edwardian England straddled the two eras: the Charity Organization Society, the most sophisticated rationalization of the old; the Webbs' Minority Report, a daring blueprint for the new. And the working class was by no means ready to exchange the former for the latter.

Pelling has given us convincing evidence on that point. A chapter in *Popular Politics*<sup>59</sup> catalogs working-class opposition to the extension of state power, "which is generally regarded as having laid the foundations of the welfare state."<sup>60</sup> He lists the Poor Law of 1834, the Housing Acts of the 1870s, and the Education Acts of 1870 and 1880 as examples of interventionist legislation that met with working-class hostility, or indifference, or both. Such opposition, Pelling points out, went hand in hand with working-class mistrust of the law. Certain trades welcomed government-imposed safety regulations and some, but not all, unions supported bills delimiting the factory working day.<sup>61</sup> Neither political party won working-class support by promulgating elaborate programs of social reform.

When the Liberals did introduce pensions, insurance, and labor ex-

<sup>55</sup> Only 60 per cent of the males of voting age had the vote after 1885. N. Blewett, "The Franchise in the United Kingdom, 1885-1918," *Past and Present*, 32 (1965): 31.

<sup>57</sup> Lady Bell, *At The Works* (London, 1969; 1st ed., 1907), 162.

<sup>58</sup> Asa Briggs, "The Welfare State in Historical Perspective," *Archives Européennes de Sociologie*, 2 (1961): 228.

<sup>59</sup> Pelling, "The Working Class and the Origins of the Welfare State," in *Popular Politics*, 1-18.

<sup>60</sup> *Ibid.*, 2.

<sup>61</sup> Page Arnot recounts the astonishment of Continental delegates to the Miners' International in Brussels in 1893, when two Englishmen opposed a motion to affirm the principle of the legal eight-hour day. "Both were working miners from Northumberland; and both were convinced that Parliament could not 'righteously and justly interfere with the hours of adult labour.' This reduced Emile Basley of France to 'a state of wonderment.' The amendment meant, in four words, 'objection to state intervention': from this he had presumed the representatives of Northumberland were anarchists. . . . 'What,' he asked, 'was the use of trade unionists sending representatives to parliament if, when they got there, these "anarchist" representatives were not to legislate at all?'" Page Arnot, *The Miners*, 1: 172.

changes, their progressivism, as we have seen, bewildered more often than it delighted the working man. The Liberal innovation most popular with the working class—old-age pensions—incurred the bitter opposition of the Friendly Societies, which saw them, as they saw national insurance, as an unwarranted and essentially unnecessary threat to their existence.<sup>62</sup> All other welfare schemes encountered at least some measure of opposition when proposed, and confusion and disappointment when enacted.<sup>63</sup> Intervention too often appeared to the working class as another name for interference.<sup>64</sup> The Poor Law had taught them that. Lady Bell, in her description of the lives of Middlesbrough iron workers, reports local disgust at a government attempt to regulate and hence improve the practice of midwifery through licensing. Since many of the midwives either forgot or refused to bother to pay their guinea fee, the practical result of this well-intentioned reform was to reduce the number of the town's practitioners.<sup>65</sup> Lady Bell concluded that social reformers lose more than they gain when they prod the worker to accept improvements he has not first been tutored to understand.

One learns after long experience that what he wants is that the next step that is of any concern to him, and that he chooses to take in his own way, should be facilitated for him; but one has to be very careful not to go a little further and make superfluous suggestions which he then, much to the discomfort of the suggester, absolutely negatives and dismisses.<sup>66</sup>

The worker had to be weaned from the belief that he would be the weaker for having accepted relief and made to see that the country might be the stronger if he would only accept his government's support. Throughout the nineteenth century the English middle class did its best to see that he learned that first lesson; now, in the twentieth, its reform-minded heirs pressed him to understand the second. That their task of re-education was arduous and frustrating speaks eloquently of the thoroughness with which the Victorian bourgeoisie catechized its working-class pupils.

Two further examples will suggest the breadth of this problem of readjustment and adaptation that the working class, along with everyone else, was forced to confront. Richard Hoggart has remarked that "the more we try to reach the core of working-class attitudes, the more surely does it appear that that core is a sense of the personal, the concrete, the local; it is embodied in the idea of, first, the family, and, second, the neighborhood."<sup>67</sup>

<sup>62</sup> Bentley B. Gilbert, *The Evolution of National Insurance in Great Britain* (London, 1966), 160.

<sup>63</sup> See, for example, Page Arnot, *The Miners*, 2: 108, 129-21, for the disappointed reaction of the miners to the Minimum Wage Act of 1912.

<sup>64</sup> Though the unions did learn that by forcing government intervention as a third party in an industry-wide strike, they could sometimes achieve victory over their employers. The miners proved the point in 1912, when they pressured the government into passing a minimum wage bill.

<sup>65</sup> Bell, *At the Works*, 265.

<sup>66</sup> *Ibid.*, 98.

<sup>67</sup> Richard Hoggart, *The Uses of Literacy* (Boston, 1961), 32.

One discovers that core at the center of Edwardian working-class life, constantly threatened, however, by a modern sense of urban alienation. Many working men and women were born and grew up far from their subsequent homes—either in the country, in another city, or in another quarter of the same city in which they later lived. Hope of better wages, the vicissitudes of urban renewal, a growing family, the lack of work near to hand, the availability of a workers' train—these and other factors, singly or in combination, were forcing more and more members of the working class to move about. The more they moved, the more difficult it became to combat the rootlessness and loneliness of a series of flats or tiny houses, generally alike, but not for that reason any the more familiar or comforting. They fought back with ritual, creating rhythms from the events of the present and epics of everyday occurrences of the past.

There is a rhythm, but it is the rhythm of a brick world, to which those of the seasons or of the great religious festivals are only incidental. At each weekend, perhaps, there is Friday night's shopping with Mother down a shopping street that is all bustle and warmth and gregarious spending, and the trains rattle and flash past constantly. There is the whole weekend ahead, with the pictures on Saturday, or a chapel concert with a hot supper in the Sunday school room; bacon and eggs for Sunday breakfast, the big Sunday tea. Then, throughout the year, Pancake Tuesday, Voting Day, which is always a holiday, Hotcross Buns on Good Friday, the Autumn "Feast," Mischief Night, and all the weeks of cadging and collecting for Bonfire Night. It is a truly urban fire, with very little wood that has known a tree for the past few years. . . . As the fireworks run out, you bake potatoes round the fire's edges.<sup>68</sup>

Remove the reference to pictures on Saturday, and the rhythm is much like that Edwardian working-class families maintained as they faced down the facts of urban anonymity and psychological nakedness.

The rhythm implied an allotted time for pleasure. Edwardians practiced enjoyment as Victorians had preached self-denial. Here was another readjustment, and an especially difficult one for the working class to make. Once rich men and women ceased to be afraid of ostentation, they had only to begin spending the capital their Victorian forebears had felt constrained to leave untouched. A working man, on the other hand, soon found to his sorrow that to desert thrift for pleasure, even to the extent of a glass or two of beer, was to put his entire family in jeopardy. Wages of twenty-five shillings per week, each halfpenny budgeted for the necessities of life, allowed for no such indulgences.<sup>69</sup> Economic circumstances

<sup>68</sup> *Ibid.*, 59.

<sup>69</sup> Rowntree's famous passage describing the amenities one would have to do without if one wished to live above the poverty line makes the point. "A family living upon the scale allowed for in this estimate must never spend a penny on railway fare or omnibus. They must never go into the country unless they walk. They must never purchase a halfpenny newspaper or spend a penny to buy a ticket for a popular concert. They must write no letters to absent children, for they cannot afford to pay the postage. They must never contribute anything to their church or chapel, or give any help to a neighbour which costs them money. They cannot

forced him to deny himself the pleasures others were beginning to take for granted and enjoy. Lady Bell deplored the fact that no one taught the working class "the great, the much-needed lesson for both old and young, the need for self-control both in word and deed."<sup>70</sup> The middle classes, schoolmasters to the poor, were ceasing to believe the lesson. Left to themselves and encouraged by propaganda such as Robert Blatchford's *Merrie England*,<sup>71</sup> workers as often as not failed to learn it, until poverty descended to teach it in perhaps the only way it could still be taught.

EVERY AGE IS an age of transition, but this was transition with a vengeance. Social institutions will hold people together only as long as the people believe in the institutions' worth or in their existence. Events brought Victorian institutions into question; once questioned, they ceased to bind Englishmen as they had before. Centrifugal force—the pull away from a common acceptance of essentially middle-class Victorian assumptions—spun the English off into the separate confusions that mark the period. The Ulsterites, the suffragettes, the workers, to a lesser degree the rest of their countrymen, were looking for ways to cope with the reality of change. In this, their "revolts" had common roots, and their uncertainties fed each other. As Phelps Brown remarks: "Events which stir the emotions in the same way boost one another's signals when they are in circuit with one another, and their aggregate effect is greater than the sum of the effects they would take separately."<sup>72</sup>

The working men's "sense of an impending clash" sprang, then, from their sense of economic grievance and of class-consciousness. It was almost certainly heightened by their understandable confusion at the rapid disappearance of Victorian institutions they had taken for granted. They were upset and not at all sure what to do. So they did many apparently contradictory things at once: they struck for wages and struck for status; they sent the TUC their money and bucked its leadership; they opposed na-

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save, nor can they join sick club or Trade Union, because they cannot pay the necessary subscriptions. The children must have no pocket money for dolls, marbles, or sweets. The father must smoke no tobacco, and must drink no beer. The mother must never buy any pretty clothes for herself or for her children, the character of the family wardrobe as of the family diet governed by the regulation, 'Nothing must be bought but that which is absolutely necessary for the maintenance of physical health, and what is bought must be of the plainest and most economical description.' Should a child fall ill, it must be attended by the parish doctor; should it die, it must be buried by the parish. Finally, the wage-earner must never be absent from his work for a single day." B. Seebohm Rowntree, *Poverty, A Study of Town Life* (London, 1910), 133-34.

<sup>70</sup> Bell, *At the Works*, 220

<sup>71</sup> Blatchford included pleasure among the "mental needs of life." "As for pleasures, their name is legion. There are such pleasures as walking, rowing, swimming, football, cricket. There are the arts, and the drama. There are the beauties of nature. There are travel and adventure. Mere words cannot convey an idea of the intensity of these pleasures." *Merrie England* (New York, 1966; 1st ed., 1894), 20.

<sup>72</sup> Phelps Brown, *British Industrial Relations*, 332.



tional insurance while they collected their pensions; they voted Labour and agreed that Labour accomplished almost nothing.

Dangerfield to the contrary, there was little joy in them as they surveyed their strange-seeming world. He is mistaken when he describes them as "wanting to live, to take chances, to throw caution to the winds."<sup>73</sup> One does catch this sense in the memoirs of some of the intellectual rebels of the time. Leonard Woolf describes the excitement of taking part "in the spring-time of a conscious revolt against the social, political, religious, moral, intellectual, and artistic institutions, beliefs, and standards of our fathers and grandfathers." "It seemed," he writes, "as though human beings might really be on the brink of becoming civilized. It was partly the feeling of relief and release as we broke out of the fog of Victorianism."<sup>74</sup> Few members of the working class shared that *élan*. Trapped between a consciousness of "rising expectations" and an atavistic fatalism that denied the possibility of change, they could not subscribe wholeheartedly to a belief in progress and a better future. The past, though a matter of dreary, impoverished sameness, was nevertheless a matter of certainty.

Was it not uncertainty, even fear perhaps, that as much as anything drove the English worker to act as he did in those years before 1914, to spend as much time as he did on the defensive?<sup>75</sup> Until historians have studied authentic working-class sources more thoroughly than they have, the suggestion can be no more than that. A final clue may lie, however, in the words with which Barrington Moore concludes his *Social Origins of Dictatorship and Democracy*. "The wellsprings of human freedom lie not only where Marx saw them, in the aspirations of classes about to take power, but perhaps even more in the dying wail of a class over whom the wave of progress is about to roll."<sup>76</sup>

<sup>73</sup> Dangerfield, *Strange Death*, 235.

<sup>74</sup> Leonard Woolf, *Beginning Again* (London, 1964), 34, 36.

<sup>75</sup> Stearns argues that French workers, especially during the period 1905-10, struck most commonly in defense of what they had managed to gain in the past rather than in anticipation of what they might achieve in the future. "A large number of strikes that apparently sought higher wage rates in reality were defending accustomed levels. . . . French workers had obviously improved their standards of living in the preceding decades, but they still had not collectively displayed a commitment to steady progress in the future." *Revolutionary Syndicalism*, 46. The same might well have been true of English workers.

<sup>76</sup> Barrington Moore, *The Social Origins of Dictatorship and Democracy* (Boston, 1967), 505.

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## Stalin and the Prospects of a Separate Peace in World War II

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VOJTECH MASTNY

THE FEAR THAT Moscow and Berlin might again come to terms preoccupied American and British statesmen long after Hitler had forced the unwilling Stalin to join the Allied coalition. In the opinion of George F. Kennan, a touchstone for appraising the wartime policies of the Americans and the British "will be found . . . in the soundness and accuracy of their fears with relation to the possibility of a separate German-Soviet peace."<sup>1</sup> The possibility never materialized; but does that mean a Russian-German rapprochement was merely an empty threat? This is indeed an important question, for the answer may influence the appraisal of both the Soviet war aims and the Western responses to them—two central themes in the current debate about the origins of the cold war.

If Stalin contemplated a separate peace at any particular moment during the war, his objectives must have been more flexible than they appear in retrospect. Any compromise with Germany would inevitably have given him considerably less territory and influence than he achieved in 1945. But was there ever a favorable situation for a compromise? And even if there was, would the Russians have discussed peace with Hitler, or would they have required his replacement by a non-Nazi government first? In either case, how much did the prospect of an armistice on the eastern front influence the policies of the Western allies? Was not Stalin himself creating a false alarm in order to exact concessions from his coalition partners?

These intriguing questions have so far received surprisingly little scholarly attention. The late Herbert Feis, for example, all but ignored them in his history of the Great Alliance.<sup>2</sup> William H. McNeill has confined his opinion to a few cautious remarks.<sup>3</sup> In the 1950s John A. Lukacs was the only one to come to grips with the problem as far as his limited evidence then

<sup>1</sup> George F. Kennan, *Russia and the West under Lenin and Stalin* (Boston, 1961), 362-63.

<sup>2</sup> Herbert Feis, *Churchill, Roosevelt, Stalin: The War They Waged and the Peace They Sought* (Princeton, 1967), 143.

<sup>3</sup> William H. McNeill, *America, Britain, and Russia: Their Co-Operation and Conflict* (London, 1953), 275, 324.

allowed; he concluded that Moscow had considered the alternative of a separate peace seriously.<sup>4</sup> A few other authors have subsequently written about it, but none has done so within the broad context the subject deserves.<sup>5</sup> The shortage of reliable evidence has been the principal obstacle for historians. All their inquiries have so far depended almost exclusively upon the testimony of Peter Kleist, the less than reputable former aide of the Nazi foreign minister, Joachim von Ribbentrop. In his postwar memoirs Kleist maintained that during the war he had received several Soviet peace feelers in Stockholm.<sup>6</sup> German authors have been somewhat more inclined to take his word than have their foreign colleagues but have done little to substantiate their judgment.<sup>7</sup> It is to be regretted that the conclusive evidence from the Moscow archives may not be available for some time to come. In the meantime, however, more than has previously been known can be deduced from additional sources accessible in the West. Unpublished records of the United States Department of State and Department of the Army—particularly the military intelligence papers—captured German documents, and little-known publications from Eastern Europe are among the sources used in this essay for the first time.<sup>8</sup>

From the Soviet point of view a peace with Berlin would have necessarily meant two very different things before and after Stalingrad. During the period of German ascendancy it could only have entailed strategic surrender—"the orderly capitulation of the remaining forces" in order to prevent the worse consequences of a defeat.<sup>9</sup> After Stalingrad had reversed military fortunes a genuine compromise was theoretically possible. Accordingly peace prospects during the periods preceding and following that crucial turning point of the war ought to be considered separately.

No sooner did Hitler and Stalin begin to fight than they were suspected of seeking a settlement. The United States assistant secretary of state, Adolf A. Berle, Jr., for example, considered their rapprochement possible as early as July 1941; the British Embassy in Moscow expected a Soviet bid for peace

<sup>4</sup> John A. Lukacs, *The Great Powers and Eastern Europe* (New York, 1953), 502-24.

<sup>5</sup> See, for example, Karl-Heinz Minuth, "Sowjetisch-deutsche Friedenskontakte," *Geschichte in Wissenschaft und Unterricht*, 16 (1965): 38-45.

<sup>6</sup> The English translation, entitled *The European Tragedy* (London, 1965), is an expanded version of the original edition of Peter Kleist, *Zwischen Hitler und Stalin, 1939-1945* (Bonn, 1950).

<sup>7</sup> For example, Lothar Gruchmann, *Der Zweite Weltkrieg: Kriegführung und Politik* (Munich, 1967), 242-43; and Boris Meissner, *Russland, die Westmächte und Deutschland* (Hamburg, 1954), 13-17.

<sup>8</sup> The following archival sources will be cited throughout this essay: Records of the Office of Strategic Services, record group 226, National Archives, Washington (hereafter OSS); Records of the War Department, General and Special Staffs—Military Intelligence (G-2), record group 165, Washington National Record Center (hereafter G-2); General Records of the Department of State, record group 59, National Archives (hereafter DS); Records of the German foreign office (Auswärtiges Amt), microcopy T-120, National Archives (hereafter AA); Records of the British Foreign Office, General Correspondence, FO 371, Public Record Office, London (hereafter FO); and Records of the British War Cabinet, Memoranda, CAB 66, Public Record Office, (hereafter CAB).

<sup>9</sup> Paul Kecskemeti, *Strategic Surrender: The Politics of Victory and Defeat* (New York, 1964), v.

two months later.<sup>10</sup> In early 1942 London was especially apprehensive about the chances of a sudden armistice.<sup>11</sup> Lord Halifax, the ambassador to Washington, repeatedly expressed his concern in private conversations.<sup>12</sup> For reasons inherent in the Soviet system, a peace initiative from Moscow was not out of the question. The Bolsheviks had proved before that the ideological antagonism between nazism and communism did not necessarily rule out collaboration. And the Russian government was under no obligation to its constituents to maintain the alliance with the West. The autocrat, who had all decisions about war and peace in his hands, could enforce his will easily, regardless of public opinion, if he chose to do so. Extensive freedom from domestic restraints was but one of the many affinities between Stalin and Hitler—the two dictators who hated but respected each other. Of the two, it was Stalin rather than Hitler who tended to underestimate the incompatibility of their respective interests. Stalin personally had a greater share than Hitler in bringing to conclusion the pact of August 1939, had benefited from it more than his Nazi counterpart, and had therefore tried to preserve it until the very last moment.

As a matter of fact, Soviet spokesmen expressed nostalgia for a *modus vivendi* with Berlin even after the Nazis had already violated it. On June 24, 1941, for example, the emigré leadership of the German Communist party in Moscow issued a statement in favor of an "indestructible alliance" between the German and Russian peoples.<sup>13</sup> Two days later Walter Ulbricht, that particularly faithful interpreter of Stalin's thoughts, drafted an appeal to Hitler's soldiers in which Ulbricht drew a sharp distinction between their unjustified attack on the "Fatherland of Socialism" and their struggle against the Western "plutocracies."<sup>14</sup> Although such explicit statements soon ceased, they nevertheless suggest that Stalin would have regarded a renewed understanding with Germany as both feasible and desirable. From his point of view an arrangement with the congenial Nazi dictator could still have been easier than dealing with the Western statesmen whose thinking was so alien to his own and who were particularly reluctant to grant him title to the territories he had seized in 1939 and 1940.

In early 1942 the British government was actually quite tempted to endorse Stalin's territorial claims in Eastern Europe in order to avert his

<sup>10</sup> Adolf A. Berle, Jr. to J. Edgar Hoover, July 10, 1941, *Foreign Relations of the United States* (hereafter *FRUS*), 1941, 1: *General, The Soviet Union* (Washington, 1958), 789-90; Sumner Welles, "Two Roosevelt Decisions: One Debit, One Credit," *Foreign Affairs*, 29 (1950-51): 189.

<sup>11</sup> Llewellyn Woodward, *British Foreign Policy in the Second World War*, 2 (London, 1971): 239, 244.

<sup>12</sup> [Sumner] W[elles], memorandum on conversation with Lord Halifax, Mar. 30, 1942, *FRUS*, 1942, 3: *Europe* (Washington, 1961), 537. See also Jan Ciechanowski, *Defeat in Victory* (New York, 1947), 109.

<sup>13</sup> Statement printed in the journal *Kommunisticheskii International* (1941), no. 6-7, quoted in report no. 44338 C, OSS.

<sup>14</sup> Walter Ulbricht, "Entwurf eines Aufrufes an die deutschen Soldaten," June 26, 1941, in his *Zur Geschichte der deutschen Arbeiterbewegung: Aus Reden und Aufsätzen*, 2: (1933-1946), supp. 2 (Berlin, 1968): 221.

possible defection. Churchill explained the reasons to Roosevelt in unequivocal terms: "The increasing gravity of the war has led me to feel that the principles of the Atlantic Charter ought not to be construed so as to deny Russia the frontiers she occupied when Germany attacked her."<sup>15</sup> But Washington objected on both moral and practical grounds and insisted that any reference to the controversial frontiers be deleted from the final text of the British-Soviet treaty, which was signed on May 26, 1942.<sup>16</sup> The Americans were loath to bestow an air of legitimacy upon the annexations that Stalin had carried out in a particularly scandalous fashion during his collusion with Hitler. In addition they were trying to avoid the Wilsonian predicament by abstaining from any commitments that might prove embarrassing after the war.

The policy of refusing to abet Stalin's misdeeds had unquestionable virtues, but the promotion of his faith in the alliance was not one of them. On December 6, 1941, the Soviet leader told the British foreign secretary, Anthony Eden, "If our war aims were different, then there would be no alliance."<sup>17</sup> Rightly or wrongly Stalin considered the "second front" the supreme proof of cooperation,<sup>18</sup> and in his mind its absence could even have cast an altogether sinister light upon the extensive material aid he was getting. Were the Western capitalists not sending him just enough to promote a military stalemate in order to benefit later from the mutual exhaustion of both belligerents? Such a putative scheme must have seemed especially plausible to Stalin, since he himself had thought along these lines in 1939-41, as the shifting emphases of the Comintern's statements at that time indicate.<sup>19</sup> And it should be remembered that the Russians, in conformity with Marxist doctrine as they understood it, vastly exaggerated the influence that British aristocrats and American financiers who were hostile to the Soviet Union were able to exercise upon the policies of their respective governments.

In this context it matters little that political considerations did not in fact cause the repeated postponements of the landing on the Continent;<sup>20</sup> in all fairness some Western actions could have hardly failed to create an impression of duplicity. The Russians were perhaps understandably disturbed about the inability of the British to give a convincing explanation of the awkward peace mission of Rudolf Hess—admittedly a difficult thing to do—and about the unwillingness of the London government to bring the

<sup>15</sup> Churchill to Roosevelt, Mar. 7, 1942, in Churchill, *The Hinge of Fate* (London, 1951), 293.

<sup>16</sup> See the treaty in Louise W. Holborn, ed., *War and Peace Aims of the United Nations*, 1 (Boston, 1943): 235-37.

<sup>17</sup> Stalin, quoted in Woodward, *British Foreign Policy*, 2: 223.

<sup>18</sup> The official Soviet view is summarized in V. M. Kulish, *Vtoroi front* (The Second Front) (Moscow, 1960), 16-20.

<sup>19</sup> The Comintern's statements are analyzed in Kurt Krupinski, *Die Komintern seit Kriegsausbruch* (Berlin, 1941).

<sup>20</sup> Evidence that it was not political considerations that postponed the Allied landings appears in Maurice Matloff and Edwin S. Snell, *Strategic Planning for Coalition Warfare, 1941-1942* (Washington, 1953), 217-32, 322-27.

self-appointed intermediary to trial.<sup>21</sup> Even more disconcerting was Roosevelt's unfulfilled promise to launch the invasion of Europe before 1943, a promise he had let Molotov believe was definite.<sup>22</sup> Then, too, no matter how genuine the president's desire to promote mutual trust, his volubility may have had the very opposite effect on Soviet minds ever disposed to suspect the worst.<sup>23</sup>

Thus Stalin had reasons, albeit the wrong ones, to question the motives of his allies in 1942. Yet these alone did not necessarily prompt him to desert them. Very soon after June 1941 the Nazis proved by their behavior that so long as they retained the upper hand the only peace terms the Soviet Union could expect would be complete submission. Russia's sole alternative was to fight on, and any signs of faltering resolve would have, if anything, given further encouragement to the enemy; or even worse, they might make the dreaded Western reversal of alliances a self-fulfilling prophecy. Stalin could have been exasperated with the coalition, but he had to maintain it. On balance, therefore, any Russian efforts to come to terms with Germany before Stalingrad may be dismissed as mere products of anxious imagination.

DURING THE WINTER of 1942-43 the situation changed radically. Having compelled the Nazis to retreat from the Caucasus, the Red Army encircled German forces at Stalingrad on November 23, 1942, and annihilated them by February 1, 1943. The power of the two belligerents now seemed to be almost balanced—a condition that E. A. Boltin, a leading Soviet military historian, believes lasted until mid-1943.<sup>24</sup> But although Moscow's military prospects brightened, a complete defeat of the enemy was still in the distant future, especially as long as the second front remained uncertain. Past combat experience gave every reason to believe that the path of victory would be arduous, and the Russians had already suffered appalling losses. Yet the military situation after Stalingrad offered the Soviet Union, for the first time since the beginning of the war, both the opportunity and the inducement to trade military assets for a political compromise. Soviet leaders, well versed in the Marxist notions about the interaction of war and politics, could have hardly missed that message. And indeed, an authoritative article published in January 1943 in the party journal *Bolshevik* indicated that they did not. Its author was none other than Colonel E. Razin, whom Stalin had chosen on other occasions as mouthpiece for his own views about military questions. The colonel affirmed that "separation of politics and strategy, and the neglect of the requirements of politics for 'purely stra-

<sup>21</sup> Woodward, *British Foreign Policy*, 2: 278-80.

<sup>22</sup> Feis, *Churchill, Roosevelt, Stalin*, 64-69.

<sup>23</sup> See Robert H. McNeal, "Roosevelt through Stalin's Spectacles," *International Journal*, 18 (1962-63): 194-206.

<sup>24</sup> E. A. Boltin, "Die Wesenszüge der sowjetischen Strategie in der Endphase des Grossen Vaterländischen Krieges," in Bernhard Weissel, ed., *Befreiung und Neubeginn* (Berlin, 1968), 65.

tegic' reasons are fraught with dangerous consequences. . . . Politics and war influence each other but they are not factors of the same order; primacy always belongs to politics."<sup>25</sup>

The obstacles to a negotiated settlement between the Soviet Union and Germany had diminished by early 1943. Although the passions aroused during the savage struggle ruled out genuine reconciliation, the enormous exertion of both belligerents was conducive to comparing the assets of imperfect peace with the liabilities of elusive victory. Aware of the opportunities, prospective mediators were readily available. Japanese diplomats repeatedly tendered their good services.<sup>26</sup> Mussolini, himself in danger of an imminent invasion by the Western Allies, implored Hitler to seek peace with the Russians.<sup>27</sup> Among the neutral countries Sweden offered a particularly convenient ambiance for preliminaries. Stockholm, easily accessible from both Moscow and Berlin, was a capital where the members of the opposing coalitions maintained extensive diplomatic and intelligence staffs.

The Soviet legation in Stockholm was headed by the colorful figure of Alexandra M. Kollontay—the aristocratic daughter of a tsarist general, a veteran Bolshevik intellectual of nonconformist leanings, and in her younger days the prophet and practitioner of unconventional views on love, sex, and family.<sup>28</sup> In fact these views, rather than diplomatic skill, had qualified her for the foreign service in the early years after the Revolution, when the Bolsheviks delighted in actions *pour épater les bourgeois*. Kollontay was given to emotions in politics and did not hide her Germanophobia even during the period of the official friendship from 1939 to 1941. By 1943 she had reached the age of seventy and was suffering from a heart ailment that confined her to a sanitarium away from the Swedish capital for most of the year. For a historian searching for clues of a Russian-German rapprochement, Kollontay is a great disappointment—a less effective envoy at the sensitive Stockholm post can hardly be imagined. But Stalin's highest-ranking diplomats seldom performed the most important missions. In Sweden, too, officials less colorful but more professional than Kollontay actually ran the legation. Among them was counselor Vladimir S. Semyonov, a specialist in German affairs, whose later career included such elevated functions as chief political adviser of the military government in occupied Germany and eventually deputy foreign minister. Another important official in Stockholm was Boris Yartsev, an expert on Finland, who is on record for having extended peace feelers from Stockholm to Helsinki in

<sup>25</sup> E. Razin, "Lenin o sushchnosti voyny" (Lenin on the Nature of War), *Bolshevik*, 19 (1943): no. 1, p. 47.

<sup>26</sup> Karl Ott to German foreign office, Mar. 3, 1942; AA, microfilm 39, frames 32999–33002; note on conversation between Ushida and German confidant, Oct. 10, 1942, *ibid.*, frames 33417–18.

<sup>27</sup> See F. W. Deakin, *The Brutal Friendship* (New York, 1962), 84–88, 93–94, 102–03.

<sup>28</sup> There is a brief, semiofficial biography of Alexandra M. Kollontay by Anna M. Itkina, *Revoliutsioner, Tribun, Diplomat* (Revolutionary, Tribune, Diplomat) (Moscow, 1964), and a sentimental account by Kollontay's friend, Isabel de Palencia, *Alexandra Kollontay: Ambassadors from Russia* (New York, 1947).

late 1942.<sup>29</sup> Both Semyonov and Yartsev supposedly maintained a very close relationship with Stalin's security chief, Lavrenti P. Beria, who was known as an advocate of partnership with Germany.<sup>30</sup>

The German personnel in Stockholm were of equally high caliber, and their selection could be interpreted as a sign of Berlin's interest in a diplomatic settlement of the war. The envoy to Sweden was Hans Thomsen, who had last served as chargé d'affaires in Washington. In addition to Thomsen, in early 1943 Ribbentrop dispatched several respectable diplomats of conservative leanings to other important neutral capitals such as Madrid and Tokyo.<sup>31</sup> The German foreign minister took pride in having achieved the understanding with the Russians in 1939, and, if his postwar statements are to be trusted, he would have liked to see its renewal in 1943.<sup>32</sup> The propaganda minister, Josef Goebbels, was another prominent Nazi favorably disposed to a settlement with Moscow.<sup>33</sup>

Thriving on excitement and lucrative connections, volunteers of sometimes dubious character were at hand in wartime Sweden to facilitate liaison with friend and foe. One such individual was a certain Edgar Clauss, a nondescript businessman of Baltic-German ancestry and a temporary resident of Stockholm's Carlton Hotel. He was accompanied by a Swedish lady whom he promised to marry after his allegedly forthcoming but in fact fictitious appointment as German minister in Stockholm. Local Germans thought that he was "either a braggart or a spy," and they warned Thomsen about him.<sup>34</sup> As a matter of fact, Clauss was both. Any further inquiry into Clauss by the German legation ceased after German intelligence authorities affirmed that Clauss's activities, which were not specified, served the interests of the Reich.<sup>35</sup> But Clauss had evidently more than one iron in the fire. He is the key person in the memoirs of Peter Kleist, who used to travel frequently between Berlin and Stockholm during the war. According to Kleist it was Clauss who told him, on December 14, 1942, that the Russians wanted a separate peace. Alluding to information from the Soviet legation, Clauss said that Moscow was ready to sign an armistice in eight days if only Berlin would respond favorably.<sup>36</sup> Although the accuracy of

<sup>29</sup> Note on conversation between Ernst von Weizsäcker and Finnish minister to Berlin, Nov. 11, 1942, AA, microfilm 39, frame 33442.

<sup>30</sup> Meissner, *Russland, die Westmächte und Deutschland*, 14; James E. McSherry, *Stalin, Hitler and Europe*, 2 (New York, 1970): 17.

<sup>31</sup> Maxime Mourin, *Les tentatives de paix dans la Seconde Guerre Mondiale* (Paris, 1949), 144-45.

<sup>32</sup> Joachim von Ribbentrop, *The Ribbentrop Memoirs*, tr. O. Watson (London, 1954), 170.

<sup>33</sup> Wilfred von Oven, *Mit Goebbels bis zum Ende* (Buenos Aires, 1949), 97-98, 157-59; see also n. 80.

<sup>34</sup> Schönwald to German consulate in Stockholm, Mar. 23, 1942, AA, microfilm 719, frames 318871-72.

<sup>35</sup> Referred to in Wied, German legation in Stockholm, to German foreign office, May 6, 1942, *ibid.*, frames 318869-70; and Kramarz, German foreign office, to German legation in Stockholm, June 12, 1942, *ibid.*, microfilm 1801, frame E 034410.

<sup>36</sup> Kleist, *European Tragedy*, 139-41.



this assertion has not been verified, other Soviet actions at that time give it weight.

On November 6, 1942, shortly before the Red Army encircled the Germans at Stalingrad, Stalin declared in a public speech that "it is not our aim to destroy all organized military force in Germany, for every literate person will understand that this is not only impossible . . . but . . . also inexpedient from the point of view of the victor."<sup>37</sup> Stalin had already, on February 23, 1942, publicly refused "to identify Hitler's clique with the German people," suggesting "that Hitlers come and go, but the German people and the German state remain."<sup>38</sup> But now he was more specific than ever before in offering friendship to the German military, the caste from which had been recruited so many prominent Russophiles in the past. Following the Treaty of Rapallo in 1922, the German military had been the chief instigators of the deals behind the scenes that enabled them to bypass the restrictions of Versailles by secretly rearming in Russian territory and enabled the Soviet Union in return to obtain German technical assistance. In 1943 the German military were more likely than other Germans to perceive that Hitler's war was lost and to seek salvation in an understanding with Moscow in the spirit of Rapallo.

Shortly after Stalin's address of November 1942 the pattern of Moscow's daily propaganda directed at Germany changed. Until late 1942 the Soviet-sponsored "German People's Radio" had urged in crude Marxist terms that the oppressed German masses intensify the class struggle against their Fascist-capitalist masters.<sup>39</sup> By January 1943 a more sophisticated theme had been introduced. The broadcasts began to report a powerful peace movement among Germans regardless of class and political conviction.<sup>40</sup> On December 6, 1942, a clandestine conference of these Germans for peace was supposed to have met in the Rhineland, and early the following year its proceedings appeared in print in Moscow.<sup>41</sup> Included were speeches by anonymous delegates ranging from Social Democrats to "Christians," organized labor to entrepreneurs, Communists to disillusioned ex-Nazis. But except for sources of Soviet origin, there is no evidence that the meeting ever took place;<sup>42</sup> in that sense at least, the "peace movement" expressed more the Russian than the German desire for peace.

The Stockholm episode reported by Kleist, Stalin's overture to the putative military opponents of Hitler, and the promotion of the imaginary

<sup>37</sup> Stalin, "Twenty-Fifth Anniversary of the Great October Socialist Revolution," Nov. 6, 1942, in Andrew Rothstein, ed. and tr., *Soviet Foreign Policy during the Patriotic War: Documents and Materials* (London, 1944), 1: 49.

<sup>38</sup> Stalin, "Order of the Day of the People's Commissar for Defense," Feb. 23, 1942, in *ibid.*, 37.

<sup>39</sup> For example, "Appell an das deutsche Volk, unterzeichnet von 158 deutschen Soldaten," in *Sie kämpften für Deutschland* (Berlin, 1959), 114-21; and similar appeals in *Pravda*, Jan. 30, Mar. 19, 1942.

<sup>40</sup> Report no. A-12777, Oct. 9, 1943, 45621 C, OSS.

<sup>41</sup> *Beratung der nationalen Friedensbewegung in Deutschland* (Moscow, 1943), 12-39.

<sup>42</sup> See Horst G. Duhnke, "German Communism in the Nazi Era" (Ph.D. dissertation, University of California, Berkeley, 1964), 585-86.

peace front were characteristic products of the peculiar atmosphere created by Stalingrad. They were suggestive of three possible courses that the change of military fortunes opened to the Russians. The first was accommodation with the current Nazi leadership. The second entailed partnership with a conservative German regime dominated by the army. And the third envisaged friendship with a Germany governed by a coalition reminiscent of a popular front but extended to include a wide sampling from the Center and the Right. In due course the Soviet Union was to explore all three of these options.

STALINGRAD INAUGURATED the most intriguing period of Soviet wartime diplomacy. The great battle brought a crushing defeat to Hitler but did not strengthen the solidarity of his enemies. In fact the Russians began almost immediately to put that solidarity to test. They harassed the British personnel attached to the arms convoys, interfering with their navigation and subjecting them to various indignities.<sup>43</sup> The official Soviet press was conspicuously deficient in acknowledging American lend-lease deliveries, a deficiency that prompted public criticism by the United States ambassador, William H. Standley, and an angry Russian reaction in return.<sup>44</sup> Most important, in both his public and his confidential statements Stalin indicated doubts about the suitability of the Allied coalition as a vehicle for his interests.

On February 23, 1943, Stalin's address on the Day of the Red Army did not even mention the Allies.<sup>45</sup> It presented the war as an exclusively Soviet-German affair. Stalin's words were not calculated to reassure Western military planners, many of whom feared that the Russians would stop fighting once they reached their prewar frontiers. Indeed the sharp curtailment of offensive operations by the Red Army on March 15, 1943, tended to confirm rather than to destroy that disturbing hypothesis.<sup>46</sup> The unprecedented calm that afterwards prevailed on the battlefields of the eastern front was perhaps justified by the great exertions of the recent months. But the beginning of the calm also coincided to a day with Stalin's "most emphatic warnings" in a reply to Churchill's notification of further obstacles to an early landing in Europe. The message from Moscow referred to "the great danger with which further delay in opening a second front in France

<sup>43</sup> S. W. Roskill, *The War at Sea, 1939-45*, 2 (London, 1950): 400-01; Woodward, *British Foreign Policy*, 2: 566.

<sup>44</sup> William H. Standley and Arthur A. Ageton, *Admiral Ambassador to Russia* (Chicago, 1955), 341-42.

<sup>45</sup> Stalin, "Order of the Day of the Red Army," Feb. 23, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 53-57. See also Charles Bohlen, memorandum, Feb. 23, 1943, and Standley to Secretary of State, Feb. 24, 1943, *FRUS, 1943*, 3: *The British Commonwealth, Eastern Europe, the Far East* (Washington, 1963), 506-09.

<sup>46</sup> See Earl F. Ziemke, *Stalingrad to Berlin: The German Defeat in the East* (Washington, 1968), 118, 128-31.

is fraught."<sup>47</sup> In his efforts to accelerate the opening of the second front Stalin had issued similar warnings before. But this time the military lull, as well as the intensive peace propaganda on the "German People's Radio," gave his words a special air of urgency. The Soviet Union was demonstrating that its attitude toward Germany was not the same as that of the Western Allies. In particular it was scrupulously avoiding any association with the demand for unconditional surrender that Roosevelt and Churchill had enunciated during their conference at Casablanca in January 1943.<sup>48</sup>

So disconcerting was the behavior of the Russians that at the end of February the British government instructed its ambassador to Moscow, Sir Archibald Clark-Kerr, to inquire on the spot what Soviet intentions with Germany really were. The envoy contacted both Molotov and Stalin, but the "reply was not in very friendly terms."<sup>49</sup> The Soviet leaders evidently did not wish to prejudice their own course of action. For the moment there were still too many open questions that only time could sort out. But having had more firsthand experience with the power and determination of the enemy than the British and Americans, the Russians definitely entertained fewer illusions about the feasibility of a permanent suppression of Germany.<sup>50</sup> They preferred an indirect solution to their security problem by seeking a zone of dependent states as protection against any future attack. Poland, not Germany, was therefore the most urgent item on their agenda.

The dramatic deterioration of relations between Moscow and the Polish government in exile during the first four months of 1943 was more of Russian than of Polish making. The Soviet Union took the first decisive steps after reiterating its claims to the disputed borderland. On February 16 the Soviet deputy foreign commissar, Alexander E. Korneichuk, took a strong public stand against the Polish representatives in London because of their unwillingness to grant those claims.<sup>51</sup> And on March 1 the Russians launched the Union of Polish Patriots, an organization that could later serve as the nucleus of a puppet government.

These Soviet actions predictably caused distress in Washington and Lon-

<sup>47</sup> Churchill to Stalin, Mar. 11, 1943; Stalin to Churchill, Mar. 15, 1943, Ministry of Foreign Affairs of the U.S.S.R., ed. and tr., *Stalin's Correspondence with Churchill and Attlee* (New York, 1965), 106, 99-102.

<sup>48</sup> See Anne Armstrong, *Unconditional Surrender* (New Brunswick, 1961), 55-58; and John L. Chase, "Unconditional Surrender Reconsidered," *Political Science Quarterly*, 70 (1955): 258-79.

<sup>49</sup> Referred to in Woodward, *British Foreign Policy*, 2: 552.

<sup>50</sup> There is still no satisfactory study about the development of the wartime Soviet policy on Germany. In addition to Duhnke's dissertation on "German Communism in the Nazi Era," the most useful studies are Karl-Heinz Ruffmann, "Das Gewicht Deutschlands in der sowjetischen Aussenpolitik bis zum Ende des Zweiten Weltkrieges," *Aus Politik und Zeitgeschichte* (1970), no. 2, pp. 3-18; Alexander Fischer, "Antifaschismus und Demokratie: Zur Deutschlandplanung der UdSSR in den Jahren 1943-1945," in *Potsdam und die deutsche Frage* (Cologne, 1970), 6-33; and Ernst Deuerlein, "Das Problem der 'Behandlung Deutschlands': Umriss eines Schlagwortes des Epochenjahres 1945," *Aus Politik und Zeitgeschichte* (1965), no. 8, pp. 26-46.

<sup>51</sup> Alexander E. Korneichuk, "Vossoedenie ukrainskogo naroda v nedrach svoego gosudarstva" (Reunion of the Ukrainian People in the Bosom of Its State), *Pravda*, Feb. 20, 1943. See also TASS statement, Mar. 3, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 264-65.

don, though not in Berlin. Moscow's insistence upon the frontier that Molotov had fixed with Ribbentrop in 1939 in fact brought back memories to the Germans of their former intimacy with Russia. But so great was the Nazis' intransigence, as well as their lack of subtlety, that they disregarded these friendly allusions. They rather chose to inflict upon the Soviet Union a major irritation. On April 13 they announced the discovery at Katyn of mass graves of murdered Polish officers and accused Moscow of responsibility for the crime. Whatever Goebbels's disposition in favor of an understanding with the Russians, he proved unable to resist this exceptional opportunity to embarrass them.

The genuine dismay and studied indignation displayed by the Soviet government, almost certainly responsible for the massacre, are hardly surprising.<sup>52</sup> More suggestive are the limits and the direction of Soviet anger. In an initial impulse to gloss over the German indiscretion the official Russian news bulletin hinted at the rather improbable explanation that the Nazis might have mistaken graves for archeological excavations.<sup>53</sup> Only when the Germans continued to insist upon their version of what had happened at Katyn did Moscow accuse them, without reservation, of the killings. At the same time, however, the Soviet government tried to divert attention from the merit of the German case in a fashion that could only give additional comfort to the Nazis.<sup>54</sup> After the Poles had proposed investigation by the International Red Cross, a proposal that Berlin promptly implemented, Moscow added insult to injury by accusing the Poles of collusion with Hitler. Worse still, the Soviet Union on April 26 broke off diplomatic relations with Poland, thus bringing the crisis to a climax.

Stalin's handling of the Katyn affair did not bar a rapprochement with Germany and may have actually served to facilitate it. There are indications of secret approaches at the very time the two belligerents were exchanging public insults. The information available is independent of that from Kleist, who did not happen to be in Stockholm in that particular period. A document in the United States Army Intelligence files mentions a communication in mid-April from Moscow to the French Communists, alerting them to be prepared for possible armistice talks.<sup>55</sup> And according to Swedish informants of the American Office of Strategic Services, such talks took place shortly thereafter.<sup>56</sup> In the latter part of April a Swede with connections at the Russian legation was said to have arranged a meeting of

<sup>52</sup> Janusz K. Zawodny summarizes the evidence in *Death in the Forest* (Notre Dame, 1962), 77-99.

<sup>53</sup> Bulletin of the Soviet radio, Apr. 15, 1943, in *Zbrodnia katyńska w świetle dokumentów* (The Katyn Crime in the Light of Documents) (London, 1948), 104.

<sup>54</sup> "Polskie sotrudniki Gitlera" (Polish Collaborators of Hitler), *Pravda*, Apr. 19, 1943.

<sup>55</sup> OSS report no. A-5094, May 11, 1943, USSR 3700, G-2. See also "The Problem of a Separate Peace between Germany and Russia," enclosed in A. J. Drexel Biddle to Secretary of State, June 26, 1943, 740.00119 EW/1530, DS.

<sup>56</sup> OSS report no. A-9469, Aug. 9, 1943, USSR 6900, G-2. See also Polish intelligence report no. 1297, early May 1943, *ibid.*

diplomats at a country estate about thirty kilometers outside of Stockholm. The participants included three unnamed German officials and, from the Soviet side, Mikhail Nikitin, Alexei Taradin, and Boris Yartsev. At one time the respective ministers, Thomsen and Kollontay, were said to have joined the discussions, which lasted for several days but did not lead to an agreement. The Germans were supposedly ready to make peace in return for a satellite Ukraine and for economic concessions in other parts of the Soviet Union, whereas the Russians insisted upon the frontier of 1941.<sup>57</sup>

This intelligence report deserves respect as the most specific contemporary piece of information about the peace feelers. Although there is no way of checking the accuracy of its details, it is quite probable that informal exchanges occurred at Soviet initiative. The pattern of Moscow's policy after Stalingrad supports rather than contradicts this speculation. It was in the spring of 1943, if ever during the war, that the time was ripe for Stalin to explore whether Berlin would be prepared to settle for what his Western allies had been so reluctant to grant him—the confirmation of the Soviet frontier of 1941. But the attitude of the Germans was altogether different from what it had been in August 1939, and they gave no sign of any willingness to restore the frontier they had violated in June 1941.

The Russian participants reportedly broke off the negotiations in Sweden at the beginning of May. In his Order of the Day on May 1 Stalin had publicly assumed a position on the subject of separate peace. He attributed the desire for a separate peace to the Nazis, who "judge their adversaries by their own standards of treachery."<sup>58</sup> He also affirmed that only unconditional surrender of the enemy could end the war, although he did not relate this demand to the Casablanca statement by Roosevelt and Churchill. Stalin's strong language suggests that he may have given up any hope of persuading the obstinate Nazis that a compromise was not only in his interest but, needless to say, also in theirs.

But there is another possible interpretation of what Stalin was saying. His statement could have been meant as an encouragement for the renewal of talks on terms more acceptable to him. The triangular nature of the Soviet-Western-German relationship was obvious, and he could alternately show a friendly face to one side or the other. His most convincing argument would have been to demonstrate to the Germans that he could obtain what he wanted from the West. From this point of view, the rather meek reaction of the British and Americans to the Katyń incident gave the impression that Stalin might be able to repair his relations with

<sup>57</sup> These terms are not mentioned specifically in relation to the April talks, but they recur in intelligence reports from late 1942 to the fall of 1943. See, for example, London SI report, Nov. 3, 1942, USSR 3850, G-2; OSS report no. A-1820, Feb. 5, 1943, USSR 6900, G-2. See also Huene, Lisbon, to German foreign office, Apr. 6, 1943, AA, microfilm 39, frames 33667-68; OSS report no. A-13022-b, Oct. 13, 1943, USSR 6900, G-2; and Kleist, *European Tragedy*, 140.

<sup>58</sup> Stalin, "Order of the Day of the Supreme Commander-in-Chief," May 1, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 58.

his coalition partners whenever he wanted. They had given proof of an overwhelming desire to maintain at least the appearances of Allied solidarity. And it was the appearance, though not the substance, of this solidarity that the Russians proceeded to promote during the next few weeks.

STALIN'S ADDRESS of May 1, 1943, heralded a conspicuous improvement in official Soviet behavior toward Great Britain and the United States, a difference the more striking since no particular change in the conduct of the Western Allies had prompted it. Alexander Werth, who lived in Moscow as correspondent for the *Sunday Times*, later recalled "the record warmth vis-à-vis Britain and America in May and June 1943."<sup>59</sup> The official press and radio prominently commemorated the anniversaries of the alliance agreements of 1942 and extolled the Allied operations in North Africa, a battlefield previously denigrated as insignificant.<sup>60</sup> Russian editorial writers ridiculed the idea of a separate peace and seconded Stalin's call for the unconditional surrender of Germany. And on May 23 the dissolution of the Comintern generated further good feelings in the West.

Yet no practical steps toward closer collaboration accompanied these friendly gestures. In particular, Stalin remained aloof to Roosevelt's urgent pleas for a tripartite summit conference that would clarify mutual war and peace goals.<sup>61</sup> It is difficult to judge to what extent Stalin's aloofness was the result of premeditation rather than of a genuine need to keep a close eye on the developments at the front—the explanation he gave to the president. As Western observers in Moscow noticed, the Russians were at that time especially nervous about the intentions of Germany.<sup>62</sup> On the one hand, the inexplicable delay of the anticipated and dreaded German summer offensive could mean that Berlin might be contemplating negotiations after all. On the other hand, however, as long as there was no positive evidence of such a readiness, Stalin had to assume that the offensive would eventually come and that he would need whatever relief his allies could give him. Whether to prompt them into action or simply to cheer up the Soviet people, the official press systematically encouraged the belief that now, after the victory in Africa, the second front was imminent.

It is against this background of inflated Russian hopes that the impact of yet another message about a delay of the cross-Channel invasion must be measured. On June 4 Roosevelt and Churchill notified their Soviet

<sup>59</sup> Alexander Werth, *Russia at War, 1941-1945* (New York, 1964), 671-72.

<sup>60</sup> "Godovshchina sovetско-angliiskogo soiuзнogo dogovora" (The Anniversary of the Soviet-English Treaty of Alliance), *Pravda*, May 26, 1943; "Koalitsiia rozhdenaia voinoi i prizvannaia obespechit pobedonosnyi mir" (A Coalition Born of War and Destined to Secure a Victorious Peace), *Voina i rabochii klass*, June 15, 1943, pp. 3-9.

<sup>61</sup> Roosevelt to Stalin, May 5, 1943; Stalin to Roosevelt, May 26, 1943, Ministry of Foreign Affairs of the U.S.S.R., ed. and tr., *Stalin's Correspondence with Roosevelt and Truman* (New York, 1965), 63-64, 66.

<sup>62</sup> Werth, *Russia at War*, 674-76.

colleague that the operation could not take place before May 1944.<sup>63</sup> Stalin's fury at this new apparent procrastination at a time so critical for him is easy to imagine and justify. But as during the Katyn crisis, his reaction was reasoned rather than impulsive. He waited for a whole week, undoubtedly weighing alternative responses and considering their probable effect. In the end he expressed his intense displeasure not only in a secret message but also through an important public gesture. He recalled almost simultaneously the reputedly pro-Western ambassadors Ivan M. Maisky and Maxim Litvinov from their respective posts in London and Washington.

By mid-June, then, the brief period of Moscow's official amity with the West was over. There is no doubt that the Roosevelt-Churchill action—or rather inaction—about the second front had preconditioned the new crisis, but it was Stalin's public behavior that actually created it. He could hardly have failed to take into account the delight that evidence of Allied discord would inevitably produce in Berlin, and he may have aimed at that very effect. In any case, he again set the stage for a rapprochement with Germany, and there are indications of Russian attempts to promote it in the second half of June. On June 16 the Swedish newspaper *Nya Dagligt Allehanda* announced in a special edition that high Soviet and German officials had been negotiating near Stockholm.<sup>64</sup> An American intelligence report identified them as Mikhail Nikitin of the Russian legation and Paul Schmidt of the press and information section of the German foreign office, and the seaside resort of Saltsjöbaden was cited as their meeting place.<sup>65</sup> The British government, too, received information from the Swedish capital that Schmidt had met with two members of the Soviet legation in a private house and that Yartsev played the key role in the contact.<sup>66</sup> According to Kleist's somewhat different account, A. M. Alexandrov, formerly counselor at the Soviet Embassy in Berlin and head of the foreign commissariat's European division, arrived in Stockholm en route to London and tried to contact him through Clauss.<sup>67</sup> The German minister, Thomsen, also heard that the Russian diplomat was in town and that he wanted "to meet with a gentleman from the German foreign service with whom he was acquainted."<sup>68</sup>

<sup>63</sup> Roosevelt to Stalin, received June 4, 1943; Stalin to Roosevelt, June 11, 1943, *Stalin's Correspondence with Roosevelt and Truman*, 67–71.

<sup>64</sup> Reported in *New York Times*, June 17, 1943. A slightly different version appeared in the Geneva newspaper *La Suisse* on Oct. 2, 1943.

<sup>65</sup> OSS report no. A-9647, Aug. 9, 1943, USSR 6900, G-2; Herschel Johnson, Stockholm, to Secretary of State, June 17, 1943, *ibid.*

<sup>66</sup> George Wiskeman to Christopher Warner, British Foreign Office, Aug. 11, 23, 1943, N 4898/66/38, FO 371/36956.

<sup>67</sup> Kleist, *European Tragedy*, 144–50.

<sup>68</sup> Thomsen, Stockholm, to German foreign office, June 21, 1943, AA, microfilm 191, frames 144408–09. Basil H. Liddell-Hart, the noted British military historian who had interrogated captured German officers, claimed in his posthumously published book that negotiators, including even Ribbentrop and Molotov, had met behind the German lines near Kirovograd in June 1943. *History of the Second World War* (New York, 1970), 488. Because of the complete lack of supporting evidence, however, this allegation deserves little credence.

After the war this incident received a retrospective publicity in the American press. In July 1947 an article in the New York magazine *Liberty* added further sensational details, which the authors claimed they had acquired during "clandestine conferences with Europeans whose lives depend on anonymity."<sup>69</sup> It is interesting that the Soviet government considered this rather shabby piece of writing worthy of a prompt response. No one less authoritative than Kollontay herself was commissioned to refute the allegations. In a column printed in *Izvestiia* she concentrated upon a single flaw in the *Liberty* article: Alexandrov could not possibly have been in Stockholm at the critical time because he was serving with the Soviet legation in Australia.<sup>70</sup> But the Russian sensitivity about the subject enhances rather than reduces the possibility that Alexandrov's colleagues from the foreign commissariat had indeed tried to extend feelers in Stockholm, even though an early indiscretion apparently prevented actual contacts. Such a speculation would also explain why both the Soviet and the German governments had denied the original Swedish newspaper story so vehemently in press communiqués issued on the same day, June 18, 1943.<sup>71</sup>

But even after the embarrassment in June Moscow did not abstain from publicizing ideas suggestive of a preference for compromise over struggle to the bitter end. On June 22, 1943, the official information agency Sovinformburo commented ambiguously that "without a second front victory over Germany is impossible."<sup>72</sup> On July 1 an authoritative article in the ideological journal *Voina i rabochii klass* castigated Western projects for the postwar punishment of the Germans.<sup>73</sup> Its author, identified as N. Malinin, derided the theory about the collective guilt of the German people and expressed misgivings about subjecting them to reparations or even to a military occupation. Of all Soviet wartime statements this article went the farthest in hinting that the Germans might be allowed to keep some of their conquests. Questioning not only Polish but also Czechoslovak aspirations for territories that were currently part of the Reich, the author implied that even the return of the Sudetenland to Czechoslovakia was not imperative. Exactly to whom Moscow intended to address these extraordinary allurements is not altogether clear. Stalin may still have hoped that the Nazis could be persuaded to negotiate, but if he did, the advent of the German offensive on July 5 all but destroyed any such hopes. It is more likely that the article foreshadowed a change of policy that had been under preparation ever since the demise of the Comintern and for which

<sup>69</sup> Paul Schwarz and Guy Richards, "A Secret Russian Mission That Almost Changed History," *Liberty*, July 5, 1947, p. 26. See also similar articles by Donald B. Sanders [pseud.], "Stalin Plotted a Separate Peace," *American Mercury*, Nov. 1947, pp. 519-27; and Robert M. W. Kempner, "Stalin's 'Separate Peace' in 1943," *United Nations World*, 4 (1950): no. 3, pp. 7-9.

<sup>70</sup> *Izvestiia*, July 29, 1947.

<sup>71</sup> TASS statement, June 18, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 269; New York *Times*, June 18, 1943.

<sup>72</sup> "Dva goda Otechestvennoi voiny Sovetskogo soiuza" (Two Years of the Patriotic War of the Soviet Union), *Pravda*, June 22, 1943.

<sup>73</sup> N. Malinin, "O tseliakh voiny" (Concerning the War Aims), *Voina i rabochii klass*, July 1, 1943, pp. 11-15.



the enemy attack provided the appropriate moment.<sup>74</sup> The formation of the Free Germany Committee on July 12 marked the beginning of the new Soviet strategy.<sup>75</sup>

Retrospective interpretations have tended to obscure the original meaning of this remarkable enterprise. Both friendly and hostile commentators have been inclined to regard the formation of the Free Germany Committee as the first step on the path that ended with the establishment of the German Communist satellite state in 1949.<sup>76</sup> But evidence that the Russians anticipated this outcome in 1943 is yet to be presented. Most contemporaries understood Russian sponsorship of the committee as a bid for partnership with German conservatives, and the Soviet Union did nothing to discourage this interpretation.<sup>77</sup> Although Communist émigrés predictably dominated the committee, its striking feature was the participation of the more moderate patriotic members who had been recruited from among the prisoners of war. Their radio appeals, urging the German army to end the war by overthrowing Hitler and withdrawing to the present frontier of the Reich, implied that this relatively modest concession would make possible a fair peace. Whether or not the Russians would have honored this implicit promise is impossible to judge. Yet by having given their blessing to the Free Germany movement they created an obstacle to a rapprochement with Hitler; they invited an understanding instead with his potential successors.

THE SOVIET VICTORY in the battle of Kursk, which ended on July 16, represents in many ways a watershed more important than Stalingrad in the history of the war. The first summertime triumph of Russian arms, it opened the perspective of their continuous and irreversible, though still far from easy, advance to the west. The Germans saw the handwriting on the wall, and doubts about the wisdom of fighting on extended this time to the highest places. Various Nazi officials, acting on their own, tried to find out about Moscow's possible terms for peace. On August 2-5, 1943, Ribbentrop's assistant Rudolf Likus went to Stockholm in order to gather information.<sup>78</sup> Two weeks later the German foreign minister sum-

<sup>74</sup> Communist Party of Germany, Directive of May 27, 1943, referred to in Bruno Löwel, "Die Gründung des NKFD im Lichte der Entwicklung der Strategie und Taktik der KPD," *Beiträge zur Geschichte der deutschen Arbeiterbewegung*, 5 (1963): 618-19.

<sup>75</sup> "Manifest des Nationalkomitees 'Freies Deutschland' an die Wehrmacht und an das deutsche Volk," July 12-13, 1943, in *Sie kämpften für Deutschland*, 146-51. The best history of the committee is Bodo Scheurig, *Free Germany* (Middletown, Conn., 1970).

<sup>76</sup> Representative samples of the opposing views are Gerhard Rossmann, "Die Entwicklung der Vorstellungen der KPD über den neuen Staat," *Beiträge zur Geschichte der deutschen Arbeiterbewegung*, 9 (1969): special no., pp. 145-62; and Peter Strassner, *Verräter: Das Nationalkomitee "Freies Deutschland"—Keimzelle der sogenannten DDR* (Munich, 1960).

<sup>77</sup> One of the best contemporary assessments is Arnold Wolfers, "Soviet Policy toward Germany," R & A Branch, 66834 R, OSS. For a different view, see the article by German émigré Egon Kötting [Eugen Westphal], "Nationalbolsjevism—Tysklands Framtid?" (National Bolshevism as Germany's Future?), *Svensk Tidskrift*, 30 (1943): 496-505.

<sup>78</sup> Rudolf Likus, note, Aug. 9, 1943, records of the German foreign office, AA, microfilm 162, frames 130779-80. See also Walter Schellenberg, *The Labyrinth* (New York, 1956), 370.

moned Kleist and instructed him to restore contact with Clauss.<sup>79</sup> In September Goebbels told Hitler that "we must come to an arrangement with one side or the other," and the Führer replied that "he would prefer negotiations with Stalin," although Hitler did not "believe they would be successful."<sup>80</sup>

All these half-hearted peace stirrings on the Nazi side ended in the planning stage. They sufficed, however, to cause grave concern among the men around Karl Goerdeler who hoped to extricate Germany from the war by getting rid of Hitler. One of the conspirators, Ulrich von Hassell, noted in his diary on August 15, 1943:

If Hitler comes to terms with Stalin, the resultant disaster cannot be imagined. It would be different with a decent, self-respecting Germany. This Germany would have to exploit all opportunities. There is only one expedient left—to make either Russia or the Anglo-Americans understand their interest in a sound Germany.<sup>81</sup>

Hassell, like most of his friends, would have preferred settlement with the Western powers if only they would modify their stiff demand for unconditional surrender. But at least a few members of the conspiracy favored approaches to the Soviet Union, perhaps because they hoped to find it more responsive or because they considered communism the wave of the future. In the late summer of 1943 Friedrich von der Schulenburg, the former ambassador to Russia, contemplated a secret mission to Moscow by breaking through the front lines.<sup>82</sup> There is also evidence, however inconclusive, that communist sympathies may have motivated the would-be assassin of Hitler, Claus von Stauffenberg.<sup>83</sup>

Did the Russians know about the plot against Hitler and attune their policies to the possibility of its success? From what is known about their intelligence network, it seems that until 1942 they had been informed quite accurately about what was happening in Germany. Their main source of information had been the famous espionage organization known as the Red Orchestra.<sup>84</sup> Although the standard East German book about the Red Orchestra claims that its members knew of the activities of Goerdeler's followers, it offers no specific evidence in support of this claim.<sup>85</sup> In

<sup>79</sup> Kleist, *European Tragedy*, 154–56.

<sup>80</sup> Goebbels, diary entry for Sept. 23, 1943, *The Goebbels Diaries*, tr. L. P. Lochner (Garden City, 1948), 477.

<sup>81</sup> Hassell, diary entry for Aug. 15, 1943, *The Von Hassell Diaries*, tr. H. Gibson (Garden City, 1947), 315.

<sup>82</sup> Ernst Kaltenbrunner to Martin Bormann, Aug. 28, 1944, in Kaltenbrunner, *Spiegelbild einer Verschwörung* (Stuttgart, 1961), 308–09. See also Hassell, diary entry for Dec. 5, 1943, *Von Hassell Diaries*, 327.

<sup>83</sup> See Hans Bernd Gisevius, *To the Bitter End* (Boston, 1947), 486; and Joachim Kramarz, *Claus Graf Stauffenberg* (Frankfurt, 1965), 175–77, 192–93. See also Hans Dress, "Fortschrittliche und reaktionäre Tendenzen in den Reformplänen des Kreisauer Kreises," in Kommission der Historiker der DDR und UdSSR, ed., *Der deutsche Imperialismus und der zweite Weltkrieg* (Berlin, 1961), 4: 587–606.

<sup>84</sup> The best book on the subject is Heinz Höhne, *Codeword DIREKTOR: The Story of the Red Orchestra* (New York, 1972).

<sup>85</sup> Karl-Heinz Biernat and Luise Kraushaar, *Die Schulze-Boysen/Harnack-Organisation im antifaschistischen Kampf* (Berlin, 1970), 28.

any case the Gestapo had destroyed that principal Soviet source of information by the fall of 1942.<sup>86</sup> In the trial in April 1944 of William Knöchel, a high-ranking German Communist whom Moscow had sent to Germany in January 1942 in order to organize the party underground, the Nazi court noted that before Knöchel's capture later in 1942 he had sent abroad important information about opposition currents in the country.<sup>87</sup> But this information must have been rather unsubstantial since the knowledge of it did not enable the Gestapo to track down the conspiracy.

As for 1943, Russian and East German literature, which normally plays up rather than down all types of clandestine pro-Soviet activities, has recorded only one instance of a line of communication between Berlin and Moscow in existence during that year. The exchanges began in spring, when Soviet confidants in Stockholm established contact, through a Swedish intermediary, with Anton Saefkow, a leading Communist organizer in Berlin. Saefkow is known to have met two associates of Goerdeler in June 1944, and Saefkow was aware of the plot against Hitler's life possibly as early as the end of 1943.<sup>88</sup> Saefkow may have notified the Russians of the plot, although available sources give the impression that little of substance passed through this Swedish channel.<sup>89</sup> Surviving participants in the conspiracy have all denied that any links with Moscow ever existed.<sup>90</sup> It is therefore unlikely that the Soviet anticipation of a revolt in Germany was anything but an intelligent guess, made without the benefit of knowing what was actually under way.

Just how great the Russians thought were the chances of a successful coup against Hitler is difficult to estimate. But its likelihood definitely increased after the Italians had set an example by deposing Mussolini on July 25, 1943. Since the change of government in Italy was accomplished by the ruling clique rather than by the masses, Moscow had a further reason to focus upon fomenting discontent among Germany's upper classes. Shortly after the Italian events the Soviet Union prepared to supplement the Free Germany Committee, on which Walter Ulbricht and his companions loomed perhaps too large, with the more respectable and exclusive League of German Officers. In the meantime the Russian victory at Kursk had convinced so many captured Germans that the war was lost that there was no shortage of those willing to join in this new enterprise.

Wolfgang Leonhard, who worked during the war in the Moscow offices

<sup>86</sup> David J. Dallin, *Soviet Espionage* (New Haven, 1955), 264-65.

<sup>87</sup> Arnold Sywottek, *Deutsche Volksdemokratie: Studien zur politischen Konzeption der KPD 1935-1946* (Düsseldorf, 1971), 118, 245.

<sup>88</sup> Gerhard Rossmann, *Der Kampf der KPD um die Einheit aller Hitlergegner* (Berlin, 1963), 45-47, 195-96, 222, 255; Gertrud Glondajewski and Gerhard Rossmann, "Ein bedeutendes politisches Dokument des illegalen antifaschistischen Kampfes der Kommunistischen Partei Deutschlands," *Beiträge zur Geschichte der deutschen Arbeiterbewegung*, 8 (1966): 646-47; Allen W. Dulles, *Germany's Underground* (New York, 1947), 173-74.

<sup>89</sup> See Rossmann, *Der Kampf der KPD*, 45-47; and Otto Winzer, *Zwölf Jahre Kampf gegen Faschismus und Krieg* (Düsseldorf, 1955), 231.

<sup>90</sup> See Bodo Scheurig, ed., *Verrat hinter Stacheldraht?* (Munich, 1965), 232-35.

of the journal *Freies Deutschland*, later reported that the inauguration of the League of German Officers, set for September 1, had been unexpectedly postponed without a substitute date.<sup>91</sup> A few days later the journal was about to publish a startling editorial entitled "Armistice—the Demand of the Day." According to Leonhard this editorial, which omitted the customary anti-Nazi verbiage, was addressed to the men currently in power in Berlin rather than to their prospective challengers. It was withheld from publication only at the last moment, and the delayed proclamation of the league followed on September 11.<sup>92</sup>

Leonhard's assertion seems to corroborate Kleist's testimony that on September 8 the Russians again attempted to establish contacts in Sweden through Clauss.<sup>93</sup> The intermediary alerted Kleist that Vladimir Dekanozov, the former Soviet ambassador to Berlin and another of Beria's protégés, was going to visit Stockholm and was eager to meet a German negotiator. According to Clauss, Moscow merely awaited a signal from Berlin and was deeply disappointed when none came.

The coincidence of Leonhard's and Kleist's allegations is well enough known and has led to the belief that Stalin may have made a major bid for peace in September rather than in April or June 1943.<sup>94</sup> Such a conclusion is possible but not probable; for aside from the evidence previously presented of Nazi unresponsiveness, Kursk had bolstered the self-confidence of the Russians sufficiently to reduce for them the value of any deal with Hitler. The hypothesis also depends much too heavily on the veracity of the disreputable Clauss, who very likely misled both the German and the Soviet diplomats about the actual extent of his intimacy with each of them.

A more probable explanation of what happened during those first few days of September is that the presence of Ribbentrop's emissary in Stockholm, along with Clauss's grandiloquence, almost convinced the Russians that Berlin had unexpectedly changed its mind about negotiating. Then the article in *Freies Deutschland* would have been a tentative sign of their interest. But the hoax was exposed just in time to spare the Russians the same embarrassment they had experienced in June, when the Swedish newspaper had leaked similar shady dealings. If this was the case, then the Soviet government, though no longer itself trying to extend peace feelers to the present German regime, was in early September 1943 still willing to respond to initiatives by others.

Soon, however, even that willingness disappeared, and the experience with Clauss may have been the last blow. On September 13 Molotov re-

<sup>91</sup> Wolfgang Leonhard, *Child of the Revolution* (Chicago, 1958), 318–20.

<sup>92</sup> Scheurig, *Free Germany*, 65. See also Heinrich von Einsiedel, *I Joined the Russians* (New Haven, 1953), 98–106.

<sup>93</sup> Kleist, *European Tragedy*, 164–71.

<sup>94</sup> See, for example, Minuth, "Sowjetisch-deutsche Friedenskontakte," 38–45; and Lionel Kochan, *The Struggle for Germany* (New York, 1967), 96.

jected a mediation offer by the Japanese ambassador, Naotake Sato, intimating to him that whatever chances there had been of an understanding between Germany and Russia no longer existed. Molotov is quoted as having said: "Under different circumstances, the Soviet government would have considered it its duty to accept the Japanese offer of mediation."<sup>95</sup>

The rules of the diplomatic triangle now required that Stalin should attempt to improve his relations with the British and the Americans. And indeed, from September onwards, he acted to instill more life into the alliance—not only in words but also in deeds. The decisive date was September 8, when, after months of dilatory excuses, he finally consented to a conference with Roosevelt and Churchill and accepted the date they had proposed.<sup>96</sup> A week later Stalin proved that he cared more about winning their confidence than about keeping open any secret channels to the enemy: Andrei Gromyko, chargé d'affaires in Washington, duly reported the Japanese mediation offer to the secretary of state, Cordell Hull.<sup>97</sup> Equally significant, Konstantin Vinogradov of the Soviet legation in Stockholm revealed to an American colleague that "German agents and intermediaries" had recently approached Russian diplomats there—a disclosure in striking contrast to his consistent denials of such approaches in the past.<sup>98</sup>

By the fall of 1943 evidence had mounted that a peace with Hitler was impossible and his replacement by more reasonable men improbable. The Russians nevertheless abstained from endorsing the formula for unconditional surrender until after the Normandy landings in June 1944.<sup>99</sup> As long as the second front was in abeyance they avoided doing anything that would prejudice a separate arrangement with an anti-Nazi regime in case Hitler's enemies would come to power after all. But after September 1943 the Soviet Union no longer went so far as actively to prepare for such an arrangement.

THE OBVIOUS QUESTION to be asked is whether Stalin did not himself try to create a false impression of his readiness to conclude a separate peace merely to make his allies more amenable to his demands. After the war many Western authors replied in the affirmative. General Albert C. Wedemeyer, for example, concluded that the Soviet dictator "was simply holding the idea over our heads and blackmailing us to give him more aid."<sup>100</sup>

<sup>95</sup> Quoted in Toshikazu Kase, *Journey to the Missouri* (New Haven, 1950), 162–63.

<sup>96</sup> Stalin to Roosevelt, Sept. 8, 1943, *Stalin's Correspondence with Roosevelt and Truman*, 90–91.

<sup>97</sup> Gromyko to Hull, Sept. 14, delivered Sept. 16, 1943, *FRUS*, 1943, 3: 696–97.

<sup>98</sup> Johnson, Stockholm, to Secretary of State, Sept. 29, 1943, *ibid.*, 698–99.

<sup>99</sup> After the Russian delegates had used delaying tactics in the European Advisory Commission for six months, they approved its draft instrument for German surrender on July 25, 1944. *FRUS*, 1944, 1: *General* (Washington, 1966), 252–54. The chief Soviet representative, Fedor Gusev, announced the confirmation of the document by his government on August 21, 1944. John G. Winant to Secretary of State, Aug. 21, 1944, *ibid.*, 276.

<sup>100</sup> Albert C. Wedemeyer, *Wedemeyer Reports!* (New York, 1958), 92.

To attribute to the Russians a genuine desire for compromise with anyone would not have been compatible with the belief in their insatiable hunger for power, a belief prevalent during the cold war.

But Stalin's aptitude at deception notwithstanding, his alleged scheme to frighten his coalition partners cannot be reconciled with the evidence. Peace rumors emanated from so many different sources, independent of one another, that they could not have possibly been disseminated from one center.<sup>101</sup> In addition Stalin had strong enough reasons to refrain from mystification: planting false reports about secret dealings with the enemy could only increase the chances that the Western capitalists and their German colleagues might turn the tables on him. Accordingly the Russians coped with the many circulating rumors in a fashion calculated to discourage speculation rather than to encourage it. They ignored those rumors that were too vague or too fantastic to be taken seriously and reacted only to the ones with substance: the three abortive feelers in Stockholm and the Japanese mediation attempts were the incidents that prompted official refutations.<sup>102</sup> Thus the Soviet government tried to divert attention from what had happened rather than attract it to what had not. Those contemporaries who thought that rumors were being deliberately planted suspected a German scheme much more often than a Russian one.<sup>103</sup> But the Nazis, though no doubt eager to sow mistrust among their adversaries, were also apprehensive that such a maneuver might backfire against their own national morale. They therefore frequently denied that any peace efforts were under way, using for that purpose the foreign service of the German radio rather than the domestic news media.<sup>104</sup>

Rumors of peace feelers proliferated throughout the summer and fall of 1943 despite Russian and German efforts to suppress them. They were especially rife in Latin America, where amateurs and imposters, within and without the diplomatic corps, disseminated the most sensational stories. On September 28, 1943, for example, the newspapers in Arequipa, Peru's second largest city, announced that an armistice between Germany and Russia had just been signed.<sup>105</sup> In Switzerland there was much talk about mediation by the Vatican.<sup>106</sup> And various Eastern European sources re-

<sup>101</sup> See memorandum by Harvey H. Smith, Chief of G-2 Central European Branch, Sept. 20, 1943, Germany 6900, G-2.

<sup>102</sup> See nn. 58, 71, 97, 98; and TASS statement, July 17, 1943, in Rothstein, *Soviet Foreign Policy*, 1: 269. A Japanese mediation attempt in June 1943 is mentioned in Ivan Krylov [pseud.], *Soviet Staff Officer* (London, 1951), 241-42.

<sup>103</sup> Navy intelligence report, no. 660-43, Sept. 26, 1943, USSR 6900, G-2; information series no. 103A, Nov. 15, 1943, Germany 6900, *ibid.*

<sup>104</sup> Broadcasting monitoring report, Aug. 21, 1943, USSR 6900, G-2; press survey, "More Rumors on a Russo-German Peace," Oct. 8, 1943, *ibid.*; broadcast summary, Oct. 15, 1943, Germany 6900, *ibid.*

<sup>105</sup> Navy intelligence report, Oct. 15, 1943, Germany 6900, G-2. At the end of September a rumor circulated in Rome that a Soviet-German armistice would be signed on October 15. British Embassy, Bern, to British Foreign Office, Oct. 1, 1943, C 11735/55/18, FO 371/34438.

<sup>106</sup> Leland Harrison, Bern, to Secretary of State, Oct. 11, 1943, *FRUS*, 1943, 3: 708-09; press survey, Nov. 1943, USSR 6900, G-2. See also Bergen, Vatican, to German foreign office, May 4, 1943, AA, microfilm 39, frame 33750.

ported alleged discussions between German and Russian agents in Sofia.<sup>107</sup> No evidence has been found to substantiate any of these reports.

Paradoxically the peace scare reached its climax when the time for a Stalin-Hitler rapprochement had already passed. In August 1943, two months after the second Stockholm episode, Robert E. Sherwood reported that the atmosphere was "alarmingly reminiscent of that which preceded the Molotov-Ribbentrop pact of August 1939."<sup>108</sup> Although the Foreign Office in London received fewer reports about peace feelers in 1943 than they had in 1942, and although they discounted them as unfounded,<sup>109</sup> government officials in Washington were worried. They anxiously discussed memorandums such as "The Proper Course of Action for the United States in the Event Russia and Germany Effect a Compromise Peace."<sup>110</sup> In October Roosevelt's chief adviser, Harry Hopkins, publicly admitted that possibility and proceeded to reassure the American people. He wrote in the *American Magazine*: "Russia, the keystone of the war, is still fighting grimly. If we lose her, I do not believe for a moment that we will lose the war, but I would change my prediction about the time of victory."<sup>111</sup>

The presupposition that the Soviet Union had been so weakened by the war that it might seek peace because of the high price of victory was somewhat incongruously blended with the belief that her "postwar position in Europe will be a dominant one" and that "every effort must be made to obtain her friendship."<sup>112</sup> The two contradictory assumptions helped to shape the Western conviction about the necessity of concessions with respect to a postwar Russian sphere of influence in Europe. Such was the situation on the eve of the critical conferences scheduled to meet in late 1943: the conference at Moscow of foreign ministers in October and the meeting at Teheran of the Big Three a month later.

Their recent military successes notwithstanding, the Russians approached the Moscow conference from a position of weakness rather than of strength. They placed on the agenda only one item, and this item was suggestive of their uppermost priority: how to hasten the end of the war—by launching the Second Front and by other means.<sup>113</sup> Regarding political questions, they had originally wanted the meeting to be "only of

<sup>107</sup> New York Times, Oct. 23, 1943; Laukhuff memorandum on conversation at the State Department with Father Odo, Oct. 19, 1943, 740.00119 EW/1934 DS; Wellington, memorandum on conversation at the State Department with Father Odo, Nov. 18, 1943, 740.0011 EW/32140, DS; report no. A-15077, Nov. 16, 1943, OSS.

<sup>108</sup> Robert E. Sherwood, *Roosevelt and Hopkins* (New York, 1948), 734.

<sup>109</sup> Woodward, *British Foreign Policy*, 2: 560.

<sup>110</sup> Maurice Matloff, *Strategic Planning for Coalition Warfare, 1943-1944* (Washington, 1959), 286-87.

<sup>111</sup> Harry Hopkins, "We Can Win in 1945," *American Magazine*, Oct. 1943, p. 100.

<sup>112</sup> "Russia's Position," Aug. 2, 1943, *FRUS: The Conferences at Washington and Quebec, 1943* (Washington, 1970), 625.

<sup>113</sup> "The Consideration of Measures to Shorten the Duration of the War against Hitlerite Germany and Her Allies in Europe," Oct. 19, 1943, *FRUS, 1943, 1: General* (Washington, 1963), 771-72.

preparatory character."<sup>114</sup> But the Soviet delegation did not object to discussing such questions once they were introduced by the British and American representatives. The record of the proceedings shows that the Western delegates were at times more accommodating than the Russian delegates had apparently expected. For example, Molotov was surprised by Eden when he withdrew without debate his previous objections to the planned Soviet-Czechoslovak treaty, which would set a precedent for a client relationship between Moscow and the small nations of East Central Europe.<sup>115</sup> Similarly Secretary of State Hull tried to meet the Russian concern for security with such radical proposals about the suppression of Germany that the foreign commissar was caught unprepared.<sup>116</sup> The Western powers tried to convince Stalin that their friendship promised greater advantages than anything Berlin possibly had to offer. The Russian leader seemed convinced when, on the last day of the conference he walked up to Hull to deny "in the most sarcastic terms . . . reports . . . that the Soviet Union and Germany might agree on peace terms."<sup>117</sup> And before the heads of state met at Teheran four weeks later Stalin made another important gesture to reassure his allies.

On November 12 Molotov handed to the United States ambassador, W. Averell Harriman, a memorandum that, along with the earlier hint by Vinogradov, is the only available document of Soviet origin that directly concerns the happenings in Stockholm.<sup>118</sup> The foreign commissar stated that German agents had recently attempted to establish contact but had been immediately turned away. He mentioned Clauss and Kleist by name—an authoritative confirmation of their roles as intermediaries that bestows a measure of authenticity on Kleist's memoirs. The only discrepancy is in dates: Kleist, as well as Vinogradov, referred to early September, Molotov to mid-October. The different dating may have been necessitated by Kollontay's statement for *Daily Express* on October 3, in which she said, "There have never been any such feelers put by the Germans to my Legation."<sup>119</sup> Too long an interval between the event and its disclosure could also have inspired undesirable queries about the motives for the delay.

As was obviously intended, the message promoted Western trust in the Soviet ally. Though belated and originally unexpected, this was in the last analysis Moscow's most important gain from its peace overtures earlier that year. Illustrative of the extent of the trust was the consent of the United States delegation at Teheran to being housed on the premises of the Soviet Embassy, which was undoubtedly well equipped with listening

<sup>114</sup> Arkadi Sobolev to Eden, Sept. 29, 1943, annex C to memorandum no. 434, CAB 66/41.

<sup>115</sup> Summary of proceedings, session of Oct. 24, 1943, *FRUS*, 1943, 1: 624-27.

<sup>116</sup> Minutes of meeting, Oct. 25, 1943, *ibid.*, 632; see also 720-23.

<sup>117</sup> Hull, memorandum, Oct. 30, 1943, *ibid.*, 687.

<sup>118</sup> Molotov to Harriman, Nov. 12, 1943, *ibid.*, 502-03.

<sup>119</sup> "Interview between Mr. Gordon Young and Mme Kollontay," Press Reading Bureau, Stockholm, to Political Intelligence Department, London, Oct. 7, 1943, N 5967/499/38, FO 371/36992.



devices.<sup>120</sup> Since there is no evidence that the guests took this special feature of their quarters into account, Stalin was thus given the unique opportunity to eavesdrop on their most intimate conversations.

At the Teheran conference Stalin appeared to be finally satisfied with his allies after they had demonstrated a willingness to meet him more than half way. They gave him a clear impression—clearer, again, than he had probably anticipated—that they would not obstruct his freedom of action in Eastern Europe. Concerning the all-important question of Poland, Roosevelt himself sought out the dictator to offer assurance that “personally he agreed with the views of Marshal Stalin” although Roosevelt “could not participate in any decision” because of concern for the Polish-American vote.<sup>121</sup>

The American and British statesmen hoped to win Russia’s cooperation by satisfying what they perceived as its reasonable aspirations. But in the absence of clear understandings about what exactly was reasonable, their efforts tended to stimulate Russian aspirations rather than to restrain them. If earlier in 1943 the Soviet Union had signaled to the Germans that it would have been contented with return to the status quo as it had obtained in 1941, there were no longer any such indications at Teheran. On the contrary, Stalin hinted that his goals would expand with expanding opportunities: “There is no need to speak at the present time about any Soviet desires, but when the time comes we will speak.”<sup>122</sup> At least one Western participant in the conference, Charles Bohlen, was convinced that those aspirations now extended even beyond the historic area of Russian interest. In what remains one of the best contemporary estimates of Stalin’s intent, Bohlen concluded that the Soviet Union wanted to become “the only important military and political force on the continent of Europe” by reducing the rest of it “to military and political impotence.”<sup>123</sup>

The roots of the developments that later culminated in the cold war should be sought in 1943 rather than in any other period in the history of the Great Alliance. With the cold war in mind, the significance of the separate peace prospects during that critical year is twofold. First, Stalin’s inability to obtain a relatively modest territorial settlement from Germany led him to augment his goals while he continued fighting the war. Second, the British and Americans, disturbed about his leanings toward a negotiated peace, became inclined to tolerate more ambitious Soviet goals. Their attitude encouraged Stalin to test the limits of their tolerance later on. And it was his overestimation of those limits that eventually made conflict inevitable.

<sup>120</sup> The President’s Log, *FRUS: The Conferences at Cairo and Teheran, 1943* (Washington, 1961), 461–64.

<sup>121</sup> Minutes of Roosevelt-Stalin meeting, Dec. 1, 1943, *ibid.*, 594.

<sup>122</sup> Minutes of meeting, Nov. 29, 1943, *ibid.*, 555.

<sup>123</sup> Bohlen, memorandum, Dec. 15, 1943, *ibid.*, 846.

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# A Soviet Historian Evaluates Stalin's Role in History

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A Review Article by ROBERT M. SLUSSER

ROY A. MEDVEDEV. *K sudu istorii (genezis i posledstviia Stalinizma)*. Moscow: Samizdat. 1968. Pp. 1399. Photographically reproduced. New York: Alfred A. Knopf. 1972. \$20.00.

———. *Let History Judge: The Origins and Consequences of Stalinism*. Translated by COLLEEN TAYLOR. Edited by DAVID JORAVSKY and GEORGES HAUPF. New York: Alfred A. Knopf. 1971. Pp. xxxiv, 566, xviii. \$12.50.

NIKITA KHRUSHCHEV earned his place in history twice: first, by his actions and policies as one of the most powerful Soviet political leaders over a period of many years; second, by the shock and stimulus he administered to Soviet historians with his attack on Stalin, launched in the secret speech at the Twentieth Congress of the Communist party of the Soviet Union (CPSU) in 1956 and deepened five years later at the party's Twenty-second Congress.

The complex response of Soviet historians and CPSU ideologists to Khrushchev's wide-ranging assault on Stalin's historical reputation has been analyzed by Nancy W. Heer in a work that takes the story down to mid-1967.<sup>1</sup> As of that date the great majority of Soviet historians held back from attempting a full-scale analysis of Stalin's career and crimes. When, in February 1966, twenty-five leading Soviet intellectuals, scientists, and artists addressed a letter to Leonid Brezhnev, first secretary of the CPSU, warning the party against rehabilitating Stalin at its then-imminent Twenty-third Congress, only two historians' names were included in the list of signatories.<sup>2</sup> Noting the absence of revisionist historians' names

<sup>1</sup> Nancy W. Heer, *Politics and History in the Soviet Union* (Cambridge, Mass., 1970). See the review by Warren B. Walsh, *AHR*, 77 (1972): 552-53.

<sup>2</sup> Peter Grose, "25 Soviet Intellectuals Oppose Any Elevation of Stalin's Status," *New York Times*, Mar. 21, 1966, p. 2. Grose mentions only eight of the signatories, among them Academician Ivan M. Maisky, who was not only a former diplomat (he had been Soviet ambassador to Great Britain from 1932 to 1943) but also a historian. See Maisky's contribution to a 1962 conference of Soviet historians, *Vsesoiuznoe soveshchanie o merakh uluchsheniia podgotovki nauchno-pedagogicheskikh kadrov po istoricheskim naukam 18-21 dekabria 1962 g.* (All-Union Meeting concerning Measures for Improvement of Training of Scientific-Pedagogical Cadres in Historical Sciences) (Moscow, 1964), 145-49, in which he calls for the publication of memoirs from the Stalin era, a genre to which he himself has made notable contributions. The other historian who

from the list of signatories, Dr. Heer comments, "No Soviet historian in any field, and certainly not a specialist in the CPSU, could acquire the kind of social distance from politics possible for a physicist or artist."<sup>3</sup> A similar point is made in a recent review article in this journal. After summarizing evidence indicating the growing intellectual maturity and professional integrity of Soviet historians working in the pre-Soviet period of Russian history, Samuel H. Baron warns, "The constraints upon work in some of the more sensitive areas—such as the history of the Communist party of the Soviet Union, and contemporary history—have diminished little if at all."<sup>4</sup>

These observations help explain why the awesome task of writing a comprehensive historical analysis of Stalinism was finally undertaken neither by a professional historian nor by a party ideologist. Roy Medvedev, the man who successfully met Khrushchev's challenge, was trained in philosophy and made his career in the field of education.<sup>5</sup> Drawn to the study of history by Khrushchev's revelations about Stalin's crimes at the Twentieth CPSU Congress and convinced by this bold action that the party had the inner moral strength needed to slough off the Stalinist legacy and return to Leninist principles, Medvedev joined the party and embarked on the study of its history under Stalin. The actual writing of the book, however, was undertaken only in 1961, following the Twenty-second CPSU Congress.<sup>6</sup> The completion of the book in substantially the form we now have it came toward the end of 1968.<sup>7</sup>

During the long process of gestation there took place a creative interaction between the author and a small but keenly responsive circle of readers, friends, and critics, who analyzed and commented on the work in progress and in many cases provided valuable source materials for its enrichment. How could such an autonomous intellectual process, entirely lacking

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signed the letter, not mentioned by Grose, was Academician S. D. Skazkin, one of the leading Soviet medievalists. See Skazkin's biography in *Sovetskaia istoricheskaia entsiklopediia* (Soviet Historical Encyclopedia), 12 (Moscow, 1969): 942.

<sup>3</sup> Heer, *Politics and History*, 178. Dr. Heer's point is weakened but not entirely invalidated by the fact that two historians did sign the letter to Brezhnev; neither was, in her sense, a "revisionist" historian. Later in 1966 Skazkin was one of three historians who signed a letter to the RSFSR Supreme Soviet protesting the enactment of two articles in the criminal code (paragraphs 190-1 and 190-3) directed against internal political dissidents. The other historian signatories were V. M. Turok, a specialist in modern German and Austrian history, and P. I. Yakir, an intrepid critic of Stalinism and the son of a Red Army leader purged by Stalin. For a report of P. I. Yakir's arrest by the secret police, see the *New York Times*, June 22, 1972, p. 2. Copies of both the letter to Brezhnev and the letter to the RSFSR Supreme Soviet have been published by Radio Liberty, Munich, in its documentary series, numbers AS-159 and AS-273. I am grateful to Mr. Peter Dornan for help in locating these and other documents.

<sup>4</sup> Samuel H. Baron, "The Transition from Feudalism to Capitalism in Russia: A Major Soviet Historical Controversy," *AHR*, 77 (1972): 728.

<sup>5</sup> Biographical information on Medvedev is given in Professor David Joravsky's introduction, *Let History Judge*, x.

<sup>6</sup> See Medvedev's foreword, *ibid.*, xxv. At the end of the work, however, Medvedev dates the beginning of the writing in August 1961 (p. 566). The Twenty-second CPSU Congress met in October 1961.

<sup>7</sup> The terminal date for composition is given as August 1968 (*ibid.*), but some materials were added as late as January 1969, e.g., a reference on page 455 to a review of wartime memoirs in *Kommunist* (1969), no. 2, p. 127.

official sanction, take place in the Soviet Union? The answer lies in the fascinating phenomenon of "Samizdat," a term coined by Soviet intellectuals on the analogy of Gosizdat (State Publishing House), to designate the "self-publication" of manuscripts that for one reason or another have not found, or are not likely to find, a regular publisher. Typed by willing volunteers, who thereby assume a direct personal risk, Samizdat manuscripts circulate on a restricted scale, known to the secret police but somehow managing to elude their controls, and providing one of the principal bases for the development of the newly self-aware, critical intelligentsia in present-day Soviet society. Medvedev intended from the outset, however, that his book should eventually be published legally in the Soviet Union, for in his view only when the party had squarely faced the historical truth about Stalin and Stalinism would it be morally strong enough to overcome the baneful consequences of his rule. Medvedev therefore submitted the manuscript on completion to the CPSU Central Committee for approval. In an action carrying grave moral implications, the party rejected Medvedev's manuscript, thereby indicating its intention of whitewashing Stalin and closing the door to unbiased historical study of his career.<sup>8</sup>

It was at just about this time that scholars in the West first learned of the existence of Medvedev's work through a brief but laudatory reference to it in another Samizdat document, a "Memorandum" by the Soviet physicist Andrei Sakharov, which the *New York Times* published in translation on July 22, 1968.<sup>9</sup> It is possible that Sakharov's well-intentioned reference to the book and the prominence given to it by Western news and propaganda media contributed to the decision by party authorities not to publish it in the Soviet Union.<sup>10</sup> It seems highly probable, however, that the decision by the CPSU leadership to halt the movement toward unbiased historical study of Stalin's career had been taken before Sakharov's "Memorandum" was published in the West, and in fact well before Medvedev completed *Let History Judge*.<sup>11</sup>

In early 1969, at a time when Medvedev was putting the finishing

<sup>8</sup> For an official history of the CPSU that deals with Stalin in the spirit approved by party ideologists, see *Istoriia Kommunisticheskoi Partii Sovetskogo Soiuza v shesti tomakh* (History of the Communist Party of the Soviet Union in Six Volumes) (Moscow, 1964-), especially vol. 4, pt. 2 (1970), which achieves the dubious feat of covering the period from 1929 to 1937 with only the barest allusion to the Great Purge.

<sup>9</sup> Andrei Sakharov's "Memorandum" is published in book form under the title *Progress, Co-existence and Intellectual Freedom* (New York, 1968). For the reference to Medvedev, see page 54. Sakharov describes the manuscript of Medvedev's book as being one-thousand pages long, a discrepancy that probably indicates he had seen only the first two parts; at the time Sakharov wrote the "Memorandum" Medvedev had not yet completed part 3, which begins on page 1031 of the Samizdat edition.

<sup>10</sup> On the difficulties caused for Medvedev by the Western publication and broadcasts of Sakharov's "Memorandum," see the book by Medvedev's twin brother, the biologist Zhores A. Medvedev, *The Medvedev Papers* (London, 1971), 388-89.

<sup>11</sup> A pointer to the approximate time by which the decision had been taken is the publication by *Voprosy istorii* in February 1968 of an article dealing in a completely Stalinist spirit with one of the purge trials in the early 1930s. See D. L. Golinkov, "Razгром ochagov vnutrennei kontrrevoliutsii v Sovetskoi Rossii" (The Destruction of Hotbeds of Domestic Counterrevolution in Soviet Russia), *Voprosy istorii* (1968), no. 2, pp. 148-65. Golinkov's article was the third in a series that began in the issue of December 1967.

touches on his manuscript, the authoritative CPSU theoretical journal *Kommunist* moved closer toward an open rehabilitation of Stalin with the publication of both a review of recent memoirs by Soviet army commanders, which gave a favorable evaluation of Stalin's role as a wartime leader,<sup>12</sup> and an article by five relatively obscure authors setting forth the principles to be observed in dealing with the history of the CPSU, in effect warning Soviet historians to cease and desist from any further analysis of the phenomenon of Stalinism.<sup>13</sup> Stung into direct action, Medvedev fired off a long letter to the editors of *Kommunist* in which he sharply criticized them for ignoring not only historical evidence about Stalin's crimes and blunders but also the directives of the Twentieth and Twenty-second CPSU Congresses condemning Stalin's "cult of personality." Medvedev's letter was never published by *Kommunist*, but it did appear, without his authorization, in *Posev*, an emigré journal published in West Germany.<sup>14</sup> Professor David Joravsky, in his introduction to *Let History Judge*, argues that it must have been the KGB that sent Medvedev's letter to *Posev*. Be that as it may, publication of the letter abroad was used as the pretext for expelling Medvedev from the party. Then, early in 1970 *Posev* published over Medvedev's name a rather crudely written article attacking the Soviet leadership as corrupt. The journal claimed that the article emanated from Samizdat sources.<sup>15</sup> Medvedev, in a letter dated March 25, 1969, to the Soviet press agency Novosti and to other news agencies, denied both that he was the author of the article and that it had ever circulated in the Soviet Union, and he charged that its publication represented "an intentional defamation."<sup>16</sup> Made acutely conscious by these developments of the danger that his long manuscript on the history of Stalinism might be published in an unauthorized and inaccurate edition in the West, and

<sup>12</sup> Ye. Boltin, "Volnuiushchie stranitsy letopisi Velikoi Otechestvennoi Voiny" (Stirring Pages in the Chronicle of the Great Fatherland War), *Kommunist* (1969), no. 2, pp. 119-28. For the evaluation of Stalin, see page 127.

<sup>13</sup> V. Golikov, S. Murashov, I. Chkhikvishvili, N. Shatagin, and S. Shaumian, "Za leninskuiu partiinost' v osveshchenii istorii KPSS" (For Leninist Party Spirit in the Treatment of the History of the CPSU), *ibid.*, no. 3, pp. 67-82. For a rebuke to Soviet historians who persist in attempting to study Stalin's role in history, see page 73.

<sup>14</sup> Roy Medvedev, "Vozmozhno li segodnia' reabilitirovat' Stalina? Otkrytoe pis'mo v zhurnal 'Kommunist'" (Is It Possible to Rehabilitate Stalin Today? An Open Letter to the Journal "Kommunist"), *Posev*, 25 (1969): no. 6, pp. 25-30; no. 7, pp. 25-34. The editors appended a note (no. 6, p. 25), explaining that the letter had circulated inside Russia in a Samizdat edition, a copy of which reached them. According to Peter Reddaway, ed., *Uncensored Russia* (London, 1971), 421, 484n., Medvedev's letter first appeared in the *Khronika tekushchikh sobytii* (Chronicle of Current Events), a Samizdat periodical.

<sup>15</sup> R. Medvedev, "Pravda o sovremennosti" (The Truth about the Present Time), *Posev*, 26 (1970): no. 1, pp. 39-45.

<sup>16</sup> See Theodore Shabad, "Russian Historian Disavows Anti-Soviet Article Attributed to Him in West," *New York Times*, Apr. 26, 1970, p. 29. Shabad quotes from Medvedev's letter but does not give its full text. The letter in which a *Posev* editor attempts to justify the publication of documents from unidentified sources in Russia, which Joravsky cites in his introduction (*Let History Judge*, xi), was written in response not to Medvedev's letter to Novosti but to a manifesto published in *Le Monde* (Apr. 11, 12-13, 1970) in which Medvedev, Sakharov, and V. F. Turchin asserted that a "Letter to Brezhnev," which *Posev* had published over their signature, was a forgery. See "Les Savants Soviétiques et le 'Samizdat,'" *Le Monde*, May 6, 1970, p. 6.

blocked from publishing it in his own country, Medvedev thereupon took the weighty and responsible decision to authorize its publication abroad, but under conditions designed to ensure that it would be presented with scrupulous accuracy and in suitable form.

This prepublication background helps explain the character of *Let History Judge*, its author's intentions in writing it, and the form in which it is presented. Knowing that background one can better understand the moral stance of the author—that of a convinced Communist and a staunch admirer of Lenin—and its pervasive influence in the book. For it is Medvedev's underlying thesis that Stalinism was not the inevitable outcome of the political system established in Russia by Lenin and his colleagues but a monstrous distortion and perversion due in large part, though not exclusively, to fatal defects in Stalin's character. Because this thesis runs directly counter to the view, widely accepted in the West, that Stalinism was the logical consequence of Lenin's actions and policies, a number of reviewers have criticized Medvedev for what they regard as his failure to understand that link.<sup>17</sup>

This is not the place for an extended discussion of the problem, but two points should be made. First, Medvedev shows himself to be well aware of the roots of Stalinism in the policies of the early years of the Soviet regime, and although he does tend to exculpate Lenin from direct responsibility for Stalinism, he nevertheless gives the fullest and most searching analysis of this problem yet provided by any scholar. Second, acceptance of the thesis that Lenin's actions and policies led directly to those of Stalin has caused many scholars in the West to assume that the problem of the historical roots of Stalinism has been solved and no longer requires serious analysis. It is greatly to be hoped that Medvedev's treatment of the problem in *Let History Judge* will lead to a profound rethinking of it by Western historians and political scientists.

A closely related criticism of Medvedev's book has been that its importance lies mainly in the evidence it provides for tendencies within the present-day movement of dissident intellectuals in the Soviet Union, with the implication—sometimes made explicit—that there is little really new in what Medvedev has to tell us about the history of the Stalin era.<sup>18</sup> There can be no doubt, of course, that *Let History Judge* does have value for an understanding of its author's political views, but its value as a contribution to history is far greater. In the words of Edward Crankshaw, it is "the first sustained, comprehensive, closely argued critique of the genesis, development and triumph of Stalin ever to be written from the inside."<sup>19</sup>

Accepting that judgment, I shall devote the remainder of this review

<sup>17</sup> Reviews stressing this approach include those by Merle Fainsod, *Book Week*, Jan. 2, 1972, p. 4, and Leonard Schapiro, *Sunday Times* (London), Mar. 26, 1972, p. 40. For a balanced discussion of the problem, see Joravsky's introduction, *Let History Judge*, xii-xvi.

<sup>18</sup> See, for example, the reviews by I. F. Stone, *New York Review of Books*, Feb. 24, 1972, pp. 14-22; Robert H. McNeal, *Russian Review*, 31 (1972): 179-81; and Robert Conquest, *Commentary*, 53 (1972): no. 6, pp. 80-89.

<sup>19</sup> Edward Crankshaw in the *Observer* (London), Mar. 26, 1972, p. 36.

article to consideration of four substantive aspects of the book: the sources on which it is based; the contributions it makes to historical knowledge; certain weaknesses in the author's analysis of Stalinism due to his ignorance or neglect of non-Soviet studies; and the relationship between the original text and the edited translation.

THE RICHNESS AND DENSITY of Medvedev's book are the result in part of the author's ability to draw on an extensive body of memoirs and other materials dealing with Stalin and the Stalin era. Some of these have circulated in Samizdat editions.<sup>20</sup> Others have been published in the Soviet Union, for example, an account of imprisonment and torture during the Great Purge by the secretary of the party leader, Ya. M. Sverdlov.<sup>21</sup> Works of fiction, or historical memoirs thinly disguised as fiction, have provided valuable insights—for example, a series of stories about concentration-camp life by V. T. Shalamov.<sup>22</sup> Survivors of the purge trials of the 1930s have added their testimony; among the most important is a detailed account by M. P. Yakubovich, one of the defendants in the show trial of the so-called All-Union Bureau in 1931, which explains how the secret police worked over the defendants to prepare them for their roles in the trial.<sup>23</sup> Memoirs by Old Chekists (secret police agents whose service dates back to the early years of Soviet power) constitute a unique group of sources.<sup>24</sup>

Many of the sources used by Medvedev have been published with official approval, for example, a number of histories of Communist party organizations in the Union Republics (Medvedev uses, among others, party histories dealing with Georgia, Armenia, Azerbaijan, Kazakhstan, Kirghizia, Uzbekistan, Tadzhikistan, Estonia, Moldavia, and the Ukraine).<sup>25</sup> Published after the Twenty-second CPSU Congress and, in most instances, before the official turn toward a partial rehabilitation of Stalin, these works provide much new information on the purge in minor-

<sup>20</sup> The most extensive collection of Samizdat materials in the West is that compiled by Radio Liberty in its *Arkhiv Samizdata*, sets of which have been deposited at the Center for Slavic and East European Studies at Ohio State University, the Center for International Studies at the Massachusetts Institute of Technology, and the Library of Congress. A number of the Samizdat documents used by Medvedev, which Professor Joravsky was unable to locate, are available in the Radio Liberty archive. If a revised edition of *Let History Judge* is prepared, it would be desirable to provide full data on all known Samizdat sources used by the author.

<sup>21</sup> E. Ya. Drabkina, "Zimnii pereval" (Winter Solstice), *Novyi mir*, 44 (1968): no. 10, pp. 3-93.

<sup>22</sup> For a listing of V. T. Shalamov's stories, see Reddaway, *Uncensored Russia*, 476.

<sup>23</sup> For a biographical sketch of Yakubovich, one of the outstanding figures in the dissident movement in the USSR, see *Khronika tekushchikh sobytii*, no. 10 (Oct. 31, 1969), as translated by Peter Reddaway in *Problems of Communism*, Mar.-Apr. 1970, pp. 51-52, and in *Uncensored Russia*, 397-402. The article by Golinkov, "Razgrom ochagov vnutrennei kontrrevoliutsii v Sovetskoi Rossii," reads like an attempt to discredit Yakubovich's far more accurate account of the 1931 trial, which circulated in a Samizdat edition.

<sup>24</sup> For example, the memoirs of S. O. Gazarian, which were circulated in a Samizdat edition, and the book by F. T. Fomin, *Zapiski starogo Chekista* (Memoirs of an Old Chekist) (Moscow, 1962).

<sup>25</sup> See the works cited on pages 203-07 and 221 of *Let History Judge*.

ity regions of the USSR. And professional historians have contributed their share. Medvedev quotes with telling effect speeches made at a conference of historians held in Moscow in December 1962—at a time, that is, when the impetus given by Khrushchev to the objective historical study of the Stalin era was still strong.<sup>26</sup>

To call attention to all the new information provided by Medvedev would be a task requiring far more space than is available. Step by step, as he follows Stalin's rise to power, Medvedev adds previously unknown details or puts familiar facts in a new and more revealing light. When he reaches the Great Purge, his contributions become so numerous and cumulatively weighty that a dialectical transformation of quantity into quality takes place, and a more profound concept of the purge emerges, that of an organic process embracing every aspect of Soviet society. A particularly valuable cluster of new information surrounds the assassination of S. M. Kirov, with telling details contributed by friends, associates, and contemporaries of the fallen party leader. In this and similar sections *Let History Judge* creates the impression of a great collective outpouring of long-repressed memories. Sometimes Medvedev casually throws in a detail that illuminates an area previously shrouded in darkness. For example, he reports that in 1948, following the conference on biology at which T. D. Lysenko triumphantly revealed the party's endorsement of his theories, Stalin ordered A. A. Zhdanov into retirement, "using as an excuse Zhdanov's supposedly incorrect position" on doctrines espoused by Lysenko, "whom Zhdanov proposed to remove from the presidency of the Agricultural Academy."<sup>27</sup> This information (for which, unfortunately, Medvedev gives no source), together with data supplied by his brother Zhores to the effect that it was Stalin personally who approved Lysenko's report to the conference,<sup>28</sup> provides for the first time a valid basis for defining the positions of Stalin and Zhdanov in the biological controversy.

More important than any single new fact or group of facts, however, is Medvedev's profound and all-embracing grasp of the purge as a historical phenomenon. Particularly noteworthy are those sections of the book in which he analyzes the support rendered the Stalin cult by various groups in Soviet society.<sup>29</sup> Like the memoirs of Nadezhda Mandelshtam,<sup>30</sup> these sections of *Let History Judge* open up for historical study a wide panorama of changes in Soviet society during the Stalin era.

Medvedev takes a highly negative attitude toward non-Soviet studies of Stalin and the history of the CPSU. This attitude, needless to say, is as

<sup>26</sup> The speeches are included in *Vsesoiuznoe soveshchanie*. On the setting of the conference and its proceedings, see Heer, *History and Politics*, 145-63.

<sup>27</sup> *Let History Judge*, 484.

<sup>28</sup> Zhores A. Medvedev, *The Rise and Fall of T. D. Lysenko*, tr. I. Michael Lerner (New York, 1969), 116-17.

<sup>29</sup> See especially chapter 11, "The Conditions Facilitating Stalin's Usurpation of Power."

<sup>30</sup> Nadezhda Mandelshtam, *Hope Against Hope*, tr. Max Hayward (New York, 1970).



tendentious and one-sided as that of Western reviewers who find nothing new in his book. There are some areas in which non-Soviet historical scholarship has made contributions of fundamental importance to the study of the Stalin era, and by ignoring this work (or having been denied access to it) Medvedev has weakened his book. In particular his attempts to understand Stalin's motives for launching the Great Purge suffer from ignorance of some key links. Medvedev has apparently never seen, for example, the text of L. B. Kamenev's notes on his conversation with N. I. Bukharin in July 1928, a document that casts a revealing light on the struggle between Stalin and his rivals and on Stalin's character and strategy.<sup>31</sup> Similarly, Medvedev has not made adequate use of *The Letter of an Old Bolshevik*, and has thus missed its penetrating analysis of high-level political maneuverings in the period preceding the assassination of Kirov.<sup>32</sup> In addition Medvedev has failed to grasp the central importance of G. K. Ordzhonikidze's role in blocking Stalin's attempt to purge the party, as well as the significance of the link between Ordzhonikidze and Bukharin. Medvedev makes the assumption—plausible but erroneous—that G. G. Yagoda was a faithful executant of Stalin's orders throughout his career, thereby missing the significance of the telegram from Stalin and Zhdanov in September 1936 demanding Yagoda's ouster as chief of the NKVD.

It is a mark of Medvedev's stature as a historian, however, that he continued to deepen his understanding of Stalinism even while he was engaged in writing his book. One of the great values of the Samizdat manuscript of *Let History Judge* is that it enables us to follow this process in action. For example, it can be shown that Medvedev's attitude toward the problem of internal opposition to Stalin after 1929 underwent a significant change while the book was being written. The fullest treatment in the book of the case of M. N. Riutin in 1932, a turning point in the history of the party, is clearly a late insertion into the text (translation, pp. 142–43; Samizdat edition, p. 317a), which stands in striking contrast with references to Riutin elsewhere that treat the case as an unimportant

<sup>31</sup> Kamenev's notes on his conversation with Bukharin first appeared in an illegal underground leaflet in the USSR entitled, "K partiinym konferentsiiam: Partiia s zaviazannymi glazami vedut k novoi katastrofe" (Toward the Party Conferences: They Are Leading the Party Blindfold toward a New Catastrophe), signed "Bol'sheviki-lenintsy (Oppozitsiia)" (Bolsheviks-Leninists [Opposition]). *Die Volkswille*, a German Trotskyite journal, published a translation into German, from which it was retranslated into Russian and published in the Menshevik *Sotsialisticheskii Vestnik*, 9 (1929): no. 6 [196], pp. 10–11, under the title "Bol'sheviki o samikh sebe" (The Bolsheviks in Their Own Words). A few months later, having meanwhile obtained a copy of the original leaflet, the *Sotsialisticheskii Vestnik* added a brief additional section under the heading, "Dopolneniia k rasskazu Bukharina: Noch' 11–12 iulia" (Addition to Bukharin's Story: The Night of July 11–12), *ibid.*, no. 9 [199], p. 10. There is a copy of the Russian text in the Trotsky Archive at Harvard. No complete translation into English has been published.

<sup>32</sup> *The Letter of an Old Bolshevik* was first published in the *Sotsialisticheskii Vestnik*, 16 (1936): nos. 23–24 [379–80], pp. 20–23, and 17 (1937): nos. 1–2 [381–82], pp. 17–24, under the title "Kak podgotovlialisia moskovskii protsess (Iz pis'ma starogo bol'shevika)" (How the Moscow Trial Was Prepared [From the Letter of an Old Bolshevik]). An anonymous translation (New York, 1937) has been reprinted in Boris I. Nicolaevsky, *Power and the Soviet Elite* (New York, 1965), 26–65. The *Letter* is discussed in a Samizdat publication, the *Politicheskii Dnevnik*, no. 25, Oct. 1966.

and factually dubious incident (e.g. translation, p. 155; Samizdat edition, p. 344).

In regard to Soviet policy in Eastern Europe after World War II, the manuscript discloses a similar evolution in Medvedev's understanding. As originally written, the section dealing with this subject differed little from orthodox Soviet works that praise the "democratic socialist revolution" wrought in East European countries with Soviet aid.<sup>33</sup> After this section was written, however, the Dubček regime in Czechoslovakia reopened the subject of the purge trials of the early 1950s, and Medvedev inserted a long section (translation, pp. 476–77; Samizdat edition, pp. 1150–1150b)—based on articles by Eugen Loeb, one of the trial defendants, that were published in Prague in the spring of 1968—that differs sharply from the passage written earlier.

The moral to be drawn from these and similar instances is the urgent need for international cooperation among historians in studying the Stalin era. Tackling that central problem in modern Russian history is a task in which neither Soviet nor Western historians are likely to make substantial progress without the others' help.

Medvedev's book deserves and demands the most rigorous, scrupulous translating, editing, and publication. Few books in the field of Soviet history have been better or more conscientiously dealt with in this respect. The translator, the editors (especially Professor Joravsky, who assumed responsibility for the final editorial supervision), and the publisher have all met high standards, and the reader of the English-language version can rest assured that it is a substantially faithful rendering of the original text. Substantially, but not completely; and since we are dealing with a work of fundamental importance, it is necessary to note certain not entirely inconsequential differences between text and translation.

The number of actual errors is gratifyingly small. One may be mentioned for the record: a passage on page 110 of the translation reads, "And such a scapegoat was found: the specialists, the intelligentsia, *who had been tainted* before the Revolution" (my italics). What Medvedev wrote, however, was "And in fact such a scapegoat was provided for Stalin by specialists from among the old Russian (and Ukrainian) intelligentsia, *which had taken shape* before the Revolution" (Samizdat edition, p. 259, my italics).

There is a slight but perceptible difference in tone between the original text and the translation. Readers of the translation will recognize Medvedev's position as a convinced Communist, but they are not likely to realize fully the pervasive influence of this attitude, since many passages, in being edited for conciseness, have lost some of their personal flavor. Compare, for example, "In 1924 . . . Stalin did not seem dangerous" (trans-

<sup>33</sup> *Let History Judge*, 484. The original (Samizdat edition, 1138–39) is both fuller and more orthodox in spirit than the translation.

lation, p. 28), with "In 1924 . . . Stalin did not seem dangerous *for the revolution*" (Samizdat edition, p. 74, my italics).

In general, the translator and editors have shown great skill in condensing Medvedev's sometimes prolix and repetitive text without sacrificing anything essential. At times, however, the trimming process has been unduly rigorous. Consider these contrasting passages: "Old Bolsheviks now recall that Stalin had the pseudonym Vasili in 1912" (translation, p. 319); "In this connection some Old Bolsheviks recall that in 1912 Stalin, on escaping to Petersburg from exile and lodging together with Aron Sol'ts in one of the rooms at T. A. Slovatinska's, used the party pseudonym 'Vasili'" (Samizdat edition, p. 702). Or these: "P. P. Postyshev, demoted from the Politburo and the Ukrainian Central Committee to a provincial post, then arrested and shot" (translation, p. 192); "P. P. Postyshev, a popular party worker, who held the post of second secretary of the Central Committee of the Communist party of the Ukraine. (Approximately one year before his arrest Postyshev was removed from the Politburo and assigned as first secretary of the Kuibyshev *obkom*)" (Samizdat edition, pp. 428-29).

More serious are deletions that have removed material of substantive value. Here are some examples: (1) Entirely omitted is an impassioned outburst (Samizdat edition, p. 413; cf. translation, p. 186) on the moral duty of the Soviet historical profession to tell the full truth about the purges. (2) There seems to be no good reason for cutting out an account of the repression of M. Leiteisen, a pioneer in the study of interplanetary travel, together with that of the engineers working at a construction bureau engaged in building a flying-wing aircraft (Samizdat edition, p. 501a; cf. translation, p. 229). (3) A passage on the intervention of Stalin and the Comintern leadership into the internal political struggle in the Communist party of Czechoslovakia in the late twenties has been completely eliminated (Samizdat edition, p. 890; cf. translation, p. 387). (4) A long section has been omitted concerning a novel by H. G. Wells that depicts the moral degeneration of a revolutionary leader—clearly relevant to Stalin and so recognized by a Soviet literary scholar in a work published in 1963.<sup>34</sup>

My intention in citing these passages is not to impugn the quality of the translation and editing, which, I wish to emphasize, meet a high standard. The purpose is rather to indicate that specialists will find close study of the original manuscript indispensable. All honor, then, to the publisher, who has gone to great trouble to make available as legible as possible a copy of the Samizdat text. His services, like those of the translator and editors, are on the same high level of intellectual responsibility and devotion to the unending quest for historical truth as that of the book itself and its author.

<sup>34</sup> Samizdat edition, 744-47. See Ya. Kagarlitskii, *Gerbert Uells: Ocherk zhizni i tvorchestva* (Herbert Wells: An Outline of His Life and Work) (Moscow, 1963), 263-65.

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## Reviews of Books

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### GENERAL

ISAAC DEUTSCHER. *Marxism in Our Time*. Edited by TAMARA DEUTSCHER. Berkeley: Ramparts Press. 1971. Pp. 312. \$5.95.

GEORGE LICHTHEIM. *George Lukács*. (Modern Masters. New York: Viking Press. 1970. Pp. xi, 146. \$5.75.

ROBERT C. TUCKER. *The Marxian Revolutionary Idea*. (Publication of the Center of International Studies, Princeton University.) New York: W. W. Norton and Company. 1969. Pp. xi, 240. \$5.95.

It is hard to say anything new about Marxism, particularly if one is trying to defend some aspect of it. Marxism has been expounded, attacked, defended, and dissected in such a perennial world-wide torrent of literature that one might well wish for a moratorium on the subject.

This is not to denigrate the importance of Marxism as a historical phenomenon of the last hundred years and more. Marxism in one reading or another has become the frame of reference of most of the labor movement and intelligentsia over a large part of the globe, not to mention its privileged position as the official philosophy in Communist countries. But critical study of Marxism has made it fairly clear that the great success of Marxism is not based so much on its logical coherence or its self-proclaimed scientific method as on an appeal that is emotional, moral, quasi religious in fact.

What emerges most clearly from the three works under review, all aiming one way or another at a positive evaluation of the doctrine, is the essentially theological character of Marxism. Though deceptively couched in the language of science, Marxism rests ultimately on an unsubstantiated assertion of faith, above all

the faith that history will bring about the ultimate utopian society. Recognition of this fideistic character of Marxism helps provide a basis for understanding the development of official Soviet thought as a quasi-religious parody of scientific rationalism. Stalin did not have quite as far to go in "debasing" the original coin of Marxist doctrine as Mr. Deutscher maintains.

Flourishing as a faith movement in a supposedly scientific age, the vast influence of Marxism is all the more surprising on this account. One may fall back on the proposition that theological modes of thought—drawing from the need to believe—are imbedded in human nature and will attach themselves to whatever system of ideas happens to be popular. More specifically this trait of ideological theologizing seems to be rooted in the Continental European intellectual tradition, which apparently cannot dispense with grand system and hence remains vulnerable to the philosophical pretensions of Marxism. Two of the works under review proceed from this frame of reference: Deutscher readily admits to it, and Lichtheim deals tolerantly with Lukács's doctrinal blinders for the same reason. As for Tucker's attempt to appreciate Marx from the Anglo-American empirical standpoint, it points up another major component of Marxism's influence, equally illogical, as the doctrine of modernizing revolutionaries who hasten to "deradicalize" their faith after they take power.

*Marxism in Our Time* is a collection of Deutscher's lectures and essays, mainly of the mid-1960s, selected and edited by his wife after his untimely death in 1967. In subject they range from the methodology of *Das Kapital* and the history of Polish communism to the bureaucratization of the Russian Revolution

and the rejection of Marxism by the New Left. They reveal Deutscher as beyond a doubt the most creative and articulate Marxist thinker to appear anywhere since his mentor Trotsky—but a committed Marxist, be it understood, with no intention of questioning the basic premises of his faith. Deutscher's objectivity only went as far as the condemnation of Stalinism as a perversion of the true faith.

Not as much can be said for George Lukács, the Hungarian Marxist philosopher and literary critic, who as the giant of Communist Marxism, relatively speaking, only throws into relief the short intellectual stature of the other theoreticians of communism. In *George Lukács*, a brief intellectual biography in the Modern Masters series, the eminent historian of Marxism and socialism George Lichtheim does his best to convey to the English-speaking reader an appreciation of Lukács's critical intelligence and his perseverance under adversity. In chapters pursuing the successive directions of emphasis in Lukács's work—literary, historical, esthetic, philosophical—Lichtheim details his long but losing struggle to maintain his intellectual individuality within the shrinking confines of his Stalinist loyalty. Tragically, Lukács's attachment to the Germanic world of reified abstractions and his fideistic commitment to Leninist Marxism sapped his resistance to the Stalinist manipulation of the doctrine (so effectively condemned by Deutscher). Except for the brief moment of revolt in Hungary in 1956, Lukács fettered himself so well that the Soviet authorities hardly needed to pull the reins on him.

Against the convoluted efforts of the two European thinkers to justify their Marxist commitments, the essays by Robert Tucker in *The Marxian Revolutionary Idea* convey a breeze of cool realism. In these largely reprinted articles Tucker presents an articulate but familiar review of the basic principles of Marxism and endeavors to sift out its relevance for the Anglo-American mind of today. The haul is not very great—mainly the notion of rehumanizing man by transcending the state and the division of labor—while the major political successes of Marxism Tucker recognizes to have been accomplished under conditions of backwardness contradicting the theory and vitiating the ideal. One wishes that a few social philosophers

would forget Marx and start afresh to tackle the problem of the actual and the ideal.

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KENNETH E. BOULDING. *A Primer on Social Dynamics: History as Dialectics and Development*. New York: Free Press, 1970. Pp. viii, 153. Cloth \$5.95, paper \$2.45.

LESLIE SKLAIR. *The Sociology of Progress*. (International Library of Sociology and Social Reconstruction.) [London: Routledge and Kegan Paul;] distrib. by Humanities Press, New York, 1970. Pp. xvi, 272. \$10.00.

Kenneth Boulding's tract is an attack on all dialectical philosophy. In the flux of history and social dynamics he distinguishes two kinds of processes: dialectical ones centered on conflict and nondialectical, developmental, and evolutionary ones in which conflict is merely incidental. He contends that the latter has been by far the most important. In his comments on this theme, apart from saying much that is obvious to historians, he triumphantly demolishes the major tenets of Marxism. He also undertakes a cost-accounting of revolution, showing that evolution is cheaper (why did revolutions ever happen?). In conclusion he sketches a vision of a new universal humanistic history composed by man-computer teams. The appendix on Japan, "the first twenty-first century country," deals with the paradoxical appeal of Marxism in a country that "has one of the least dialectical of all histories." The tract is written in the social science jargon: "The development of democracy is the equivalent, in the sphere of social distance, to the reduction in the cost of transport and in the loss of power gradient in a geographical sphere" (p. 49); appropriately the author excels in reducing human phenomena to charts, graphs, and mathematical symbols. As for his command of historical reality one quotation suffices: "In the Soviet Union [Lenin's] war communism created a major famine and almost destroyed industrial production" (p. 93)—when in fact industry had come to a virtual standstill and famine had started even before the Bolsheviks took power.

Leslie Sklair, a sociologist at the London School of Economics, has written a more solid and ambitious work, though equally baffling to historians, defending the idea of progress as es-

sential for the theory and practice of sociology. He begins by tracing selectively the history of the idea of progress in the context of evolving sociological thought within the Western tradition in a manner familiar and useful to practitioners of the history of ideas. His historical survey ends with a discussion of the current distrust of "progress" in the writings of Jacques Ellul, Herbert Marcuse, and Ernst Gellner, "writers so divorced from life as it is daily lived that they do not recognize material and moral progress when they see it" (p. 107). He then sets out to "demonstrate that however ideologically unfashionable and scorned theoretically the idea of progress may be, it is essential to modern social thought, and that a sociological theory of progress is not only possible but necessary." The heart of his sociological theory of progress is "the sociological ethic." It establishes norms for deciding "whether an action contributes to the satisfaction of one or more of the individual or social needs." If it does the action is progressive. It would seem that in his learned sociological terminology the author merely re-establishes the traditional liberal social gospel: When there are problems to be solved in society, the best solutions—and the most progressive—are those that satisfy more people than any other suggested solution; they offer the greatest good to the greatest number.

The author claims to have made a special contribution to sociological theory by separating "innovational" progress from "non-innovational" progress. The former refers to the contributions made by science and technology and to the awareness (since the eighteenth century) of progress based on these achievements. As for the mind-boggling notion of noninnovational progress the author supplies too few facts for clear definition. He seems unaware that changes in attitudes, creeds, and institutions also constitute innovations. European development before 1700 certainly abounds with innovations of all kinds contributing to the rising confidence expressed in the notion of progress. What makes the author's argument even more obscure is his distinction between innovativeness and inventiveness; he argues, for instance, that Meiji Japan was high in terms of innovativeness and low in inventiveness (as are most contemporary developing countries). At the end he contends that innovational and nonin-

novational progress can be equally progressive on the road to modernity (whatever that undefined term stands for), which is his way of admitting that under present conditions "scientific" progress may even be regressive. What then constitutes truly progressive progress in our present world this eminently Europe-centered book does not reveal.

Reclining in his armchair after reviewing these books, a historian—any historian, I dare say—will exclaim: if only these authors knew more history! To judge by these works history is not dying out among the social scientists, as has been predicted. But, by Clio, what starved and ill-shapen history sometimes emerges from their labors!

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BENJAMIN B. WOLMAN, editor. *The Psychoanalytic Interpretation of History*. Foreword by WILLIAM L. LANGER. New York: Basic Books. 1971. Pp. x, 240. \$8.95.

In the first of three biographical studies in this collection Gustav Bychowski relies upon Trotsky's biography to illuminate Stalin's childhood and makes use of the "primal horde" as a theoretical model or explanatory device. He adds little to Khrushchev's analysis in terms of the "cult of personality."

Peter Loewenberg, less casual in his use of sources and psychoanalytic concepts, has been at pains to show that the "personal fantasies" of the Zionist leader, Theodor Herzl, were a "prelude to action" (p. 150). Certain of Herzl's experiences, in large part known from his *Diaries* and other sources, easily inferred from his fiction, indicate that he was unable to maintain ordinary personal ties of love or friendship but was able to derive an equivalent satisfaction from, on the one hand, his vision of the New Jerusalem and, on the other, "the mass response to his public utterances" (p. 179). Here Loewenberg has given considerable substance to the elusive term "charisma." He does not take up this problem from the other side, the relation of follower to leader, but he shows that Herzl, in adapting his fantasies to reality, "first codified" "a new ethos of militant resistance," a new "ego ideal" or "model of behavior," based in part on "the myth of chiv-

alry" (pp. 169-70), in part on the internalized "values of the dominant majority" (p. 171). This "new ethos" belongs chiefly to a later period ("was adopted out of the holocaust" [p. 1969]) and was not necessarily characteristic of Herzl's mass following in his lifetime. The confusion on this point derives from the sequence in which the argument is presented, not from its substance. This small matter aside, Loewenberg's paper is a forceful demonstration of the value of a psychoanalytic approach to the study of history.

Robert Waite has tried to answer the question, "why did Hitler himself become an anti-Semite?" (p. 195). His evidence ranges from the general background—the atmosphere of Vienna, von List's anti-Semitic pamphlets—to the particular circumstances of Hitler's life. His "tentative hypothesis" is that Hitler, burdened by incestuous desires and inclined toward possibly indulging in a "monstrous sexual perversion," came to feel "a truly massive amount of self-hatred and self-loathing which he projected onto the Jews" (p. 207). The argument is plausible but too dependent on the category of an alleged personality type, the anti-Semite. This category, taken seriously, would undermine both historian and psychoanalyst. *Who* is the target of projected emotions, Jew or Jesuit, Free-Mason or bourgeois, is chiefly a historical question. There is certainly nothing in the "monstrous sexual perversion" that would determine a projection specifically onto the Jews. Waite adduces other evidence, chiefly in connection with the Jewish physician who attended Hitler's mother, but his question, "why did Hitler himself become an anti-Semite?" I think really begs the question.

Psychoanalytically informed biography is of limited use to the historian. As Professor Langer remarks in the foreword, "The great hope . . . must be for help in understanding the group or mass actions of the past" (p. viii). Unfortunately the four remaining papers in this collection offer no help at all.

Two contributors address themselves to theoretical and methodological problems, but to little effect. On theoretical matters Robert Waelder adds nothing to Freud, while Waelder's interpretations of historical events go little beyond casual observation. As for the strange series of minilectures offered by the

editor, Benjamin Wolman, there is little to be said. Destrudo rides again.

Ronald Grimsley summarizes the views of several French literary critics more or less indebted to Freud. Robert Lifton, who draws his most telling examples from imaginative literature, has something more original to offer. He is "convinced that a universally shared style of self-process is emerging" (p. 34), a process "characterized by an interminable series of experiments and explorations—some shallow, some profound—each of which may be readily abandoned in favor of still new psychological 'quests'" (p. 37). This view is not so much argued as enlarged upon in provocative and entertaining fashion. Perhaps Lifton's shrewdest insight is that the "equation of nurturance with a threat to autonomy" is "a major theme of contemporary life" (p. 45). He intends his essay "to be no more than a preliminary statement of an idea" that he hopes "to pursue more thoroughly in the future" (p. 33). Perhaps something will come of it. Meanwhile the reader interested in history and psychoanalysis might see Loewenberg's paper recently published in the *AHR* (76 [1971]: 1457-1502), "The Psychohistorical Origins of the Nazi Youth Cohort." Do not skip the footnotes.

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PAUL K. CONKIN and ROLAND N. STROMBERG. *The Heritage and Challenge of History*. New York: Dodd, Mead and Company. 1971. Pp. ix, 279. \$4.95.

In the past historians whose creative energies had waned would occasionally pause to reflect on the history of their discipline, producing encyclopedic volumes that did little to enhance their scholarly reputations. Now much younger scholars at the height of their creative powers are expressing in print their thoughts concerning the nature of their chosen discipline, commenting on its noble heritage and uncertain future. Such a shift has probably occurred because, as Conkin and Stromberg assert in their preface, "historians today feel more disquietude than at any time in our recent past." Such a "crisis of confidence" results in, among other things, much self-analysis. It no longer suffices

to let old men (or philosophers) ponder the nature of history.

As the title indicates, this particular effort to still the disquiet within the profession consists of two quite distinct parts. The first segment, written by Stromberg, is essentially a brief history of history; the second, written by Conkin, is an analytical piece similar in character to much of the literature on history written by philosophers of science.

In part 1 historians such as Herodotus and Tacitus are dealt with in a page or less each, the history of history for the entire ancient world requiring only sixteen pages. Unless the reader can bring to the material a rather substantial understanding of history and ancient cultures, it is questionable what purpose these introductory chapters serve. Anyone interested in the contribution to historical consciousness of the early Hebrews and Christians, for instance, would do much better turning to the writings of John Marcus or Paul Tillich. The treatment of historical scholarship since Ranke is more satisfactory and leads the reader into the issues that now trouble the profession. Stromberg's attempt to point out why the times are now more favorable to historical as contrasted with social science approaches is not very convincing. While the critique of scientism has accelerated of late, there is little reason to think that traditional historical approaches will be viewed as appealing alternatives.

In part 2 Conkin defines history as true stories about the past. Without raising the old relativists' doubts about an incomplete story ever being a true story, one can ask whether it is any longer adequate to equate history with the construction of narratives. Much that historians now do can be considered storytelling only if one has a most fertile imagination. But the key to Conkin's theoretical position is his attempt to establish the unique qualities of historical thinking. It places him within the confines of straight-line history as contrasted with some adventure toward a radical redefinition of historical scholarship. The focus is on epistemological questions, on matters of perceiving and knowing human experience, and on the relevance of such questions for written history. In five short, very carefully written chapters Conkin confronts the central theoretical issues

associated with causation, generalization, objectivity, and the human use of history.

Many will finish this volume reassured that historians need only continue down those paths clearly laid out by their eminent predecessors; some will be less certain that this represents an adequate response to the current "crisis of confidence."

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GEORGES LEFEBVRE. *La naissance de l'historiographie moderne*. Preface by GUY P. PALMADE. (Nouvelle bibliothèque scientifique.) Paris: Flammarion, Éditeur. 1971. Pp. 348. 38 fr.

NORMAN F. CANTOR. *Perspectives on the European Past: Conversations with Historians*. New York: Macmillan Company. 1971. Pp. xix, 359, 362. Cloth \$11.95, paper, in two volumes, \$4.95 each.

L. P. CURTIS, JR., editor. *The Historian's Workshop: Original Essays by Sixteen Historians*. New York: Alfred A. Knopf. 1970. Pp. xxiv, 326. \$7.95.

There was a time when historians were reluctant to write about historiography—whether in the guise of history of history, the methods of history, or philosophy of history. To judge from the pages of our professional journals this time is now over; historians are in a retrospective, analytical, and speculative mood. That is not altogether a bad thing; surely an unexamined profession is not worth pursuing. But the reflective and analytical books and articles now proliferating cannot all be magisterial contributions to our understanding of our craft. Certainly none of the three books under review is likely to advance our self-knowledge a great deal, though one of them, L. P. Curtis's collection, contains much suggestive material.

The publication of Lefebvre's course on the rise of modern historiography is largely an act of piety. The book is the "mise au point" of some of Lefebvre's last lectures at the Sorbonne in 1945 and 1946. Since Lefebvre was a great historian this posthumous collection is welcome simply as a product of his pen, especially since his lectures as printed here retain the personal tone, the informative aside, and the occasional ripe aphorism that he generally banished from his more formal work. But as a



history of modern history it is a distinct disappointment: compelled to move after two introductory chapters from the Renaissance to our day in slightly more than three hundred pages, Lefebvre sounds summary, downright breathless. He treats—or rather dismisses—Jacob Burckhardt in thirteen lines; even Karl Marx, one of Lefebvre's favorites, must make do with four pages. Such a hasty overflight makes superficiality inescapable. Beyond this, for all his interest in sociological history, Lefebvre here seems distinctly old-fashioned. Perhaps if he had depended less on Eduard Fueter's antique and unreliable *Geschichte der neueren Historiographie* (the "classic" work that, the editor tells us, served as the basis of Lefebvre's lectures) he might have ventured more freely into new fields of interpretation. As it is, we have a useful and brief reference work with some interesting asides.

Norman Cantor's more ambitious compilation of interviews with distinguished ancient and European historians is neither brief nor particularly useful. The only value of this strange production—and it is doubtless considerable—emerges from the occasionally inspired responses to Cantor's jejune interrogatives. Thus, Leonard Krieger manages to say something new about the emergence of German nationalism, and A. William Salomone ventures some fascinating speculations about nineteenth-century European culture. There are nuggets elsewhere in this ponderous volume; since its list of thirty-two contributors reads like a *Who's Who* of historians—including such old masters as Ronald Syme, R. W. Southern, A. G. Dickens, Asa Briggs, and Gordon Craig—it could hardly be without them. But what purpose is this designed to serve? It cannot sustain comparison with its pendant, John A. Garraty's *Interpreting American History: Conversations with Historians* (1970). Garraty, after obviously doing much careful homework, asked probing questions about the state of the field, the direction of research, and the particular contribution of each respondent. The result was an informative, often highly interesting, set of conversations that stand as supplements to textbooks and specialized monographs. Cantor's compilation, on the other hand, is a rival to the textbook, rather than its companion. His brisk and per-

functory questions—"Did the rise of industry completely transform English society?"—generally elicit fragments of potted short histories on various areas of ancient and European history; we can get the same subjects treated in more lucid and orderly fashion—often by the very historians here interviewed—in other places. On occasion, indeed, some of Cantor's respondents make stabs at evaluating the historical literature, with some questionable results. Frank Manuel gives it as his opinion that "for a new student in the field" of the Enlightenment "I would say that Becker's brilliant essay is still the best single work from which to get a feel for the eighteenth century." Becker was a sensitive and literate historian, but it is precisely the "feel" of the eighteenth century that we fail to get from him—if by "feel" we mean a sense of what the age was actually about. "*The Heavenly City*," Manuel continues, "is a major essay: it is profoundly respected even by Europeans who have written longer works on the same subject. It has been attacked, of course, for its errors in detail, but Becker wouldn't have cared." I can only wonder at such peculiarly unprofessional idealization: the essay has been attacked for far more than "errors in detail," as Manuel must know; but even if it had been attacked only for that, Becker would have done well to care—does good history not depend on accurate detail? Of course, opinions like these are not Cantor's, the interviewer's, fault—the introduction and headnotes, on the other hand, are; apart from the factual information they contain, they are patronizing, tasteless, and embarrassing.

Curtis's collection of original essays is embarrassing as well, but on purpose. Curtis has had a new idea, which I can only applaud—to ask historians to talk freely about their "frame of reference" and their way of working that emerge in the writing of one of their books. Since historians' autobiographies are rare and on the whole unrevealing, the spectacle of a historian—or, as here, sixteen historians—in undress is bound to be a startling sight. While the essays here collected are of varying merit, the enterprise cannot help but be immensely useful; if it encourages other historians to bare their ways of thinking, their false starts, their passionate convictions and occasional disap-

pointments, the profession will be all the richer for it.

Since the middle of the nineteenth century, since William Whewell and Claude Bernard, students of scientific procedure have argued against the Baconian notion that we compile facts first and make our theories later; in recent years philosophers, historians of science, and distinguished practitioners, like Karl Popper, Thomas Kuhn, and P. B. Medawar, have strengthened our theoretical understanding and extended our factual knowledge of how natural scientists actually work; they have firmly discriminated between the logic of science (which tells us how a statement can be proved to be true) and the psychology of scientists (which tells us how they arrive at their statements in the first place). We urgently need a similar psychology of historians—perhaps more urgently than a psychology for history, a subject that threatens to engulf us these days. A psychology of procedure requires large amounts of factual information, and the present collection of partial autobiographies makes an admirable start. Autobiography is an art, and most of Curtis's collaborators—the fifteen of fifty-two who finally accepted the editor's invitation—are a little uncomfortable with it. Yet most of them manage to be informative, if not always wholly by intention. It would be invidious to single out any particular essay as "the best"; given the nature of the enterprise, each reader must choose the essay, or essays, he finds most instructive. My own favorites are Lynn White's witty and forthright account of his growing interest in the function of technology in civilization, John William Ward's economical and self-critical reappraisal of his study of Andrew Jackson as a culture hero, and the late Joseph Levenson's thoughtful explanation of his intentions in writing his great trilogy on Confucian China. Other contributors shed light on their work—R. R. Palmer on the *Age of the Democratic Revolution* (1959), John Pocock on his fascination with time, George Rudé on his long-term occupation with crowds. Sometimes the light is a little lurid: in the course of psychoanalyzing his psychoanalytical biography of Lou Salome and his subsequent work on Leopold III of Belgium, Rudolph Binion casually connects literature with life through the situation of "a young lady of my

close acquaintance" who "reenacted a traumatically unfulfilled love after five years of brooding over it." After she had gone "to pieces this second time round," Binion "psychoanalyzed her at her urgent request—and in the process solved, until further notice, my Lou-Leopold problem left outstanding." L. P. Curtis has opened Pandora's box; we may expect further revelations with trepidation but also with real interest.

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HELMUT HIRSCH. *Lehrer machen Geschichte: Das Institut für Erziehungswissenschaften und das Internationale Schulbuchinstitut. Ein Beitrag zur Kontinuitätsforschung.* (Schriftenreihe zur Geschichte und politischen Bildung.) Ratingen: A. Henn Verlag. 1971. Pp. 264.

In 1969 the American Historical Association cosponsored the publication of Donald W. Robinson's *As Others See Us: International Views of American History*, in which successive American history topics were treated by historians from different nations. The result was a startling lesson in relativism. There are apparently several versions of the history of the French and Indian Wars: the American, the English, and the French; the Indian version remains to be written. As the international community becomes more closely knit, attempts will have to be made to give history a global perspective.

Efforts to write history without national bias began after World War I. In 1925 the Scandinavian countries concluded the first agreement for revision of textbooks by mutual discussion. In 1933 the first international treaty agreed upon by Brazil and Argentina provided for periodical textbook revision. By 1938 twenty-two nations had some form of agreement. Germany and France reached one in 1935.

This book describes one significant German attempt to play its part in textbook revisions of the post-World War II era. The origin of the International School Book Institute located at Brunswick is to be found in the Research Institute for Education founded there in 1930 by August Riegel. Riegel was later dismissed from his post by the Nazis and the institute was put in abeyance. But the library of the foreign history texts accumulated by Riegel found its way

into the hands of George Eckhart and became the nucleus of the International School Book Institute he founded in 1949.

The purpose of the institute is quoted as being inspired by a Polish historian who crisply exhorted his colleagues "to try to eliminate all tendentious meanings from history." Actually the more immediate motivation was to redeem Germany from having been "diverted into narrow channels" by the Nazi interlude.

The book proceeds to describe in great detail the circumstances of the foundation and development of the institute. In furthering his work George Eckhart sought the support of several organizations both inside and outside his country. By 1965 the institute became affiliated with the Council of Europe. Its present name is Schulbuch Zentrum für Geschichte und Geographie der Länder des Europarats.

The functions of the institute consisted in organizing conferences, collecting library materials, rewriting textbooks, and making representations to other countries for rewriting textbooks. The book recounts instances of Belgian and English hostilities, the East-West controversies, and clashes with the Americans. In spite of such difficulties Eckhart, who, in addition to the directorship of the institute and a professorship at the Kant Hochschule at Brunswick, also served as member of the German Commission to UNESCO, was able through his contacts to resolve successfully mutual revision conflicts with countries such as the United States, France, Belgium, and Japan.

The book is unfortunately replete with irritating hints of the author's personal vanity as well as excessive local and institutional pride and German national patriotism. This mars not a little the fine story of a courageous and serious attempt to denationalize history.

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LOUIS GOTTSCHALK *et al.* *The Foundations of the Modern World.* (History of Mankind: Cultural and Scientific Development, Volume 4. Prepared under the auspices and with financial assistance of the United Nations Educational, Scientific and Cultural Organization.) New York: Harper and Row for the International Commission for a History of the Scientific and

Cultural Development of Mankind. 1969. Pp. xxxi, 1133. \$20.00.

The volume at hand is the fourth in UNESCO's attempt to examine mankind's cultural and scientific development as a single story. Quite apart from any objections to the implied distinction between science and culture, I feel compelled to register serious doubts about the entire enterprise as conceived and executed.

First, however, one should be clear about the conception itself. The preceding volumes deal, respectively, with "Prehistory and the Beginnings of Civilization," "The Ancient World," and "The World, A.D. 400 to A.D. 1300." The succeeding two volumes are entitled "The World in the Nineteenth Century" and, forthrightly, "The Twentieth Century" (when the definition of the world obviously became complicated).

Certain problems presented by this fourth volume will be readily apparent to most students of history. The author-editors, who generously acknowledge the critical collaboration of no fewer than sixty-two consultants, including seven who appear to have been most deeply involved, were doubtless justified in treating the period 1300-1775 as one having a genuine coherence. Most of us in the European field are uneasy when we are cut off too sternly from the Middle Ages. The record of an "old Europe" often challenges our standard periodization. The troublesome question remains whether the record of other civilizations helps very much in this regard.

What is still more troublesome, however, is the decision of Professor Gottschalk and his colleagues to adopt a principle of organization that seems to me not to work. After roughly 100 pages of political summary we confront over 350 pages of religious history, sometimes tinged with metaphysics. Then, in succession, come paired chapters on political and social thought, "literary communication and belles-lettres" (an uneasy distinction), and the visual arts and music. Finally, there are 250 pages on science, technology, and education.

Granted that every historian has to make choices all the time between topical and chronological approaches, the line followed in the volume under review is so defiant of history's wholeness as to cast doubt on the very idea of a

"history of mankind." To take one example, few students, reading early in the book about Jansenism as a theological position, will be able to remember its role (with Gallicanism) when they get back to seventeenth- and eighteenth-century political thought some hundreds of pages later.

The author-editors deserve credit for having faced up to this difficulty and even, in a preface "Note on the Preparation and Editing of Volume IV," having reprinted the objections of Professor Roland Mousnier. Those objections, which still appear to me valid, include the following: "The links between the facts studied in each case and those considered in other categories are rarely shown. . . . There is lacking a general view of all these human activities" (p. xviii). Professor Gottschalk answers this complaint quite reasonably by pointing out that every scheme of organization involves a trade off, a surrender of some advantages to secure others. On balance, however, it seems that Professor Mousnier is right in pressing his query: Is the only object of this exercise an encyclopedic work of random reference?

The dissatisfaction that remains with me, however, has a different source. It has to do with the very idea of "world history" dominated by the representational structure of the United Nations and written by negotiation, which has its place—elsewhere. Asian history, African history, and Latin American history all deserve to be studied rigorously in their own terms. Those terms need not, and should not, exclude careful comparisons with other fields.

What appears to me to happen, however, when sixty-five historians, including seven Russians, a Syrian, an Israeli, two Indians, five Hungarians, and two South Vietnamese, "collaborate" with dozens of other scholars from other lands is that history, as an assignment of recovery and interpretation, simply gets lost. One final illustration is in order. Perhaps, given limitations on space, British political development between 1300 and 1775 could not be assigned more than the two paragraphs it receives on page 12. But I have yet to be convinced that England in those centuries was less significant to the history of mankind, *all mankind*, than were Mandingo, the Mali Empire, Kano, and the Ashanti federation, dealt with earlier and at greater length (pp. 2-4).

The above expresses no racism, of any of the currently available varieties. It does express doubts about history as a product of bargaining. Soon after the Second World War some German and French historians got together, contritely, and agreed that Louis XIV was rapacious but a prince of his time. They also agreed that the Franco-Prussian War was both sides' fault. The propositions were doubtless true, but neither advanced historical studies very much. The global approach has, so far at least, offered us nothing better.

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*Third International Conference of Economic History, Munich, 1965.* Volume 2. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Congrès et colloques, Number 10.) Paris: Mouton & Co. 1968. Pp. 304. 48 fr.

The twenty-seven papers and the ten discussions assembled in this volume represent the combined output of only one section (Agricultural Production and Productivity) of the Third International Conference of Economic History, which met in Munich in 1965. Despite the sometimes heroic efforts of the late Jean Meuvret (who was responsible for the section) to impart cohesion to such a large and disparate body of papers, it is patently impossible to seize upon a single theme other than a common interest in the measurement of agricultural production and some explanation of the vicissitudes of agricultural growth in various areas of the world. Needless to add, all areas are not treated in proportion to their importance to agricultural history. The European world clearly dominates (twenty-one of the twenty-seven papers); Japan is represented by two papers; the United States, Mexico, Argentina, and India by one each. Without treating an international conference of historians like the Washington Naval Conference and recognizing that availability of sources and research talent must play a determining role in the assignment of papers, greater effort should still be made to solicit research from economic historians of Asia, Africa, and the Americas at future international conferences, especially in this area of agricultural economics and rural sociology. And surely, to have only two short

papers on England and none on the Low Countries—the pioneers in agricultural change—is unfortunate in the extreme.

About half of the articles in the collection treat such problems as the completeness and authenticity of sources (the relative value of private account books, church *dîmes*, and public fiscal records), the arithmetic of crop yields (by seed, by area, by manpower), the relation of crop rotations to livestock, and the relation of area, soils, population density, and land tenures to mechanization and nonmechanical innovation. No doubt, as a result of the Munich conference we know more about how to measure productivity—at least of cereals. But with the exception of R. A. C. Parker's brief paper on the Coke estate in Norfolk, the papers treating Western Europe are not primarily concerned with landlord-tenant relations, reinvestment rates, or the broader social aspects of agricultural production. The representatives from Central and Eastern Europe, on the other hand, were less hesitant to enlarge the scope of their assignment, often tracing the agricultural history of entire countries over several centuries. Thus, V. K. Yatsounski of Moscow's Academy of Sciences presents a seventeen-page paper on Russian agricultural production from 1500 to 1917, while László Makkai of the Institute of Social Sciences in Budapest presents a nine-page résumé of Hungarian agriculture in the "era of late feudalism," that is, from 1550 to 1850. Contrast this perspective with Emmanuel Le Roy Ladurie's nine pages on "Grain Yields in Languedoc," replete with tables and tight arithmetic.

As examples of economic history pursued in Eastern Europe, consider the papers of Yatsounski and François Matejek on Russian and Czechoslovakian agriculture in the four centuries from 1550 to World War I. Their use of a threefold periodization of agrarian history—"feudal," "capitalist," and "socialist"—creates some initial difficulties, since it is not always clear whether political, ideological, or economic events determine the transition from one epoch to the next. In these two papers the emancipation of the serfs in 1848 and 1861 ushers in the *époque capitaliste* and initiates major changes in the rate of agricultural growth. Matejek establishes a veritable "agricultural revolution" in the thirty years after

1848, punctuated by the planting of new forage crops, the near elimination of the fallow, and the introduction of the potato and sugar beet along with expansion of such derivative Czech industries as brewing, distilling, and starch-making. Yatsounski traces a similar, if less dramatic development in Russian agriculture after 1861, but he attributes much of it to the initiative of the larger landlords rather than to Matejek's state agricultural schools, which Matejek claims spread technical know-how to the *peuple*. Yatsounski in fact believes that the agrarian policies of the tsarist government that aimed at increasing the number of small holdings actually "inhibited productive forces on the land." His colleague, A. M. Anfimow, makes the same observation about the Stolypin reforms of 1906-14. Matejek, on the other hand, not only refuses to condemn the small holder, but he even attributes substantial increases in bovine livestock in the 1870s to the *paysan* rather than to the large landlord. It would appear that Matejek shares some of Jean Meuvret's respect for "agrarian individualism"—even "petty bourgeois individualism"—as alternative promoters of agricultural growth.

Economic issues are not always kept distinct from a bias in favor of agrarian populism. All papers concerning Eastern Europe agree that serfdom was the great stumbling block to agricultural progress—so great in fact that it obscures other possible obstacles. Furthermore, I am not convinced that the system of *latifundia* per se (and one would like to know more about areas and management) blocked innovation on the land. Some attention should be given to the psychology of landlords. It may well be that the Polish magnates spent all of their profits from grain exports on luxury goods from the West, but, as the English example suggests, large holdings were not always starved for reinvestment capital. In one of the discussions Pierre Vilar suggested that there may have been an aristocratic *laissez-aller* psychology (as in Spain) that remained after the formal seigniorial system had gone but before the new "mentality of capitalist exploiter" (or entrepreneur?) had arrived. Perhaps there are landlords and there are landlords, regardless of the system of land tenure. But psychological aspects of the problem (despite the existence of volu-

minous private estate papers) were lightly treated, to say the least. Miklos Szuhay of the Karl Marx University in Budapest seemed to capture the main thrust of his own and his colleagues' interpretation when he concluded that "real" agricultural growth could only take place "by breaking up the system of large [private] landholdings . . . and, above all, by the socialist reorganization of agriculture." Does this mean that there are "efficient" and "inefficient" forms of large-scale agriculture? In any event, there is not much place here for the emancipated peasant, either as independent farmer or as promoter of agricultural growth.

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ROBERT SCHWOEBEL, editor. *Renaissance Men and Ideas*. New York: St. Martin's Press. 1971. Pp. xxii, 137. Cloth \$6.00, paper \$2.95.

This collection is designed "to bridge the ever-widening gap between the world of the specialized scholar and that of the undergraduate student, between the scholarly journal and the textbook." In his introduction Robert Schwoebel performs the impressive feat of touching on general problems raised by the concept of the Renaissance and bringing together the common themes of the nine articles that follow. Without talking down to the book's intended audience or making a cheap plug for the relevance of the Renaissance he indicates that thinkers of that period concerned themselves with perennially perplexing problems: "the nature and uses of power, the responsibility and possibility of education, the limits of reason, the sources of moral authority, the social and ethical implications of scientific discovery and technology." The serious but vigorous tone he sets is maintained throughout the volume; all the contributors write well and offer sound and illuminating interpretations of their material.

*Renaissance Men and Ideas* includes the following studies: "Renaissance Humanism: Petrarch and Valla" by Jerrold E. Seigel, "Printing and the Spread of Humanism in Germany: The Example of Albrecht von Eyb" by Rudolf Hirsch, "The English Renaissance: Sir Thomas More" by George Richard Potter, "Machiavelli" by Felix Gilbert, "Pius II and the Renaissance Papacy" by Robert Schwoebel, "Lu-

ther as Scholar and Thinker" by Lewis W. Spitz, "Copernicus and Renaissance Astronomy" by Edward Rosen, "The Paideia of a Renaissance Gentleman: Castiglione's *Book of the Courtier*" by Roslyn Brogue Henning, and "Montaigne on the Absurdity and Dignity of Man" by Donald M. Frame. All are of very high quality, and intelligent undergraduates should find each interesting and informative. I felt that two of the studies were especially helpful in putting more or less familiar material into a new and more viable pedagogical framework. Other essays in the volume will no doubt seem particularly enlightening to scholar-teachers with different backgrounds and interests.

Of all the contributions to a collection where the emphasis is on intellectual history Hirsch's study comes the closest to hinting at the insights that quantitative approaches can bring; it also helps to confirm Elizabeth Eisenstein's recent assertions. His thesis that Northern Europe was not as far behind Italy in concern for the classics as has usually been supposed is supported by impressive lists of German first editions of classical texts put out for the most part by printers who did not specialize exclusively in scholarly works. A consideration of the *oeuvre* of one early figure, Albrecht von Eyb (1420-75), suggests further that some modification of the traditional notion of a time lag in northern humanism needs to be made. Primarily a popularizer, Eyb in his *Margarita poetica* and *Ehebuch* wove together a tissue of classical citations and extracts designed for practical application. The fact that these books reached a very extensive audience (an estimated 4,000 copies of the *Margarita* and 7,000 of the *Ehebuch* were published) indicates that the new medium of printing provided some degree of exposure to humanist ideas for Germans not fortunate enough to travel to Italy or to participate in aristocratic learned sodalities.

In the study on Machiavelli we find a scholar's distillation of many years' research on a persistently puzzling subject. Not only does Felix Gilbert demonstrate complete mastery of the immense literature on Machiavelli; Gilbert provides a key to understanding why almost every intellectual generation has felt a need to reassess the meaning and intention of his works. In Gilbert's view (as in Isaiah Berlin's) modern scholarship can come closer than that of pre-

vious epochs to taking Machiavelli's stark world view at face value because the twentieth century, like the early sixteenth, is a time when old certainties no longer hold. The central consideration in Machiavelli's writings, Gilbert maintains, is power in the context of the ruler-subject relationship—power that, when effectively institutionalized, can prolong the life of a state at the apex of the inevitably recurring cycle of historical development. In Machiavelli's age "the feeling of having lost control over events was dominant": hence his obsession with the redefinition of the political world as a realm of flux and with the revaluation of "virtue" as the ability to control men and events. Gilbert's essay, along with the others in this volume, is a solid, provocative reinterpretation that has much to offer the specialist as well as the neophyte in Renaissance studies.

ANNE JACOBSON SCHUTTE  
Lawrence University

J. G. ROWE and W. H. STOCKDALE, editors. *Florilegium Historiale: Essays Presented to Wallace K. Ferguson*. [Toronto:] University of Toronto Press, in association with the University of Western Ontario. 1971. Pp. xiii, 401. \$16.50.

Sixteen scholars have contributed to honor Wallace K. Ferguson on his seventieth birthday—fourteen (one of them the honorand's brother) with articles on different aspects of the Renaissance and two as careful, imaginative editors. The product of this energy is the sort of book I like: 246 pages of text and 117 pages of notes and apparatus, not counting illustrations. Furthermore, the notes are largely to primary materials, manuscripts, and old books; and the articles bear on Ferguson's seminal work on the Renaissance and the idea of the Renaissance, in and beyond historiography. This means that the contributors have consciously worked to clarify problems Ferguson raised in his books and articles: two (Paul Kristeller and Myron Gilmore) write on Erasmus; four on humanists (Hans Baron on Petrarch, Eugene F. Rice, Jr. on Lefèvre d'Étaples, Arthur Ferguson on early Tudor humanists, and Denys Hay on the geographical humanists who contributed to "the idea of Italy"); and two (Millard Meiss and Edward Lowinsky) on the arts. Frederic Lane and Felix Gilbert offer

precise delineation of problems in Venetian politics and historiography; Nicolai Rubinstein provides a quiet, brilliant analysis of Machiavelli's uses of the variously loaded word *stato*; J. R. Lander analyzes the relations of the first two Tudor monarchs to their troublesome (and essential) magnates; finally, J. R. Hale presents another folio on his perennial topic of Renaissance wars and rumors of wars, this time a study of the sermons dealing with warfare from the Armada to the Civil War.

The articles are very different from one another. Both Baron and Rice deal with what Renaissance scholars call "medievalism," Baron with Petrarch's Augustinianism, and Rice with Lefèvre's publications of medieval mystical texts, but Baron does not quite define the important revision of Petrarch's life that he offers, though Rice is clear about what Lefèvre's industry meant in French intellectual life. In another context Myron Gilmore presents an unquestionably disputatious Erasmus, never letting pass an opportunity in his own defense, who, at the article's end, folds his hands in the irenic gesture historians prefer to see him take. Arthur Ferguson's graceful article recapitulates much from his 1962 book but lays special stress on the English humanists' sense of history, here interpreted as man's emergence at the beginning of his development from a disagreeable state of nature, rather than as history in any modern sense.

Lane's and Gilbert's articles are models in their kinds; both show how and why things happened—why the Venetian council had to be enlarged and how that enlargement was effected; why Venetian historiography in fact took a radically new turn in the Renaissance, for reasons laid out by Mr. Lane. Meiss's article is an iconographical contribution that abides by its stated limitations; Lowinsky manages as ever to speculate beguilingly, this time via Anne Boleyn and her music man Mark Smeaton. Rubinstein's article is a model of intellectual analysis, giving far more than it promises; Lander's researches make even plainer than before the forcefulness of Henry VII in a very ticklish situation indeed. Marvin Becker's study of the "quest for identity" in the early Renaissance develops a theme on which I have earlier heard him lecture: despite citations to the right authors (Ladner, Leff,

Kantorowicz, Trinkaus, Garin), the use of "in" phrases (identity crisis, ego support, socially-defined roles, civic persona, scheme of ceremonial identity), and references to topics like love, chivalry, historiography, the Church, government, and commercial enterprises, I had been unable to understand his premises or the order of his argument when I heard them spoken. Now that I read this article (note that I am not criticizing what some historians may consider sociological jargon; that I think I can understand and certainly find useful), I find that my earlier deafness has become blindness. I don't know who the early Italian Renaissance was that it should seek its identity: one has to be careful with this sort of subject, when even Huizinga often nods. Becker has, however, tried to synthesize some of the many problems Ferguson has frequently referred to: his is the zeal of the house. For its many different virtues scholars will have to use this *Festschrift*; what is more telling, Wallace Ferguson will use it—and what nicer present could a man hope to receive on his birthday?

†ROSALIE L. COLIE  
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ERNEST F. DIBBLE and EARLE W. NEWTON, editors. *Spain and Her Rivals on the Gulf Coast*. (Proceedings: Second Gulf Coast History and Humanities Conference.) Pensacola: State of Florida, Department of State; Historic Pensacola Preservation Board. 1971. Pp. vi, 143. Cloth \$5.00, paper \$3.00.

During the three centuries of history from 1513 to the early 1800s three European powers, Spain, France, and England, contended for supremacy in the territory from the Florida peninsula to Texas. In this struggle Spain appeared first and led the way in promoting exploration and colonization, but the rival nations subsequently entered the contest for primacy and eventually made their own significant contributions in the creation of the patterns of life so characteristic today of this part of the American scene.

At the Second Gulf Coast History and Humanities Conference, held in December 1970, the five formal papers read, which constitute principally the subject matter of the present publication, are all related to the colonial beginnings of this Gulf region and are also "in-

terdisciplinary," as a glance at their titles will reveal. They are as follows: "Gulf Coast History: An Overview" by Alfred Thomas; "French, Spanish, and English Indian Policy on the Gulf Coast, 1513-1763: A Comparison" by John J. TePaske; "Revolt in Louisiana: A Threat to Franco-Spanish Amistad" by J. Preston Moore; "The Spanish Gulf Coast Cultural Assemblage, 1500-1763" by Hale Smith; and "Gulf Coast Architecture" by Samuel Wilson, Jr. Also included is an appendix prepared by William S. Coker and Jack D. L. Holmes dealing with the sources for the history of the Spanish borderlands.

The reader of these scholarly studies is likely to increase his understanding and appreciation of the Gulf Coast and its history. Perhaps he may also come to recognize with Dr. Thomas (and as did Herbert Bolton and others, including myself years ago) the grave error of the traditional school of historians who view the origins of American civilization "almost exclusively in terms of 13 English colonies along the Atlantic Seaboard." How ironical this position is when, in fact, "the vast region from Florida to California" had its own earlier and separate colonial beginnings! It is well to add that this publication is greatly enhanced by the inclusion, in connection with Wilson's paper, of thirty-two pages of illustrations of colonial buildings, among them fortifications and other public and private structures.

VERNE E. CHATELAIN  
University of Maryland

ERNEST S. DODGE. *Beyond the Capes: Pacific Exploration from Captain Cook to the Challenger, 1776-1877*. Boston: Little, Brown and Company. 1971. Pp. xv, 429. \$12.50.

"Geography," said the eighteenth-century French explorer Louis de Bougainville, "is a science of facts" (quoted in J. C. Beaglehole, *The Exploration of the Pacific* [3d ed.; Stanford, 1966], 3). The history of geographical exploration is thus a branch of narrative history, nowadays out of fashion, consisting as it does of stories of high adventure, of the deeds of men who hazarded their lives to bring back the news of far-off lands. In this handsome book Ernest Dodge has told the story of the final century of geographical exploration in the Pa-



cific by Europeans and North Americans. Beginning with the broad general outline of Pacific geography established by James Cook, Dodge groups his explorers by nationality and treats the discoveries of nearly all the major voyages through H.M.S. *Challenger's* circumnavigation of 1872-76. (The major omissions are Russian voyages for which there are no translated accounts.) Beaglehole's book, which served Dodge as a model, follows the three-century-long quest for *Terra Australis Incognitae* until Cook's facts exposed it as myth. Unlike Beaglehole, Dodge has no thread running through his account. The result is good, old-fashioned, narrative history. The author has gone directly to the explorers' accounts (or their English translations), from which he quotes liberally without interrupting the narrative flow, paying especial attention to their ethnographic observations. These tales, some lurid, some sober, were eagerly seized upon by those who stayed at home, uneasy enough about the degree of civilization in their own societies to enjoy reading about unenlightened primitives. Commerce and politics also helped launch the sailing vessels, which, as they roamed round the world, found hitherto unknown Pacific shores. Furs from the Northwest coast of North America, *bêche-de-mer* (an edible sea cucumber) and sandalwood from the Pacific Islands, all were exchanged in China for tea, silks, and porcelains. New geographic knowledge came also from the hunt for seals and whales. The flag followed trade into the farthest corners of the Pacific, most often to survey the coasts and make them safe for commerce, sometimes, as in the expeditions of von Bellingshausen and Wilkes, out of that pure quest for knowledge which is science.

Dodge's book is largely an account of the white man's encounter with savages, told from the white man's point of view with a few comments from the author (e.g., his condemnation of the brutality of Captain Charles Wilkes, U.S.N., in the Fiji Islands in 1840, [pp. 349-50]). Nothing is seen from the natives' viewpoint (cf. H. E. Maude, *Of Islands and Men* [Melbourne, 1968]), nor is the explorers' discussed critically (cf. Bernard Smith, *European Vision and the South Pacific 1768-1850* [Oxford, 1960]). The author has reproduced in black and white fifty-eight representative plates

from the contemporary printed narratives of the voyages he recounts; his one color plate (also the dust jacket) is an inauthentic modern painting of a Royal Navy surveying vessel. Though the maps are exceptionally well drawn they are inadequate. The reader who wants to follow the explorers' tracks to all the places Dodge mentions will need a superior atlas in front of him. Dodge provides a list of sources, arranged by chapters, in lieu of footnotes. A valuable chronological listing of voyages and their commanders, followed by an index wholly of proper names, completes the book.

HAROLD L. BURSTYN  
Carnegie-Mellon University

JACQUES FREYMOND, published under the direction of. *La Première Internationale: Recueil de documents*. Volume 3, *Les conflits au sein de l'Internationale, 1872-1873*; volume 4, *Les congrès et les conférences de l'Internationale, 1873-1877*. Texts established and annotated by BERT ANDRÉAS and MIKLÓS MOLNÁR, with the collaboration of CAROLE WITZIG and LADISLAS MYSYROWICZ. (Publications de l'Institut Universitaire de Hautes Études Internationales, Number 48.) Geneva: the Institut. 1971. Pp. xviii, 668; 835.

Nearly ten years ago the general editor of the two tomes under review issued the first two volumes of documents bearing on the history of the International Workingmen's Association from 1864 to 1872. The volumes before us continue the record of the association to its end in 1877. The first series of documents gave us only the official proceedings of the meetings and the reports relating to them; the second supplements the official account with accessory material. With an eye to comprehensiveness the editors have included such documents as the minutes and reports of local and regional federations, circulars of dissident groups, articles written for the press by outstanding men, letters that were only peripheral to the large body, even manifestoes of seceded sects. Now pieces of this character are undoubtedly valuable, for many of them have been difficult to locate. Still it may be asked whether many of these addenda really belong in the collection under consideration. For example, are the articles of Marx and Engels on anarchism directly relevant to the documentary story of the International? The same question may be asked

with respect to Cafiero's long letter of June 12, 1872, to Engels, however pertinent it is to anarchist theory. And finally does the Blanquist manifesto of 1874, *Aux Communeux*, which has little bearing on the International, dovetail in this assembling of papers, especially since its authors left the association some two years earlier?

These questions are not designed to derogate from the high value of the present collection. The annotations are extraordinary. There are nearly 2,100 notes spread over 390 pages. And they are not the usual notes. Many of them are longish clarifications of sources or provide biographical data on prominent and obscure members of the separate organizations, so that we have a kind of biographical dictionary of them. Much of the material was derived from manuscript papers or police records. The editors have often cited from dossiers in the Paris Prefecture of Police, and it may be assumed that they checked their findings with other sources. I still recall the amazing fantasies in such French police dossiers as those on the International in America and on Marx, alias Williams.

The present volumes span the five years following the Hague Congress, during which two battered and moribund Internationals, the centrist and the federalist, were vying for supremacy. The one was Marxist in orientation; the other, anarchist and autonomist, with the shadow of Bakunin hanging over it. The first held two congresses, one in Geneva in 1873 and another in Philadelphia in 1876. The Congress in Geneva turned out to be a fiasco, so much so that the General Council in New York was unable to get the full resolutions, not to mention a connected story of the proceedings. The Congress in Philadelphia was the swan song of the Marxist International.

The anarchist International held four congresses, starting with the one in Geneva in 1873. Despite reports of growth in Switzerland and Spain, it too was passing out. It was torn by differences over two major questions, that of public services (which inevitably brought to the fore the divisive issues of the state and political action) and that of the general strike (which many anarchists regarded as the open sesame of emancipation). By the time of the Brussels Congress of 1874 its substance was

going to pieces. Two years later only a remnant organization met at Berne. Delegates of what was left of it gathered at Verviers in 1877 in preparation for a joint meeting with socialists at Ghent.

The editors were well advised to conclude the collection with the Ghent Congress. Though it does not belong in the series of meetings held by either of the two Internationals, it represented an earnest effort to restore a form of unity. In the history of socialism it stands as the forerunner of the Second International.

A word about the bibliographical equipment. The editors have apparently drawn on many libraries, archives, and special collections of unpublished papers. This in itself represents colossal industry. They have furthermore furnished us with lists of publications and periodicals, which, despite many omissions, form one of the best bibliographies on the First International I have seen. It is not too much to say that the present volumes will be a reservoir of original sources for historians of labor and social philosophy.

SAMUEL BERNSTEIN  
Portland, Maine

LOUISE H. HUNTER. *Buddhism in Hawaii: Its Impact on a Yankee Community*. Honolulu: University of Hawaii Press. 1971. Pp. x, 266. \$9.00.

Students of Hawaiian history, sociology, and politics will appreciate *Buddhism in Hawaii*. While its nearest counterpart, Frances Hilary Conroy's *The Japanese Frontier in Hawaii* (1953), deals with the interaction of the two dominant groups—the Americans and the Japanese—in the political, economic, and social scene and the rising antagonism up to 1898, Mrs. Hunter emphasizes the vicissitudes of the Japanese Buddhists from the arrival of the first group in 1868 through the tensions of the mid-1950s. Most of the first 141 men, with 6 women and 1 child, went to work on plantations. Forty of the most dissatisfied returned to Japan, 36 journeyed to the United States, while 37 sojourned in the Hawaiian Kingdom. Some of these were converted to Christianity by devout Protestant or Catholic missionaries. The Reciprocity Treaty of 1876 with the

United States, heralding an enormous expansion of the sugar and related industries, pointed out the need for imported labor. King Kalakaua's visit with Emperor Meiji in 1881 resulted in a special agreement with the Japanese government for the resumption of Japanese immigration, and there followed thousands of contract laborers.

The first Buddhist priest arrived in 1889 and plans were promptly drawn for a mission hall and a Buddhist temple in the heart of Puritan Honolulu. Louise Hunter treats the initial setbacks and persistent obstacles Buddhist leadership encountered and the conflict between the Buddhist community and the Caucasian community in the changing political context of Hawaii. The narrative is neatly woven into the history of the kingdom, the republic, and the territory, along with the hatred engendered by Pearl Harbor, the mistreatment of *Nisei*, and World War II, which was totally disruptive of Buddhist activities. Although these activities were revived after the conflict, there was a suppression of Buddhist youth programs.

Mrs. Hunter has written a scholarly work within well-defined limits and has made a substantial contribution to the historical literature of Hawaii. Readers of her book will acquire a better understanding of the efforts and frustrations of the spokesmen of Buddhism in the Hawaiian Islands as well as an appreciation of the contribution of Buddhism to the community. "Its teaching made its followers good citizens, good neighbors, kindly men and women. No religion could be expected to do more" (p. 209).

MERZE TATE

Howard University

BERNARD FERGUSON. *The Trumpet in the Hall, 1930-1958*. London: Collins, 1971. Pp. 286. £2.25.

Sir Bernard Fergusson's memoirs make fascinating reading. He writes with great verve and skill, and one finishes his book with the feeling, not very common after reading the memoirs of retired soldiers, that it has certainly been worth the time. He entered the army in 1930 and retired in 1958, his career thus covering the British Army's last great war and the subsequent run-down of both that army and

the empire it served. His assessment of the strange figure of Orde Wingate, under whom Fergusson served from 1942 to 1944, is very interesting, as is his account of two tours in Palestine, first as an intelligence officer in 1937-38 and then while seconded as an assistant inspector general of the Palestine police in 1947.

The book is above all an important document on the social history of the British Army in Brigadier Fergusson's time. He came, like many British regulars of the last two hundred years, from Scottish landed stock. His father and grandfather had been soldiers. When he joined the Black Watch from Sandhurst in 1931, the minimum private allowance required was £ 250 a year, and, Fergusson records, "there were at least three subalterns with private incomes of over £ 2,000 a year, . . . and one married subaltern who kept a butler." He manages to convey, better than any military memoirist I have encountered, what a small clubby world the British regular army was. The strength of the regimental tradition is also given ample illustration. "The Black Watch was more like a religion than a regiment; I certainly always thought of myself as being in the Black Watch rather than in the Army." All of this makes his account of the years after 1945 rather poignant. Officer selection procedures change, the regimental tradition comes under attack, the "American rules . . . and American jargon" of SHAPE prove that "we, the British, were in very truth the junior partners of the Americans . . . we were no longer Equal Top Nation." The book appropriately comes to an end with the tragicomic Suez operation (during which Fergusson was a rather miscast director of psychological warfare). *The Trumpet in the Hall* is a monument—and a farewell—to the army that Bernard Fergusson knew.

RAYMOND CALLAHAN

University of Delaware

V. L. ISRAELIAN and L. N. KUTAKOV. *Diplomatiia agressorov: Germano-italo-iaponskii fashistskii blok. Istoriiia ego vozniknoveniia i krakha* [The Diplomacy of the Aggressors: The German-Italian-Japanese Fascist Bloc. History of Its Rise and Fall]. Moscow: Izdatel'stvo "Nauka." 1967. Pp. 434.

This is a semipopular diplomatic history of the Axis powers from the invasion of Poland to the

capitulation of Japan. It is remarkable neither for new data nor novel interpretations. Its chief interest for American historians is likely to be simply the demonstration it provides of how readily the summarized descriptions of individual events can be woven together to support different general interpretations. It shows how independent one's view of the macrocosm can be from one's view of the microcosms.

The book is framed in the orthodox Soviet Marxist-Leninist matrix of historical assumptions. The central body of the study, however, is remarkably free of obeisances to Marx and Lenin. The introductory chapter leads up to the Nazi invasion of Poland, following the conventional Soviet interpretation of 1930s diplomacy and focusing on Munich. At each critical point here a protective citation of party documents sustains the authors. The body of the study comprises twelve chapters running from fascist diplomacy during the period of "phony war," through the fall of France, the Tripartite Pact, the attack on the USSR, Japanese aggression in the Pacific, the turning of the tide of war in Russia, to the breakdown of the Axis and the end-of-war maneuvers of Axis leadership. The center of gravity of the book is, predictably and legitimately, the outlook and decisions of Axis diplomacy regarding the Soviet Union. The short conclusion draws a moral about the futile fiasco into which imperialism pushed the Axis at this juncture of the historical process, only to produce in the long range a strengthening of the forces of socialism.

The tone of the book is that of a straightforward outline without the loose edges of uncertain or perplexing areas. A clear answer is asserted for all questions. Personalities and coincidental determinants are de-emphasized, as appropriate to the philosophic stance adopted. Stalin's name appears, but infrequently and then usually in quotations from correspondence. The scope entails more survey than intensive and exhaustive historical inquiry. When one adds the characteristic of a tale fitted to a predetermined pattern that is never really questioned, there results a semblance of superficiality that is probably unavoidable given the context in which the book was produced.

But, accepting the limitations of Soviet canons of criticism and interpretation, this is not a trivial work. The twelve central chapters are

not propagandistic; they proceed in a clear, professional succession, creating a coherent synthesis. Heavy reliance is placed upon the Documents of German Foreign Policy, 1918-45. Other prominent sources include the records of the International Military Tribunal for the Far East and the Nuremberg Trials, U.S. Pearl Harbor inquiry materials, and the Soviet documentary collections of the war years. German and Italian memoirs have been used, and there are occasional references to Western secondary literature. The overall impression left is of a serious historical effort by competent scholars that pushes against rather stringent ideological constraints upon historical scholarship.

DAVID D. FINLEY  
Colorado College

MARK W. ZACHER. *Dag Hammarskjöld's United Nations*. (Columbia University Studies in International Organization, Number 7.) New York: Columbia University Press. 1970. Pp. 295. \$7.00.

DAVID A. KAY. *The New Nations in the United Nations, 1960-1967*. (Columbia University Studies in International Organization, Number 8.) New York: Columbia University Press. 1970. Pp. xiii, 254. \$10.00.

These two books, similar in weight and length, concerned with the same international organization, and published in the same series by the same publisher in the same year, could hardly be more unlike. They spring from very different investigative traditions, aspire to very different kinds of knowledge, ascribe historical momentum to very different sources, employ very different research methodologies, and arrive at very different types of conclusions, thus leaving the reviewer with very different assessments of their utility.

The Zacher monograph is best described as intellectual history. Based on a close examination of the writings and pronouncements of the United Nations' second secretary general, Zacher seeks to reconstruct the strategies and tactics employed by Hammarskjöld during his eight years in the office that are relevant to the peaceful settling of disputes, controlling the use of force, promoting arms control and disarmament, and building a more peaceful world order. In so doing Zacher implicitly ascribes historical causation to the will of individuals and relies exclusively on Hammarskjöld's views as

evidence of the way in which leaders make rather than reflect the forces of history. Thus in the end Zacher is content to conclude that Hammarskjöld was an important figure in the fledgling organization over which he presided.

The Kay monograph, on the other hand, is most appropriately characterized as behavioral analysis. Based on a close, quantitative analysis of roll-call votes cast in the UN General Assembly by new (post-1960) members, Kay attempts to discern the patterns of organization and influence through which the new nations express their concerns and move toward their goals. His assessment focuses on the participation of the new nations in the consideration of decolonization, human rights, economic development, East-West differences, and organizational issues over several sessions of the General Assembly. The result is an implicit ascription of causation to the behavior of large collectivities and the political processes through which they interact. The patterns of influence uncovered by the analysis leave little room for individuals as causal agents, with the result that Kay's conclusions deal with the capacity of the new nations and the United Nations to harmonize their interests and achieve their respective aims.

For me the Kay volume has considerably greater utility than the analysis by Zacher. Both the history and the future of the United Nations strike me as lying in the actions and reactions of many people in many groups in many situations. The secretary general is not irrelevant to those interactions, and certainly every facet of the United Nations should come under scrutiny, but in the long run, if not in the short run as well, the course of international history seems likely to be shaped mainly by the convergence of forces in which the talents, aspirations, and perspectives of particular individuals are of relatively minor importance.

JAMES N. ROSENAU  
*Ohio State University*

#### ANCIENT

RONALD JESSUP. *South East England*. (Ancient Peoples and Places, Volume 69.) New York: Praeger Publishers. 1970. Pp. 273. \$8.50.

The three southeastern counties of England—Kent, Surrey, and Sussex—contain some

of the most important archeological sites in the country, since they were the gateway for invaders from the Continent. Ronald Jessup's work provides a useful survey of prehistoric sites and artifacts uncovered in the region. He begins with the topological background then covers chronologically the findings from various cultures from earliest hunters and food-gatherers to the megalith builders, Bronze Age peoples, and Celts, down to the Romans. The longest chapter by far is the one on Roman Britain. The book contains seventy-five plates, a number of drawings, lists of sites and museums, and a full bibliography. Unfortunately there are no footnotes.

The absence of notes raises the question of the readership at which Jessup's book is aimed. An absence of any historical background comparable to the topological background will confound readers who are not specialists in prehistoric Britain, as will the use, without providing definitions, of technical terms such as "Hoxian Interglacial Period" or "Hallstatt culture." Nonspecialist readers will regret the scarcity of "intimate sidelights on human life" that archeological artifacts can offer. Somehow the author's imagination and literary skill are inadequate to bring the prehistoric peoples back to life for the reader. Perhaps he tried to do too much in too short a book. If he had omitted the Roman occupation, more familiar to readers, he might have had greater success with the earlier, less familiar periods.

Perhaps Jessup is aiming his work only at specialists in prehistoric archeology. Even these readers may be disappointed, since they will find few accounts of new finds, but mainly summaries of excavations already described in British archeological journals. Yet the book will earn a place on the bookshelf of the serious student of the archeology of prehistoric Britain, because it does draw together in one convenient volume descriptions of sites that otherwise would be scattered in dozens of journals and local publications.

RALPH V. TURNER  
*Florida State University*

I. E. S. EDWARDS *et al.*, editors. *The Cambridge Ancient History*. Volume 1, part 2, *Early History of the Middle East*. 3d ed.; New York:

Cambridge University Press. 1971. Pp. xxiii, 1058. \$23.50.

Volume 1, part 2 of the new *Cambridge Ancient History* takes the story of the Middle East from the dawn of civilization forward to about 1750 B.C. Of its seventeen chapters, three are devoted to Egypt (Early Dynastic, Old Kingdom, Middle Kingdom), five to the lower Tigris-Euphrates valley (Protoliterate to just before the Age of Hammurabi), two to Anatolia, three to the Syria-Palestine area, and one each to Assyria, Persia, and the Aegean world along with a final chapter that deals with Indo-European origins and the impact of the Indo-European invasions upon the Middle East. Contrary to the impression of uneven distribution of bulk that these mere statistics might convey, the net result of this division of chapters is a very satisfactory balance of the material.

Nearly half a century, forty-eight years to be exact, separate this volume from volume 1 of the old edition, and one cannot resist the temptation to make certain comparisons:

Volume 1 of the first edition (1923) ran just over 700 pages and contained 17 chapters, the work of 9 contributing authors. The first volume of the new edition has been issued in parts 1 and 2 of which the latter, the one under review here, itself has 1058 pages, 17 chapters, and a total of 18 authors. In the older edition the general subject matter of the new volume 1, part 2 was covered in 400 pages. As for the bibliographies accompanying the chapters, 36 pages sufficed for volume 1 of the *editio princeps*; in this second part alone of the new volume 1 the bibliographies account for 112 pages.

From this it is not difficult to arrive at the conclusion that our fund of knowledge has increased not merely in bulk but also with regard to certain regions (Elam, Anatolia, Syria-Palestine) of which our view was a bit hazy fifty years ago. Moreover, where in the old edition T. Eric Peet did most of the chapters on Egypt and Stephen Langdon on Mesopotamia, we find that no fewer than six authors provided the new material on these same topics. One obvious conclusion to be drawn from this is that specialization has grown apace in the past fifty years.

On the other hand, one may ask the question "How new is this new edition?" It is

called the third, and it is dated 1971. This means that its chapters, issued as separate fascicles mostly between 1961 and 1965, constituted edition two and that there was some revision before they were put together into a single volume. Careful examination does not show this revision to have been extensive except that new bibliography has been added including a few items as recent as 1968. It therefore seems fair to state that the present volume pretty much sums up scholarly knowledge and opinion as it was in 1965. The world does not stand still, and there have been some changes since that time.

These remarks are intended more as a caveat than as criticism. The new edition has been an enormous undertaking, and its preparation consumed years. Seven of the eighteen authors are now dead—most recently, C. J. Gadd in 1969, and R. DeVaux and Carl Blegen in 1971. As everybody knows, books of this sort are longer in press nowadays than used to be the case. In the edition of 1923, the bibliography included works published as recently as 1922. More important perhaps was the fact that the first edition appeared right after World War I during which scholarly activity and archeological field work had virtually come to a standstill and thus given the authors a chance to survey and judge the condition of a temporarily static field. No such opportunity was afforded the authors of the new chapters; they could look back upon their journey and describe the scenery at the point where they had left the train, but the streamliner on which they had been traveling was already miles away.

The present volume, however, is open to one general and serious criticism, and the blame originates with the excessive specialization imposed upon scholars by the growth and proliferation of the field. The old edition was intelligible to the nonspecialist historian and even the general reader, but most of the new chapters do not have these virtues. On the contrary they were written by specialists for other specialists to read. They fail to provide the overview that the nonspecialist has a right to expect in a volume of this kind, and he is in no position to judge the worth of the section he may be reading. It may be of some consolation to him to learn, however, that a certain proportion of this material, timely though it once was, will

not stand the test of time and will be thrown into the discard.

An extreme, though not the only example, of topicality is afforded by James Mellaart's chapters (18 and 19a) in which an interpretation of material from a few assumed "type sites" in a virtually unexplored area is marshaled to construct hypotheses that could be negated by a few excavations on new sites in the same region. Surely our experience with Mesopotamian pottery from "type sites" as a clue to change and the identity of peoples should warn against easy acceptance of fragmentary evidence and vague similarity as historical criteria for much of anything.

Jaded perhaps by familiarity and disappointment with the fascicules that constituted the second edition, I cannot muster a great deal of enthusiasm for edition three. I have expressed my sympathy for the editors in regard to some of their problems but cannot condone a policy that allowed the authors to write for one another rather than the audience for which the *CAH* was originally intended.

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MORTON SMITH. *Palestinian Parties and Politics That Shaped the Old Testament*. (Lectures on the History of Religions, sponsored by the American Council of Learned Societies. New Series, Number 9.) New York: Columbia University Press. 1971. Pp. viii, 348. \$9.00.

The thesis of this important book is that the Hebrew Bible, the "Old Testament," is the product of the victor in a prolonged conflict between two parties within Palestinian Israelite religion. "The syncretistic form of the cult" honored Yahweh along with local deities; it "was spread widely by Israelite deportation and/or emigration from the eighth century on, and evidently secured considerable adherence from gentiles." The Old Testament "representation of the Israelites as constantly in conflict with the demands of their own religion" is in fact an attack upon this "syncretistic piety" by the other side, the stricter "Yahweh alone" party; this group (at first a minority of the Israelite population) gains the support of the Persian court during the exile in the sixth century.

With the success of Nehemiah as governor in

Jerusalem for the Persians, the Yahweh-alone party evolves into the "separatist party," going beyond opposing official syncretism (which had come to an end) to even more stringent demands, that is, that all who are "polluted" by the worship of other gods be barred from the Jerusalem temple and from marriage with Judeans. The separatists are supported by the Jerusalem masses and the levites; they are also apparently pro-Persian. (A mark of their success is the gradual emergence of the institution of the synagogue, which favors laity over priesthood, community assembly over temple service, and prayer in place of sacrifice; in the gentile world, too, at this time sacrifice is giving place to more "spiritual" forms of worship.) The assimilationists, on the other hand, include nearly all the priests and the gentry of Jerusalem, Judaea, and the surrounding area; they favor Egypt.

The position of this separatist Yahweh-alone group was "enhanced by the success of the literature which it produced or edited (most of the books now in the Old Testament) and by the traditional prestige of Jerusalem. Consequently the adherents of the old, popular form of the cult gradually assimilated their claims and practices to those of Jerusalem, and in effect were converted to the Yahweh-alone position." The Maccabees greatly further this process, which becomes "substantially complete" with the rabbinic Judaism of the late Roman Empire.

Smith is applying a method known already in other areas of religious studies, the identification and analysis of the various competing parties within a larger religious group; the result is a fascinating description of the ebb and flow of social, political, and religious forces particularly in the period (to which five of the seven chapters are devoted) between the fall of Jerusalem in 587 and the rise of the Maccabees in the second century B.C. He is skilled at relating political and religious developments in Palestine to those in other parts of the ancient world; his discussions of "Hellenization" and of the social stratification known from Amos and Hesiod, for example, and his presentation of Nehemiah as a classical tyrant should be read carefully by all who are interested in the Greek world.

Seldom is the Old Testament treated thus:

not as normative "sacred history," not as data for social scientific analysis, but as the product of an ancient religion, to be examined in its broad historical context as any other ancient religion would be. The emphasis on "parties" will surely be questioned: does this religion really fall so neatly into a left and right wing? Did the variations in Persian or Egyptian political influence have such strong effect upon it? If syncretistic Judaism was so common, would it really have been possible to obliterate it almost completely? But Smith presents a well-reasoned, well-documented position (nearly half the book is taken up with bibliography, indexes, and footnotes); the burden of proof is now with those who would debate him.

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A. M. SNODGRASS. *The Dark Age of Greece: An Archaeological Survey of the Eleventh to the Eighth Centuries BC*. Edinburgh: University Press; distrib. by Aldine-Atherton, Chicago. 1971. Pp. xxiv, 456. \$27.50.

The subtitle of this volume is far too modest. Professor Snodgrass has indeed surveyed in masterly fashion an impressive quantity of diverse and widely scattered physical remains from the period 1100–700 B.C. More than 350 sites are listed in the index, and these extend from Epirus and Macedonia in the north to Crete in the south, from Cyprus and the Asia Minor mainland in the east to the western colonies. But this is much more than a survey of the archeological evidence, for he interprets this material as he moves along, especially in the final two chapters devoted to external relations and internal development. Before reaching his own conclusions, moreover, he summarizes other interpretations that have been offered so that the reader has a good summary of the state of modern scholarship before him. The volume is logically laid out; the author considers the material in its chronological sequence and regional context, and this allows us to see both chronological development and regional diversity more clearly than has previously been the case. The text is illuminated by 138 well-placed illustrations of generally excellent quality.

Much of what Snodgrass has to say has been

said before. His discussion of the pottery and the chronology derived from it is based largely on the works of Vincent Desborough and J. Nicholas Coldstream. His overall view of the Dark Age as a period of stagnation and isolation followed by the renewal of overseas contacts and several centuries of slow but steady progress until the pace of change accelerates markedly about 750 B.C., is generally similar to that advanced by Chester G. Starr. Yet the author is no slavish follower of his predecessors; he suggests numerous modifications of his own in matters both large and small. His chapter on the metal objects may be the most comprehensive discussion of this often neglected body of evidence that has yet appeared. But it is primarily in his interpretation of the archeological material that the volume will generate discussion, for many of his conclusions are original and provocative.

While Snodgrass believes that the period justly deserves the designation Dark Age he does not share the opinion that it was inaugurated suddenly by a wave of invaders from Central Europe. He finds no evidence in the physical remains that an intrusive population element was responsible for the widespread destruction that took place throughout Greece toward the end of the Bronze Age. To arrive at this conclusion he regards the presence of intrusive metal objects as insignificant when viewed against the evidence for continuity, particularly in pottery styles; and following Deshayes he rejects Desborough's notion that single burial in cist tombs is really a new phenomenon and argues that it was a carry-over from Middle Helladic times that never completely died out in the Mycenaean period. The destruction was caused, he believes, by migrating Thessalian, Boeotian, and Dorian Greeks, who were largely indistinguishable in their physical culture from the Mycenaean Greeks and who, prior to their migration, lived on the outer fringes of the Mycenaean world. Snodgrass sees Athens clearly in the vanguard throughout the Dark Age; regions outside Athens he classifies as progressive, imitative, and isolated. Athenian leadership, he suggests, may have derived from the political unification of Attica perhaps as early as the late tenth century. He would limit the period during which Greece was isolated from the outside world to hardly more



than three quarters of a century between 1025 and 950 B.C. He notes that it was precisely during this period that iron artifacts begin to turn up in proportionately significant numbers in the more advanced areas of Greece, but after 950 there is a reaction against the use of iron for certain objects. He concludes that the Greeks began to work iron not because it was inherently superior to bronze but because their source of bronze ores had been cut off.

This is only a sampling of what awaits the reader of this exciting volume; it contains far more than can be dealt with here. While over-generalization from isolated finds is not always avoided, and a few slips have inevitably crept in, these are minor when viewed against the whole. Professor Snodgrass has not solved all the problems of the Dark Age, but he has risen far above the tradition of the ancient Greeks and its modern expounders. There will be many who find it difficult to accept some of his conclusions, but even his critics will have to acknowledge that he has shed much light on the Greek Dark Age. As a result, many of our fundamental beliefs about the period will have to be thoroughly re-examined.

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HUGH LLOYD-JONES. *The Justice of Zeus*. (Sather Classical Lectures, Volume 41.) Berkeley and Los Angeles: University of California Press. 1971. Pp. xiv, 230. \$8.50.

This volume presents a provocative if highly personal polemic against the current direction and emphases of work in Greek intellectual history from its beginnings with the Homeric poems through the philosophy of Plato in the fourth century B.C. Basically, as his title indicates, Lloyd-Jones attempts to show that the Greeks conceived of Zeus as a just god who rewarded good and punished evil; that the Greeks as early as the *Iliad* had a concept of justice (*dike*), of right and wrong; and that they conceived of the cosmos as an ordered and moral entity.

There is merit in this point of view; I have sympathy for it and would agree at many points. On Pindar, for instance, Lloyd-Jones

appears to agree with me on the poet's theology ("Pindar's Second Pythian Ode," *American Journal of Philology*, 84 [1963]: 377-89) although he does not cite me. His work, however, largely fails because of its highly contentious style and because of a certain selectiveness of argumentation.

Lloyd-Jones develops each line of reasoning by arguing against a previous interpretation and in particular against the anthropological and psychological approaches of E. R. Dodds and A. W. H. Adkins. This is distracting in itself; but, more seriously, I feel that Lloyd-Jones has missed the general intent of his opponents and distorts the essential nature of their work. In his chapter on Sophocles he builds his argument in reaction to an article by Dodds ("On Misunderstanding the *Oedipus Rex*," *Greece and Rome*, 13 [1966]: 37-49). Dodds's intent was to criticize classical education at Oxford by analyzing the kinds of interpretation given by the undergraduates of the *Oedipus Rex*. His own interpretation is based on that of B. M. W. Knox (*Oedipus at Thebes*, [1957]). Nowhere does Lloyd-Jones cite Knox. It is certainly permissible to disagree with Knox's work but not to ignore it.

In his chapter on Homer, Lloyd-Jones shows limited understanding of the implications of oral technique. Greater understanding would in fact have buttressed his argument. Specifically Adam Parry's article "The Language of Achilles" (*Transactions of the American Philological Association*, 87 [1956]: 1-7) would have supported his argument about the inadequacy of deriving conceptual structure from the study of individual words.

Lloyd-Jones's knowledge, learning, and sympathy for the Greek struggle at the beginnings of Western civilization are impressive. One trusts that in the future he will rid himself of the contentious style and present us with a positive treatment of Greek thought. Classicists and ancient historians should read this work, generalists only if they are willing to read all the works Lloyd-Jones attacks.

I must in conclusion complain about the work of the University of California Press. The type is handsome and readable although the Greek, used sparsely, is badly aligned. But the notes collected at the back and numbered separately for each chapter are not provided

with any headings. Consequently the search for a specific footnote is tedious and bothersome.

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G. E. R. LLOYD. *Early Greek Science: Thales to Aristotle*. (Ancient Culture and Society.) New York: W. W. Norton and Company. 1970. Pp. 156. \$6.00.

JACK LINDSAY. *Origins of Astrology*. New York: Barnes and Noble. 1971. Pp. xii, 480. \$12.75.

In his slim, clear, and cogent volume G. E. R. Lloyd presents the development of Greek science from what we can see of its beginnings through Aristotle and his most immediate successors. His work is nothing less than an excellent analysis of nonethical philosophy in this period. He includes medicine and gives much attention to astronomy but rightly shows that the epistemology and cosmology of the philosophers are central to our understanding of the problems. After the very fact of the formulation of the questions that science must answer their major achievements were the establishment of empirical research and of the importance of mathematics for understanding natural phenomena. There is a useful consideration of the degree to which experimentation and observation were employed, and sometimes misapplied, and of the degree to which they were either irrelevant or beyond the reach of the scientists of the time. For historians the discussion of the social conditions under which Greek science began and Greek doctors and philosophers worked will be of particular interest. Lloyd's comments are perceptive, and he is not tempted to go beyond the very limited evidence we have. One may regret that he has virtually nothing to say on technology—marginal to what he makes his central subject. (Technology is generally neglected in the study of Greek science as shown by the reference to the unsatisfactory work of Forbes in the bibliography as well as a pious gesture toward Lévi-Strauss for the primitives.) But more regrettable is that the limitation of the period prevents us from following Lloyd on to the great era of Hellenistic science. To lament the price of the book is only to say that one would like to see the widest use of it.

Lindsay's *The Origins of Astrology* is in-

tended as a companion to his *Origins of Alchemy* (1970) and is generous in pages and in information, culled through wide and generally intelligent reading. The footnotes and bibliography, however, are so compressed that they are virtually useless; their purpose seems to be to assure the reader that the author has indeed read the works not to help the reader to go to them himself. Lindsay overwhelms the reader with the quantity of data, and undoubtedly his work will be a rich quarry for many a college term paper. He tries to keep the main lines of development before us and does not shirk general interpretation. He tries to make a case for Democritus as a great popularizer of Babylonian astronomy among the Greeks, but against this and for all questions of the relationship of *Early Greek Philosophy and the Orient* (1971), see M. L. West's new book by that title. Lindsay sees astrology, as he saw alchemy, as representing a whole view of the world in which the events of the heavens and earth are interrelated. One may demur at his notion that it flooded into Greece as a result of disillusionment with democracy after 400 B.C., but the concluding chapter in which astrology aligns itself with post-Newtonian physics gives a hint at the passionate enthusiasm that has propelled the author and the weary reader through so much dreariness.

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CHESTER G. STARR. *Athenian Coinage, 480-449 B.C.* New York: Oxford University Press. 1970. Pp. viii, 95, 26 plates. \$10.50.

The years from 480 to 449, which witnessed the development of the Athenian Empire, undeniably represent an "important but ill-lit period" in Greek history. Since contemporary evidence is extremely scanty for these years, the great abundance of Athenian coinage might seem an obvious source of illumination. But as Starr correctly observes, this material is "commonly treated as a wasteland," subject to "an amazing amalgam of views often illogical, even inconsistent," passed on without serious analysis. The consequence has been "the apparent destruction of any historical value for the coinage." Starr's object is to study the coinage for its potential value as contemporary evidence,

and he begins where any solid approach must: with an attempt at classification and chronological arrangement.

An important segment of the book (chs. 2-4) is devoted to a description and systematic classification of the coinage into five groups covering 480-449, and general remarks about the vast but relatively unchanging bulk of material from 449 to the end of the century. The principles of arrangement depend largely on developments in style and particularly in design. This method seems the most feasible, since classical Athenian coins bear no date or magistrate's mark; the hoard evidence is too scanty and uncertain to permit sequential arrangement on this basis alone; and the basic elements of the coinage, Athena's head on the obverse and the owl, olive-twigg, crescent moon, and ethnic on the reverse, manifest numerous minor variations and stylistic developments. Treatment of each group includes a description of common characteristics; tabular catalog with detailed observations, weights, references to publications or collections and to plate illustrations; and a discussion of dating criteria. Starr argues cogently on the basis of the Erechtheion hoard (ca. 480) and general historical considerations (widespread Persian devastation of Attica, the need to resupply the mines with a labor-force, etc.) to a date of approximately 477 for the resumption of minting activity with a new, post-Persian War coinage (distinguished by the crescent moon and the inclusion of olive-leaves on Athena's helmet). Detailed stylistic analysis for subsequent groups follows. Two further chronological lynchpins are the Greek victory of the Eurymedon about 467 (to which is associated the brief and unusual issue of decadrachms) and the events of 449 (Peace of Callias, Congress Decree, and initiation of Pericles' building program with League funds), which necessitated the mass standardized issues that dominate the later fifth century. Starr estimates that these issues represent ninety per cent of coinage, 480-ca. 410.

This new classification leads to several interesting possibilities, including the reassignment of several issues apparently misdated in Seltman's *Athens: Its History and Coinage* (1924); new emphasis on the impoverishment of Athens prior to 449; the suggestion that the famous Currency Decree may have been a proposal of

the Treasurers of Athena intended primarily to simplify their tasks, rather than a mark of Athenian coercion and imperialism; and the probability that Athenian coinage came to be a standard medium of exchange only after Athens had monopolized Aegean silver.

Stylistic arrangement can be impressionistic and arbitrary, but this classification is a clear, internally coherent system based on careful analysis of the numismatic evidence and broad historical perspective. Its weakest link is the terminus of 449, established by reference to historical events that are hotly debated by historians. While the argument cannot be conclusive (as Starr recognizes) it is very persuasively presented. The book is a lucid and valuable contribution demonstrating how historians and numismatists can combine their special areas of study to broaden knowledge of classical Greece.

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THOMAS FISCHER. *Untersuchungen zum Partherkrieg Antiochos' VII. im Rahmen der Seleukidengeschichte*. Tübingen: the Author. 1970. Pp. ix, 124. DM 12.

This is a typical German doctoral thesis in the classical history field. The author, a student of Hermann Bengtson of Munich, has collected all of the sources on the eastern aspirations of the last Seleucid ruler to attempt a reconquest of Iran, which had fallen away from the Hellenistic dynasty. His conclusions are that Antiochus VII led two expeditions, not one, against the Parthians in the period from 131 to the spring of 129 B.C. when he was killed in Parthylene instead of in Media, as most scholars have supposed. Further the Seleucid army remained intact and was again led against the Parthians by a son called Seleucus until Demetrius II usurped power from his nephew. The real catastrophe to the Seleucid army came under this Seleucus and not under Antiochus VII.

Since the classical sources are so fragmentary the historian of this period is free to speculate on the discrepancies in them. Furthermore, there are no cuneiform tablets from Mesopotamia to check the chronology of the events of 131-130 B.C., and even the dates are open to dispute. Since the sources indicate that Anti-

ochus VII with a small group of followers was surprised by the Parthians and killed, Fischer assumes that the Seleucid army must have remained intact and was then led by Seleucus until he was defeated and captured. This is possible but we simply do not know. Likewise, because one source says Antiochus VII invaded Parthylene instead of Parthia, the author assumes that the Seleucid king invaded the homeland of the Parthians. But all the sources are much later than the events, and one should not count on a fine distinction in the sources rather than a not unnatural *lapsus*.

I can bring only one piece of evidence to counter Fischer. If Antiochus spent two winters, 130 and 129 B.C., on the Iranian plateau, the absence of his coins from the mint of Ecbatana (Hamadan) is difficult to explain. Then one would speculate otherwise than the author, but the end result was the loss of Iran forever to the Seleucids.

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DAVID STOCKTON. *Cicero: A Political Biography*. New York: Oxford University Press. 1971. Pp. ix, 359. \$11.25.

As consul in 63 B.C. Marcus Tullius Cicero managed to rescue Rome from the conspiracy of Catiline, an episode comparable to Everett Dirksen outfoxing Aaron Burr. An orator of great skill, vast ambition, and towering conceit (*O fortunatam natam me consule Romam*) Cicero was a political opportunist of middling ability and an eloquent spokesman for more powerful men. He also wrote a number of treatises, crammed with borrowed learning and banalities that impress clergymen as profound, but at least he was no Bryan—though an augur, Cicero debunked divination. It is regrettable that the self-made image of Cicero has always been admired by the type of mind that confuses American congressmen with statesmen. However, Cicero's voluminous correspondence reveals him and his society with such devastating frankness that Carcopino could argue that Augustus published it in order to discredit the old order. So much is known of the real Cicero that Professor Stockton can only make him look commendable by comparing him with a really repulsive character, Brutus, who rammed de-

crees through the Senate for venal personal gain and yet murdered Caesar for being "above the law." Stockton's book is a political biography aimed at undergraduates and general readers and as such is not bad, but specialists will balk at some debatable points in the first half. Despite disclaimers, the author implies that there was "a party of reform" in Republican Rome and that it was the Populares.

The second half of the book is much better and more sensible in its treatment of men and problems. What is more, Stockton writes very well—for example, Cicero "was the archetypically conservative with a small 'c.' Politics fascinated and absorbed him: the day-to-day play of affairs, the gossip, the intrigue, the formal pageant, the clash and color of personalities. Policies bored him." Good prose is no small virtue at a time when much historical writing is pedestrian and jargon-ridden, probably because of the profession's penchant for mediocrity in thought and expression. Stockton's epitaph on Cicero is superb: "His immense success as consul seduced him into entertaining ideas above his real station, leaving him with the appetite for a controlling position without the basic means to satisfy it, either in the shape of widely based political support and connections or—to be honest—the intellectual and moral equipment for such a role. For the next twenty years, 'the glory of the Nones' was to hang around his neck like a millstone dragging him further and further down." To his credit, Stockton has not told a tragic tale but an honest and instructive one with heuristic value for fields other than Roman history.

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## MEDIEVAL

R. W. SOUTHERN. *Medieval Humanism*. (Torchbook Library Edition.) New York: Harper and Row. 1970. Pp. x, 261. \$9.00.

The twelve essays collected in this volume, some of them now printed or printed in full for the first time, are grouped under four headings: biographical sketches of Bede, St. Anselm, and Meister Eckhart under the heading "Three Stages of European Experience"; essays on me-

dieval humanism, the school of Chartres, Abelard and Heloise, and Peter of Blois under "Aspects of Humanism" (the heart of the book); discussions of England's first entry into Europe in 1066 and on England's place in the twelfth-century renaissance under "Europe and the 'Other World'"; and biographical pieces on Ranulf Flambard, Henry I, and Pope Adrian IV under "Three Types of Practical Wisdom." They focus on themes that have concerned Professor Southern for some years, one of them, indeed, being "an extensive revision" of the much admired paper on Ranulf Flambard originally published in 1933 and first written, the author tells us, at the suggestion of Professor V. H. Galbraith to whom the volume is dedicated. As their titles suggest, these essays concern English history as well as Continental, the history of practical affairs as well as that of spiritual and intellectual life, broad-gauge problems of historical interpretation as well as detailed questions of manuscript provenance and textual criticism. Though I must confess to having experienced a little difficulty in discerning the "unity of theme" to which the author aspires, the "unity of treatment," on the other hand, is readily apparent. For if the subject matter of these essays reflects very well the impressive range of Professor Southern's interests, the way in which he handles it also exhibits throughout that unusual combination of bold originality with steadiness of historical judgment and reflective sensitivity to the feeling of an era that we have come to expect of his work. Nowhere is this more evident than in the numerous sections of the book concerned with biographical matters: in less than twenty pages, for example, he does more to render accessible to the modern reader both the letters of Abelard and Heloise and the personalities concealed behind them than has many a lengthier analysis.

Admiration, then, is very much in order. This is a distinguished volume. But admiration does not (and, I suspect, will not) necessarily imply agreement. There is much to provoke in these essays. Many, no doubt, will be so provoked, and most, I would guess, by the two central contributions on medieval humanism in general and on the School of Chartres in particular. For me, the latter of these, with its fascinating debunking of the special distinction

traditionally accorded to the twelfth-century school at Chartres, carries conviction. But the former, with its redefinition of humanism, its depiction of Renaissance humanism as a "product of disillusion with the great projects of the recent past" (p. 60), and its insistence that "the period from about 1100 to about 1320" was "one of the great ages of humanism in the history of Europe: perhaps the greatest of all" (p. 31), however golden its vision, raises as many questions as it answers. The academic or literary view of humanism as a specific program of studies may well have its roots in the Renaissance era rather than in the conditions of the twelfth century itself, but it is no more remote from those conditions than any other view—including, perhaps, that of Professor Southern himself. It retains, moreover, the added (if fustian) advantage of providing us with an appropriate label wherewith to distinguish the approach to learning characteristic of the twelfth-century schools from that more technical and specialized approach which came to dominate in the universities of the thirteenth century. But these are big issues, and if it is the merit of this book to have raised them with such force, it is also its charm to have succeeded in doing so with such urbanity.

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JOHN BEELER. *Warfare in Feudal Europe, 730-1200*. Ithaca: Cornell University Press. 1971. Pp. xvi, 272. \$7.50.

Professor Beeler's book has the merit of brevity and readability. The scope of his essay, for that is what it essentially is, is enormous both in time and space. The chapters deal with Carolingian Europe, the early Capetians, Norman Italy, the Norman Conquest, crusader Syria, southern France and Christian Spain, central and northern Italy, and Germany. Moreover, he writes almost as much on the politics of war as on warfare itself. He takes an original approach, stressing the role of nonfeudal elements, of sieges as opposed to battles, of naval support, and of the terms of obligation and contract. In this he corrects traditional misunderstandings in the direction pointed by recent monographic and periodical literature. It is nevertheless difficult to assess the value of Dr.

Beeler's findings on any particular issue, as his book is practically devoid of critical apparatus. This also means that the book is of limited value to scholars, as any serious student would have to search unaided for the monographs, articles, and sources that underpin the author's findings. I will take a single example, the Normans in Sicily, as a test case. Beeler adds nothing to the material in Chalandon's monograph, which in turn (as far as the conquest of the island of Sicily is concerned) is based almost entirely on a single source, the chronicle of Geoffrey Malaterra. What Beeler gives is a brief, selective summary with stress on those elements that general accounts have usually underestimated or ignored. For the most part exact dates and details are eliminated, though, per contra, when seeking to establish a point the author will occasionally make an excursion into precise reconstruction. References to original sources, which are extremely scarce, tend to be in the form of such phrases as "it is related that . . .," and so on.

Some aspects of the subject receive little or no attention. Examples are the Peace and Truce of God, the use of parish levies, and the nature of the siege weapons employed. On the other hand, the author is trustworthy and properly cautious in dealing with numbers and composition of forces.

The only way to write about warfare in a period almost entirely lacking in record sources is by intelligent estimates of the reliability of a few chronicles written by monks remote from the business of war. Hence a major work on the subject would bristle with critical analysis and references. As it is, we have to accept Dr. Beeler's book largely on trust, an unhappy situation for the reader. It is my opinion that the essay here presented could have remained a reliable short survey, as clearly intended, without such a wholesale sacrifice of the scholarly references and criticism of sources usual in a revisionist monograph.

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*Il monachesimo e la riforma ecclesiastica (1049-1122): Atti della quarta Settimana internazionale di studio, Mendola, 23-29 agosto 1968.* (Pubblicazioni dell'Università Cattolica del Sacro Cuore. Contributi, Third Series. Varia,

Number 7.) Milan: Editrice Vita e Pensiero. 1971. Pp. xvi, 540. L. 10,000.

This volume contains the proceedings of the fourth study week in the series organized at La Mendola by the Catholic University of the Sacred Heart in Milan. This triennial conference, like the similar gatherings at Todi and Spoleto, attracts a large and distinguished body of medievalists, and the impulse and enrichment given to the studies concerned, together with the widening of horizons and interests resulting from the contacts among scholars of many nations, cannot easily be overestimated. To those who were not fortunate enough to be present this publication will be a revelation of the wealth of scholarship that is currently being expended on monastic history. In this field the French and Belgian contribution has long been recognized, but the Italian element has increased very considerably since World War II. Professors Capitani, Fonseca, Tabacco, and Violante, together with Dom Gregorio Penco, are only five among a regiment of students of the first class engaged in monastic history. Germany and Austria have never lacked representatives, but it is good to see scholars from Central Europe, Poland, and Czechoslovakia. Even so, the roll call of La Mendola has several lacunae. Spain is not represented in strength, there are many other Cistercian and Benedictine medievalists besides Dom Jean Leclercq and Dom A. Dimier, only two English historians were present, and America had only a single, albeit a notable, representative.

The topic that binds together the individual items of the present collection is unquestionably both important and fertile. Indeed, it might almost seem to have proved itself at once too manifold and too superficially familiar to give birth to a well-proportioned, comprehensive, and deeply probing survey comparable to that of the previous conference on hermit life in the West. After all, monks and monasticism were at the very heart of the Gregorian reform; they were at once its major agents and the primary objects of attack in its prehistory. Every medievalist on a degree course could be expected to write an essay on the subject. This may explain the absence from this volume of any study of the great monastic personalities, such as Damiani, Humbert, or the Cluniac popes, and any discussion of the mutual inter-

action of monasticism and reform, but room might well have been found for an exposition of the programmatical monachization of the entire Church, the people of God, their worship, and their piety, which was undoubtedly a feature of the epoch and one that remained to provide a target for the Fathers of Vatican II.

Having said so much, it is time to turn to the individual contributions, which are too numerous to receive more than brief mention. The admirable opening discourse by Professor Fonseca comes nearer than any other piece to giving a survey of the whole field, and his footnotes give a fair picture of the work that is being done. Dom Grégoire of Clervaux outlines and discusses the share of Monte Cassino in the early stages of the papal reform. There is room here for a major work of scholarship that would exhibit this great abbey in its golden age, its art, its learning, and its relations with Rome and Byzantium. One short episode in its life is studied by Professor Wollasch of Fribourg, that of the papal election of Nicholas II. Professor Marselli (Rome) then provides a lengthy study of the Carthusians and a shorter one of the Cistercians. Both he and a later lecturer stress the significance of Guigo I in the history of the Charterhouse, a significance comparable to that of Stephen Harding in the history of the white monks. Professor Tabacco (Turin) then gives a clear and well-documented review of the establishment of monastic foundations by the bishops; this is followed by a review by Professor Christopher Brooke (London) of the part played by kings, especially English kings, as patrons. Professor Kloczowski gives a very welcome account, with a bibliography of Polish works, of the plantation and development of Benedictine monasteries in Poland, processes begun by St. Adalbert (d. 997), fostered by kings and bishops, largely destroyed later, and reconstituted by Duke Casimir and Boleslav the Bold, largely with the aid of monks from Bavaria.

We then pass to the realm of the mind and spirit. Dom Grégoire (Clervaux) and Professor Mollat (the Sorbonne) discuss monastic poverty and see the monks, as corporate possessioners, still aiming at the virtue of personal poverty, the way of life known to later Franciscans as *usus pauper*, still regarding alms and hospitality, rather than assimilation, as their duty to-

ward material poverty, though there were many critics preparing the way for St. Francis. Dom Anselm Dimier's (Scourmont) chronological list of monastic buildings, with short comments and dates, will remain as a mine of reference, though it must have been somewhat overwhelming as a lecture. Professor Meersseman (Fribourg) provided a whiff of controversy in discussing the "monastic theology" of Dom Jean Leclercq. Meersseman had a point. There were several monks among the dialecticians (for example, Anselm and Abelard); there were the canonists, too often neglected by historians in the past; there was the intuitional or spiritual approach to theology common both to monks, such as Eadmer, and theologians, such as the Victorines. Yet in spite of this, "monastic theology" remains a significant phrase. Dom Leclercq, in the lecture that follows, met the broadside with a wave of the hand and passed on to a full and richly documented account of monastic historians and their aims. There can be no other monastic historian comparable to Dom Jean in the virtuosity with which he can produce a fresh and valuable contribution to every conference he attends. La Mendola caught him "on the hop," so to say, between Indochina and Japan, but the quality of his lecture showed no sign of fatigue. He was followed by Professor Giles Constable (Harvard), who discussed the attitude of reformers, monks, and others to the possession by monks of churches, tithes, and other *spiritualia*. Here attempts by theorists and rigorists to drive the monks off the rich field of spiritual income were at first partially successful, but the twelfth century saw a gradual return, even among the Cistercians, to the *status quo ante*. Next comes a short but remarkably taut and lucid review by Professor Duby (Aix-en-Provence) of the fluctuations between revenues in kind and in rent and the attitudes toward agrarian labor on the part of various religious orders. The Continental evidence he alleges bears out the suggestions of English economic historians that before and after the Conquest a fair proportion of the economy of monasteries rested on money. Professor Kottze (Ratisbon) looks at areas of monastic culture and suggests that while Hirsau was predominantly Augustinian in its reading, Gorze was more open to classical literature, at least in its reading habits, and

that Cluny was less interested in both Fathers and classics. If the contrast of cult versus culture is too sharp a view of Cluny and Gorze, it has at least a vestige of justification.

Finally, there are four communications. Dom Penco (Finalpia) describes the function of Fruttaria (in the foothills of the Alps north of Turin) as a kind of buffer between Italian and French, reformed and unreformed, zones. W. Kurze (German Institute of Rome), with notes on the expansion and constitution of the order of Camaldoli, pleads for a critical history of the order, and J. Plocha discusses the origin (probably Bavarian) of the Polish abbey of Mogilno. Professor Capitani (Lecce) writes at some length on the imperial connections in abbeys of central and southern Italy. On an earlier page Professor Zerbi (Milan) had given a summary of the topics discussed and distributed the appropriate bouquets of commendation.

The value and interest of the lectures are greatly enhanced by the records of the discussions that followed them and that often modified or emphasized particular points made by the lecturer. The interventions of such well-known historians as Professor Raymonde Foreville (Caen) and Professor J. F. Lemarignier were particularly helpful. One wonders whether shorthand or tape recording was the medium employed in preserving these discussions, which were presumably subedited to a certain extent. In either case the reportage is good, and the rare occasions where the English, for example, makes little sense were probably due to phonetic difficulties. The printing in general, though polyglot, is practically faultless.

As the years pass and the number of such conferences multiplies, suitable topics with the requisite depth and breadth will become harder to find until after twenty years or so another generation can take a fresh look at old issues. But the pool of research and scholarship on which the organizers can now draw is so great that we need not fear for the immediate future. (The success of such meetings, so experience seems to show, is the selection of a subject of considerable depth but limited range and the provision of an assembly of limited size and scholarly competence in that subject. Crowded conferences and scattered interests make for social, rather than academic, profit.

La Mendola deserves the support of all medievalists interested in the epoch of spiritual reform and intellectual adolescence in medieval Europe.)

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HENRY MARSH. *Dark Age Britain: Some Sources of History*. [Hamden, Conn.:] Archon Books. 1970. Pp. 221. \$7.50.

Nine literary sources of varied character and overlapping coverage supply the bulk of our historical knowledge of Britain during the half thousand years following the Roman withdrawal. At the beginning of the period the paramount event is the *adventus saxonum*. Gildas, Nennius, the *Annales Cambriae*, and *Brut y Tywysogion* deal with the struggle between the Romano-British provincials and the Germanic invaders from the point of view of the former while Bede and the *Anglo-Saxon Chronicle* reflect English traditions. In the middle of the period interest attaches chiefly to Bede's account of the establishment and progress of Christianity among the Anglo-Saxons. During the eighth and ninth centuries the central theme is political evolution culminating in the emergence of the English kingdom under Alfred. This history is traced primarily in the *Chronicle* with Asser's biography of Alfred providing additional matter at the end of the period. Two post-Conquest writers, William of Malmesbury and Geoffrey of Monmouth, also contribute some information about these earlier times, perhaps drawn from lost materials, although the latter's work is more fiction than history.

Mr. Marsh discusses each of these works in sufficient detail to permit appreciation of their individuality and comparative reliability while also revealing a pattern of relationships within the group. In this regard he is open to some criticism. He appears unaware of the dependence of much of the early *Chronicle* on Bede, and he invokes lost British sources for evidence manifestly obtained elsewhere. Even so, and despite a regrettable puerility of style (the book is crowded with "proud armies," "ruthless adventurers," "cruel ships," *ad nauseam*), the author might have produced a useful popular study had he been better informed about his subject.



As it is the book offers little to the knowledgeable student, and the inexperienced student will be ill-served by exposure to its errors.

For example, Mr. Marsh is mistaken in claiming that Britons are mentioned in early English laws (p. 44); Arthur's death is not given as 570 in the *Annales Cambriae* (p. 82; cf. p. 51); the E rescension of the *Chronicle* was maintained only until 1154, not "100 years or more after the Norman conquest" (p. 93); St. Neot, whatever his date, certainly did not live "100 years and more after Asser's death" (p. 149); the Battle of Hastings was fought in October, not September (p. 155); and Harold Godwinson was not present at the Battle of Fulford (p. 156). Moreover, the genealogy and chronology of Alfred's family is incorrect (p. 173); the discussion of Manuscript A of the *Annales Cambriae* is nonsensical (pp. 45-46); and the St. Alban entry in the *Chronicle* is not "derived from a British source" (p. 95) but from Bede (bk. 1, ch. 7), who took it from a *Passio Albani* of which Mr. Marsh is apparently ignorant (p. 112).

In addition to blunders misprints abound: for instance, interdiction for indiction (p. 93), Britain for Briton (p. 98), 751 for 731 (p. 124), Coxson for Cotton and Beohtric for Eohric (p. 145), Arwulf for Arnulf (p. 150), *Enchirdion* for *Enchiridion* (p. 151), and Aethwulf for Aethelwulf (index). The "Note on Texts and Translations," in lieu of bibliography, overlooks several recent, now standard editions and translations, notably, Williams's *Gildas*, Wade-Evans's *Nennius*, Plummer's (and now Colgrave and Mynors's) *Bede*, Whitelock's *Chronicle*, Jane's *Asser*, and Campbell's *Aethelweard*.

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PETER CLEMOES and KATHLEEN HUGHES, editors. *England before the Conquest: Studies in Primary Sources Presented to Dorothy Whitelock*. New York: Cambridge University Press. 1971. Pp. xvi, 418. \$23.50.

To honor Dorothy Whitelock on her seventieth birthday some of her ablest co-workers in the field of Anglo-Saxon studies have contributed twenty-two scholarly articles, which are published here along with a bibliography of

Miss Whitelock's own wide-ranging works. The twenty-two articles span the variety of disciplines that are presently being directed toward the investigation of the Anglo-Saxons: history, literature, art history, place-name studies, linguistics, numismatics, paleography, and archaeology. The term "primary sources" in the subtitle embraces not only historical documents but also literary works, coins, buildings and building sites, place-names, and artifacts. Altogether these studies bear impressive testimony to the intelligence, sophistication, and scope of investigative techniques in modern Anglo-Saxon scholarship. They disclose, too, the seminal importance of Dorothy Whitelock's own contributions to Anglo-Saxon literature and history.

Compared to other collections of this sort, the present volume maintains a remarkably uniform standard of excellence. Assembled here are examples of the top drawer work of the finest scholars in the field. Peter Hunter Blair, applying shrewd critical analysis to Bede's *Ecclesiastical History*, suggests an important chronological revision in the relationships of Paulinus, Queen Aethelberg, and King Edwin that underlay the conversion of Northumbria, and in the process one learns more of Bede's own method of writing history. Bede's use of sources is further illuminated in Paul Meyvaert's subtle textual criticism of the *Libellus Responsionum* in which it is shown that Bede's text of the *Libellus* was not a contemporary forgery, as has been suggested, but a corrupt version of a much older original. J. M. Wallace-Hadrill deals expertly with relationships between St. Boniface and the Frankish Church and with antecedent Frankish ecclesiastical reform, giving us in the process a singularly persuasive reinterpretation of Charles Martel's confiscations of Church property. Kathleen Hughes summarizes and amplifies past scholarship on Anglo-Celtic ecclesiastical relationships after Whitby, tracing the work of Irish churchmen in England and English churchmen in Ireland and demonstrating their fruitful influences on one another up to the Synod of Chelsea in 816. Nicholas Brooks employs his intimate knowledge of the documentary sources to revise current opinion on the origin and diffusion of the threefold military obligation. Neil Ker, using rigorous techniques of handwriting

analysis, finds the hand of Wulfstan the homilist, bishop of Worcester and archbishop of York, in a number of Worcester manuscripts including Hemming's cartulary. Similarly, John Pope, taking issue with Dorothy Whitelock, argues from a stylistic analysis of Aelfric's rhythmical prose that the Old English version of the Ely Privilege was composed by Aelfric himself. Stylistic analysis is deftly used in Cecily Clark's study of the pre-Conquest *Anglo-Saxon Chronicle*, wherein the distinction between history and annal receives valuable clarification.

These examples demonstrate both the scholarly ingenuity and the interdisciplinary range of the contributors, and the remaining pieces follow the pattern. Henry Loyn's learned and graceful state-of-the-field chapter points to the vitality and variety of Anglo-Saxon towns and suggests promising new avenues of investigation. Political theory is represented by Dorothy Bethurum Loomis's investigation of the homilist Wulfstan's ideas of polity and *ecclesia*, which anticipate Gregorianism in their emphasis on churchmen's role in the world and the Anglo-Norman Anonymous in their rejection of papal monarchy, but differ from both—and from Carolingian political thought—in their conclusion that bishops are the proper leaders of the Christian commonwealth. The multidisciplinary approach finds further expression in Kenneth Cameron's application of place-name evidence—specifically, the Grimston hybrids—to the problem of Scandinavian settlement in the territory of the Five Boroughs, in R. I. Page's use of inscriptions to cast light on the survival of the Scandinavian language in England, and in the chapter by Olof von Feilitzen and Christopher Blunt, which catalogs the names of moneyers on the coins of King Edgar, enabling the authors to identify a considerable number of Scandinavian and Continental personal names. Michael Dolley's chapter on the nummular brooch recently discovered at Sulgrave instructs us on the author's expert methodology no less than on the subject of his inquiry. The same is true of H. M. Taylor's innovative use of "structural criticism" to date and explain the components of the Anglo-Saxon church of St. Wystan's at Repton. The technique of identifying earlier sources in Anglo-Saxon writings is demonstrated in Janet

Bately's study of classical additions to the Orosius translation and in Alistair Campbell's article on earlier heroic verse in *Beowulf*, which confirms his interpretation of the poem as a carefully constructed literary work rather than the product of an illiterate singer. The late Francis Wormald contributed a masterful treatment of the stylistic antecedents to the Winchester school of illumination back to the reign of Alfred. Several disciplines meet in Peter Clemoes's illustrated discussion of how contemporary visual representations and liturgical passages influenced Cynewulf's poem on the Ascension. René Derolez provides a useful study of Anglo-Saxon cartography and geographical knowledge in the course of analyzing the account of the Norwegian explorer Othere in the Old English translation of Orosius. And J. E. Cross uses a variety of literary and historical sources to investigate Anglo-Saxon attitudes on the "just war." The *Festschrift* concludes with an admirable article by Martin Biddle summarizing present knowledge in several fields of Anglo-Saxon archeology and urging the necessity of future interdisciplinary training such as to produce individual scholars with expertise in both history and archeology.

*England Before the Conquest* is not without fault. It is extraordinarily expensive, and it suffers from the organizational choppiness common to all *Festschriften*. Anglo-Saxon England awaits its new Stenton. But as a comprehensive and authoritative presentation of the current state of the art in Anglo-Saxon studies, this book will be essential to students and scholars of the period in all the relevant disciplines.

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T. B. PUGH, editor. *Glamorgan County History*. Volume 3, *The Middle Ages: The Marcher Lordships of Glamorgan and Morgannwg and Gower and Kilvey from the Norman Conquest to the Act of Union of England and Wales*. Cardiff: University of Wales Press for the Glamorgan County History Committee. 1971. Pp. xix, 704, 6 maps, 33 plates. £12.00.

This is a fascinating volume, dealing with an area and a period that offer material of deep significance: the intermingling of Norman and Anglo-Saxon with the Welsh, the contrast of

upland and lowland, the workings of Marcher lordship, the growth and decline of one of the great earldoms that played a notable part in English and Welsh politics, the impact on Marcher outlook of the Edwardian conquest of North Wales, and the varied fortunes of religion and the Church. This volume also relates the vicissitudes of economic life, the rise of the gentry, and last, but by no means least, the remarkable literary tradition of Morgannwg. With regard to this tradition, Ceri M. Lewis in a fine essay has said that, among the major historic regions of Wales, Morgannwg alone can legitimately claim to have nurtured not merely one, but two great literary traditions.

A distinguished group of writers, under the able editorship of T. B. Pugh, has produced a volume that will bear comparison with the best of the Victoria County Histories. Most of the contributors are in Welsh universities, and the volume is incidentally a tribute to modern Welsh historical writing, being specially marked by researches in depth into original sources. But the editor himself is at the University of Southampton. Other writers are in London and Leeds. Michael Altschul, who writes with much authority on the lordship of Glamorgan and Morgannwg in the thirteenth and early fourteenth centuries, is at Western Reserve University in Cleveland, Ohio.

The themes touched on above show the breadth of the treatment and its value to all students of the period. The scope might conceivably have been extended a little to deal specifically with the influence on the outlook and English policy of the lords of Glamorgan, as these were affected by Welsh power in the North under Llywelyn ap Gruffydd and by the growing power of the English monarchy, especially in the reign of Edward I. Whether, and to what extent, such developments influenced the attitude of the lords of Glamorgan toward English politics it may be impossible to say. Mr. Pugh, who writes about the fourteenth and fifteenth centuries, makes no real effort to do so. He is content, for the most part, to record the ambitions and aggrandisements of the lords, pursued as relentlessly at the royal court as in Wales. He will not even concede any qualities of statesmanship to Hugh Despenser the Younger, under Edward II, challenging such veteran authorities as Stubbs and Tout.

His summary of Warwick the Kingmaker's attitude in the fifteenth century is similarly quite uncompromising: "It was defence of his own territorial interests in the Welsh Marches that compelled Warwick to become a Yorkist, and fight against the king at St. Albans on May 22" (p. 196).

This is nevertheless a memorable volume. It sums up the quite remarkable researches of the last generation and will be an indispensable vade mecum to all future scholars in the field.

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F. R. H. DU BOULAY and CAROLINE M. BARRON, editors. *The Reign of Richard II: Essays in Honour of May McKisack*. [London:] University of London, Athlone Press; distrib. by Oxford University Press, New York. 1971. Pp. xvi, 335. \$16.00.

ANTHONY GOODMAN. *The Loyal Conspiracy: The Lords Appellant under Richard II*. Coral Gables, Fla.: University of Miami Press. 1971. Pp. xi, 212. \$7.95.

The reign of Richard II has probably received more attention from English medievalists than any other save those of the Conqueror and John. The explanation lies not only in its great events but also in its contribution to the training of English undergraduates in history. First at Manchester and then at Oxford it provided a "special subject"—the study of a short period from its sources, which forms the culmination of a history student's career at an English university. This *Festschrift*, commemorating the achievements of Professor McKisack, who founded such a "special subject" at London, is a testimony to the stimulus that undergraduate teaching on the reign has given to research.

A number of useful studies examine technical aspects of Richard II's diplomacy, Henry of Derby's expeditions to Prussia, the local governments of East Anglia, London chantries, and the place of the reign in Tudor and Stuart historiography. Two describe the king's relations with the leading cities of his kingdom, though in the light of Caroline Barron's examination of those with London it is difficult to accept John H. Harvey's suggestion that Richard seriously intended moving his capital to York. Three contributors have produced excep-

tionally valuable studies. J. J. N. Palmer discusses Richard's foreign policy after 1389, showing that it made a vital contribution to the collapse of French ambitions in Italy. The Anglo-French expedition, which was planned to go there in the spring of 1397, was intended to force on the rival popes the compromise solution to which English policy had helped convert the French. R. R. Davies illuminates the last two years of the reign, describing how Richard made Cheshire the base for a remarkable concentration of power, which, benefiting from the Arundel forfeitures, extended into Wales. For the first time we obtain precise information on the scale and expense of the famous Cheshire retinue (pp. 268-69). The value of the contribution by R. L. Storey transcends the limits of the reign. He demonstrates that the order restricting the right to grant liveries to secular peers does not deserve the attention it has received from the historians of "bastard feudalism." It was an ordinance issued by king and council after, and not a statute of, the Parliament of January 1390; nor did it deal at all with retaining. At the same time his discussion contributes to parliamentary history by exhibiting friction between Lords and Commons over the maintenance of law and order.

Other studies do not merit such praise. Barbara Harvey's discussion of the monks of Westminster and Oxford may throw useful light on the relations between monastic communities and the universities, but most of it is irrelevant to Richard II's reign. In view of V. H. Galbraith's long career of remarkable scholarship, the mixture of the amateur and the professional in his "Thoughts about the Peasants' Revolt" comes as a great disappointment. His remarks on villeinage are not rooted in a thorough knowledge of the literature. It would have been so much better if he had worked out in detail his suggestion that Walsingham was indebted to the same source as the Anonimale chronicle for his account of the revolt. Does he really consider (as p. 51 implies) that they shared the same source in their accounts of the Good Parliament? The matter is too important to be dealt with in asides. The weakest study of all is J. A. Tuck's. Padded out with references to royal patronage from the sixteenth through the eighteenth centuries, it provides a thin sketch of administrative practice that hardly

accords with the promise of the title "Richard II's System of Patronage." We learn nothing of the way patronage fitted into royal policy as a whole.

The industry with which Anthony Goodman has culled the sources of the reign for his book on the Appellants is not matched by either his analytical skill or the interest of his conclusions. Roughly one-quarter of the text is a turgid survey of the events of 1387 to 1389. There follow seven chapters containing biographical studies of the Appellants and, in the case of Thomas of Woodstock and the earls of Arundel and Warwick, discussions of their landed wealth and affinities. For most of the time the connection between these and the book's main theme is not clear. Mr. Goodman may be right that Thomas of Woodstock and Mowbray needed royal patronage to maintain their position. But two pages of conclusion, containing a somewhat rhetorical question about a *crise nobiliaire* followed by guesses, are hardly a substitute for sustained argument. The book's title begs questions that are hardly raised and certainly not answered. How, for example, can it be reconciled with evidence that Richard was deposed for a few days? The state of the sources may make a more satisfying treatment difficult. But, even so, this effort is not worth a book.

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VEIKKO LITZEN. *A War of Roses and Lilies: The Theme of Succession in Sir John Fortescue's Works*. (Suomalaisen Tiedeakatemia Toimituksia, Series B, Number 173.) Helsinki: Suomalainen Tiedeakatemia. 1971. Pp. 73. 11 M.

This is a careful and detailed little study of one of the main themes in the published work of Sir John Fortescue (?1385-1473), chief justice of England, chancellor, polemicist, councilor, and political theorist. Mr. Litzen argues that the most fruitful approach to Fortescue's work is through his theories of succession. For of all his known works only one, now lost, is "irrelevant to the problems of English and French kingship." The critical problem of fifteenth-century kingship in England was that of a disputed succession, and Fortescue's explanations of what constituted the right to govern are the

heart and soul of his writing. Moreover, it is by taking this angle of approach that we can best see Fortescue as a political theorist of and in his own time.

The impetus to Fortescue's serious political writing came with Richard of York's claim to the throne in October 1460, as "lawful heir of Edward III and Henry III." To the beginning of the Lancastrian exile the following year Fortescue tried to refute York's claim, principally by denying the female right to transmit a claim to the throne. In exile he became a polemicist, writing to gain the support of the king of France for his master. Upon his re-establishment—at eighty—as councilor to Edward IV he "refuted many of his old arguments," but never abandoned those essential to the "bearing of *De Natura*," his principal work on the succession.

Mr. Litzen is concerned with exonerating Fortescue from charges of duplicity (in serving the house whose right to kingship he had for so long denied). Mr. Litzen's judgment of Fortescue recalls that of Sir John Fortescue's own great descendant, the historian of the British Army: "He was loyal in a time of treachery, upright in a time of violence, constructive in a time of destruction, and hopeful in a time of despair."

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MICHEL ROBILIN. *Le terroir de Paris aux époques gallo-romaine et franque: Peuplement et développement dans la Civitas des Parisii (Seine, Seine-et-Oise)*. Preface by ALBERT GRENIER. 2d rev. ed.; Paris: Éditions A. et J. Picard. 1971. Pp. xiv, 6-491. 110 fr.

Some twenty years ago Michel Roblin's *Le terroir de Paris* marked the first basic revision of the d'Arbois de Jubainville and Camille Jullian explanation for the pattern of human settlement during the Gallo-Roman and Merovingian periods in the area around Paris as well as in most of France. This second edition, essentially an amplification of the first, incorporates the research of Roblin and fellow scholars during the intervening twenty years. Like the first it is a model of scholarship combining the knowledge and methodology of the historian, geographer, geologist, archeologist, orographist, numismatist, botanist, and zoolo-

gist. Roblin's objective is to trace the origins of rural settlement, the clearing of land, the growth of villages and of hamlets, and the pattern of land and water routes. His hope has been "concrétiser l'oeuvre humaine au cours des siècles."

During the twenties when Jullian did his celebrated work on Gallo-Roman colonization he agreed with Jubainville that the names of early Frankish rural settlements or villages were derived from the names of Gallo-Roman domains whose names came from their Roman masters. Finding that numerous place names in the Merovingian and Carolingian periods ended in *acus*, he argued, for example, that the domain of *Sabinus* came to be called *Sabiniacus* from which were derived the French Savignac of the Midi and the Savigny of the north. There was a similar derivation for other such names in the Paris region as Orly, Passy, Antony, Bobigny, and Chevilly. If, then, the Merovingian and Carolingian estates and villages were the direct heirs of the Gallo-Roman, there was, according to Jullian, a continuity of civilization and agrarian exploitation running from antiquity up to the present day.

The research of Roblin appears to undermine the Jubainville-Jullian explanation by arguing with ample evidence that Gallo-Roman domains owed their names not only to their masters but also to common names. For example, the Latin *Buxus* that in French is *buisson* (bush or thicket) became *Buxiacus* and eventually Boissy or Bussy. Roblin found thirty toponyms derived from proper names as against forty derived from common names, with the origin of another ten being uncertain. He proves that many of the common names were those of streams, rivers, routes, and other parts of the physical terrain that he could not precisely date because they even antedated the Celtic or pre-Roman period. Noting also that numerous villages (*communes* today) around Paris were named after such saints as Etienne, Maurice, Martin, and Germain, he contends that the foundation of a church upon a certain site generally followed soon after the introduction of the cult of the saint for which the church was named. When he could date the introduction of a saint's cult into a region, he could then be reasonably accurate in his dating of the church and obtain a clearer notion of chro-

nology. Such painstaking research has permitted Roblin to date the clearance of thickets and forests in the area around Paris as well as their eventual settlement and cultivation.

A treasure house of information, this convincing and perhaps definitive work on the settlement and clearance of lands of the so-called *Civitas Parisiaca* in the Gallo-Roman and Frankish periods contains detailed studies of all the communes in the Paris area, indexes of place and common names, an exhaustive bibliography, supplementary appendixes, and fine aerial photographs.

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JOSEPH R. STRAYER. *The Albigensian Crusades*. (Crosscurrents in World History.) New York: Dial Press. 1971. Pp. 201. \$7.95.

In this readable book Professor Strayer again shows the lively confidence and ease that come from long intimacy with his favorite subject, thirteenth-century France. He begins by sketching the conditions in what, following some recent French writers, he calls Occitania during the twelfth and thirteenth centuries. He briefly details the origins and beliefs of Catharism. He then proceeds to a narrative account of the events leading up to the Crusades, the wars and politics of the Crusades themselves, the final extirpation of the heretics by the Inquisition, and the absorption of Occitania by France. There is an appendix with the author's own translations of some Catharist rituals and a somewhat exiguous bibliography.

Throughout, the author judiciously avoids catharophile extremes on the one hand and apologies for papacy or monarchy on the other. It may be that the serenity of his account somewhat mutes the human agony and that he does not, beyond the conventional expression of distaste for crusaders and inquisitors, fully plumb the pathos of the exploitation of religion by Christian leaders in order to sanction violence for political ends. Yet he conveys clearly the tragic sense of good men moving—largely unwittingly and unnecessarily—to their ruin. Pedro II of Aragon, the hero of the Catholic Reconquista, slain at Muret by the Catholic forces of Simon de Montfort; Amaury de Montfort earnestly and incompetently striving

to retain his father's conquests; Louis VIII's Occitan triumph of 1226 ending in his death from a fever contracted on the campaign; the well-meaning but indecisive Count Raymond VI hopelessly hoping to please the pope and the king and Simon de Montfort and his own people: in these and other incidents Professor Strayer gently holds an ironic mirror to the sad countenance of human folly.

For the author the chief importance of the Crusades lies in their political effects. They gave Occitania—and a Mediterranean coastline—to France, making that country the strongest in Europe and encouraging its rulers to press their power further than perhaps they ought to have done. At the same time the Crusades encouraged the papacy to be too dependent upon political and military power, particularly that of France. Thus the crisis of Crown and papacy that built under Louis IX and exploded under Philip the Fair was in large measure prepared by the Crusades.

The general reader and the student will be grateful for this book, the most coherent brief account of the Albigensian Crusades yet to appear.

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DIMITRI OBOLENSKY. *The Byzantine Commonwealth: Eastern Europe, 500-1453*. (History of Civilization.) New York: Praeger Publishers. 1971. Pp. xiv, 445. \$15.00.

This book deals with the history of Byzantium's relations with the mostly Slavic peoples of Eastern Europe and with the Byzantine heritage shared by them. The geographic focus is on the regions now constituting Yugoslavia, Bulgaria, Romania, and Russia, but the territories of present-day Czechoslovakia and Hungary also appear. The internal history of the Byzantine Empire and of the Slavic peoples as well as relations with non-Slavic (Arab, Turkish, Western) groups are discussed only to the extent that they are indispensable for an understanding of the main theme.

The geographic setting of the book is described in highly informative and readable fashion in the first chapter. It emphasizes the physical features and the medieval road nets of

the northern Balkans, of the North shore of the Black Sea, and of Russia. There follows a second chapter in which the author discusses the origins of the Slavic peoples and of some of their Turkish conquerors and their early relations with Byzantium. In the next two chapters Obolensky tells of the military, religious, cultural, and economic relations of the Byzantine Empire with the Balkan Slavs in the ninth and tenth centuries. Chapter 5 is devoted to Byzantium's relations with East Central Europe and concentrates on the missionary work of Cyril and Methodius in Moravia and on Byzantino-Magyar relations to the eleventh century. The author then turns in the sixth chapter to a consideration of the Empire's dealings with the Turks of Central Asia, the Khazars, and the Russians.

The narrative has thus reached the beginning of the eleventh century, in some cases even a later period. At this point Obolensky inserts a predominantly analytical chapter on "The Bonds of the Commonwealth," in which he describes the various types of political ties that bound the members of the Commonwealth to Byzantium—for example, the military annexation of Bulgaria by Basil II, the more tenuous subjection of Serbia, and the largely theoretical dependence of Kievan Russia. Byzantium's relations with Eastern Europe after the Fourth Crusade and the Mongol conquest of Russia are the subject of chapter 8. The three concluding chapters are again analytical and predominantly cultural. In chapter 9 Obolensky discusses the various factors facilitating or impeding the diffusion of Byzantine civilization in the Slavic world: ethnic conditions in the receiving countries; merchants, mercenaries, and monks as carriers of Byzantine civilization; geographic factors; the degree of nomadism or settled conditions; and the political organization in the Slavic countries. The tenth chapter analyzes the impact of Byzantium upon the religion and law of the Commonwealth, and the last chapter, one of the most interesting in this excellent work, literature and art. In a brief epilogue the author discusses the fate of the Byzantine heritage in the Slavic world after the Ottoman conquest of the Byzantine Empire.

The principal concept of the book, that of the Byzantine Commonwealth, is unfamiliar.

In spite of a disclaimer in the introduction one cannot help feeling that it is inspired by the model of the British Commonwealth of Nations rather than by that of the *res publica Romana* or of the Roman Empire (but see in this regard the interesting discussion of the possible survival of the Roman concept of *foederatio* on pages 276–77). Obolensky defines the Byzantine Commonwealth as "that supranational community of Christian states of which Constantinople was the centre and Eastern Europe the peripheral domain" (p. 277) and claims that "it was a real society, not a mere intellectual abstraction" (p. 3). It achieved its greatest territorial extent and unprecedented cultural and political cohesion in the early eleventh century (p. 203) and "came to a close" with the Ottoman conquest of Constantinople or somewhat later (pp. 362–63). The Byzantine Commonwealth was so real that it and its cultural centers can be mapped (see maps 7, 9, and 10). Its nature and vitality can most clearly be perceived in the realm of culture (p. 272): it created a supranational literature in Old Church Slavonic (p. 325) as well as a number of regional "recensions" of Byzantine civilization (p. 294) and permitted intellectual movements, such as Hesychasm and the thought of the school initiated by Euthymius of Trnovo, to migrate throughout its vast area. Obolensky uses the concept skillfully and discreetly, and in his hands it lends cohesion to a long work dealing with a great variety of peoples and cultures. It has its conceptual difficulties, however. Similar books could be written on Byzantium's relations with the Western and Near Eastern worlds where Byzantine civilization also had its impact. Were there additional Byzantine commonwealths in Western Europe (Venice, Southern Italy, Sicily) and in the Moslem world? Or was what Obolensky means by the term only one (largely Slavic) part of a larger whole embracing all the regions influenced at one time or another by Byzantium? Furthermore, does one do justice to the Slavic peoples by emphasizing the influence of Byzantine civilization and mentioning their pre-Byzantine or non-Byzantine traditions largely as factors facilitating or impeding Byzantinization, as Obolensky does so suggestively? Clearly the Byzantine perspective represents a healthy antidote against nationalist views of history frequently held in the

Balkan countries, but it does not tell the whole story. Whatever the answer may be to theoretical reservations of this kind there can be no doubt that Byzantine relations with, and the Byzantine impact upon, Eastern Europe are a legitimate and fruitful field for investigation and that the concept of a Byzantine Commonwealth has enabled Obolensky to produce a unified and outstanding survey of the entire field.

Indeed its strength lies in the breadth of the geographic perspective, the useful synthesis of previous investigations—many of them written in Slavic languages—and the skillful alternation between narrative and analytical chapters, which makes the book a pleasure to read. It offers a rich fare of which the summary of chapters gives only a very imperfect idea. Readers will discover in it an interesting coverage of well-known episodes in Byzantine and Slavic history, such as the account of Slavic origins, or of the ecclesiastical mission of Cyril and Methodius to Moravia and of its consequences for Slavic civilization, or of the Byzantine conquest of Bulgaria in the tenth and eleventh centuries and of the conversion of Russia. Obolensky also develops many less familiar topics—for instance, the administrative and ecclesiastical reorganization of the Balkan regions reconquered from the Slavs in the ninth century, heresies such as Bogomilism, manifestations of political or cultural resistance to Byzantinization and symptoms of regional deviations from the Byzantine model, national assemblies in Serbia, the cult of rulers and royal martyrs in Russia and Serbia, regional innovations in the liturgy and art of the Slavic countries (for example, the representations of the Feast of the Protective Veil of Our Lady in Russian icon painting). It is rare indeed that one fails to find information on topics relating to Byzantino-Slavic relations. In this regard I note the absence of a reference to recent archaeological work in Moravia, which has yielded considerable information on the Christianization of the country prior to the arrival of the Byzantine missionaries or to Basil II's subjection of the autocephalous archbishopric of Ochrida in Bulgaria to the Byzantine emperor rather than to the patriarch of Constantinople.

The schematic but adequate black-and-white maps and the ninety-three clear and carefully

selected photographs, frequently of insufficiently known art objects illustrating Byzantino-Slavic relations, are closely integrated with the text and facilitate its understanding. Footnotes are used sparingly. The book concludes with a suggestive and valuable bibliography of almost thirty pages where the reader will find, topically arranged, the basic literature both in Slavic and Western languages.

The book is so useful and interesting that it deserves a prominent role in the historical curriculum of American universities and colleges. It will of course serve admirably as reading matter for students interested in Byzantino-Slavic relations. But it is too stimulating to restrict its function. It should also find its place in courses devoting attention to the history of the Byzantine Empire, the Balkan Peninsula, or Russia, perhaps even in classes on Western medieval history.

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#### MODERN EUROPE

J. H. ELLIOTT. *The Old World and the New, 1492-1650*. (The Wiles Lectures given at the Queen's University, Belfast, 1969. Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1970. Pp. x, 118. Cloth \$5.95, paper \$1.95.

Most historians will agree that the discovery and settlement of the New World was one of the pivotal occurrences in modern history. But what exactly constitutes its significance? Was it the implantation of European religion, language, and culture upon a new continent, or was it the exploitation and domination of the Amerindian civilizations with European technology and institutions? The question has another side, too, and it is this to which Professor J. H. Elliott addresses himself. What was the impact—intellectual, sociological, economic, and political—of the discovery of America upon Europe?

The question is more easily formulated than answered, and it is unlikely that anyone will give a more succinct, thought-provoking, and meaningful analysis of it than Professor Elliott has done. He grapples with the puzzling problem of why the incorporation of the New



World into the intellectual pattern of the Old took so long. The main impediments, as he sees them, were the people's inability to fully comprehend the new societies. This was due in part to the barriers of time, distance, sociointellectual heritage, and language. Since the process of comprehending something new and different requires the reassessment and possible abandoning of many traditional assumptions, it was difficult for European society to come to terms with America. Elliott brilliantly summarizes the process by which the Indies were gradually assimilated into the European system of thought.

He then evaluates the impact of New World silver upon the economic life of Europe and the changes in political direction brought about by the expanded area of political jurisdiction and conflict. While rejecting the notion that the Price Revolution was caused by the influx of American treasure, the author sensibly demonstrates that its presence helped keep the inflation going once it was started. Similarly, he shows that although European politics certainly was not dominated by the presence of America, by the middle of the seventeenth century America was no longer a mere appendage of the Castilian and Portuguese Crowns but had been integrated into the political-diplomatic structure of Europe.

This is a thoughtful and well-written book, which should stimulate and point the way for future study and research.

DE LAMAR JENSEN

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STILLMAN DRAKE. *Galileo Studies: Personality, Tradition, and Revolution*. Ann Arbor: University of Michigan Press. 1970. Pp. 289. \$8.50.

We have all met Galileo. General education and civilization courses, basic surveys or cultural courses in the sciences, and of course the history of science itself have introduced us to him so often that we greet him familiarly by his first name—a symbolic judgment given to no other individual in the history of modern science. But few have come to know him as well as Stillman Drake. A lifetime of scholarship devoted to the great Tuscan iconoclast has brought to light essential manuscripts and pro-

duced critical editions of key works. In addition, translations and commentaries of and upon Galileo, including the work of his as well as of our contemporaries, have created a corpus Galileanna so closely connected with Professor Drake's name that we have come to think of this distinguished scholar as a proper familiar, a timeless companion of his own hero (a Sagredo come to life!).

In these essays, whose title evokes Alexandre Koyré's brilliant pioneer *Etudes Galiléennes* (1939), Drake pays homage to a great historian and clearly sets out "to suggest the complementary character" (p. 14) of his own internalist and psychological method to Koyré's externalist and philosophical approach. Thus methodology sets the stage, and where Drake's concern with *Personality, Tradition, and Revolution* (the subtitle and true theme of his work) gives us a sketch—penciled, fuzzy at the edges, drawn to scale—in short, a sketch of a human being, Koyré's logical analysis has given us a Renaissance oil, an intellectual portrait beyond mutable experience. In our asymptotic search for truth we are indebted to both.

Of the thirteen papers assembled in this volume all but three have been published before, between 1957 and 1970, mostly in *Isis*, *Physis*, and *Osiris*, but also in the *British Journal for the History of Science*, the *Journal of the History of Ideas*, and in special volumes such as the *Saggi su Galileo Galilei* (Florence, 1967) and *Art, Science and History in the Renaissance* (Baltimore, 1968). Their re-issue in this volume has allowed Drake to recreate them, fusing, altering, and redrawing the scattered arguments of his earlier gleanings into substantial, independent essays of value to both general and specialized readers. Of the new pieces, "Physics and Tradition before Galileo" and "The Scientific Personality of Galileo" will be of most interest to the general reader. The former supplies a much needed narrative statement on the filiation and continuity between Galileo's ideas and sixteenth-century physical thought. The latter teaches us all, as taught we must be time and time again, that thoughtful men as well as the blind conglomerate pressures of circumstance make history, that the personality of Galileo was a significant element in his leadership of the nascent scientific community of Renaissance Europe, that a maturing scientific

conscience would become a force to contend with in human affairs.

Taken together, these essays sing with the joy of scholarship, yet what we have are mainly tone poems and figured pieces; excellent though they may be, we can only hope that larger synthesizing compositions lie ahead.

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CARSTEN HOLBRAAD. *The Concert of Europe: A Study in German and British International Theory, 1815-1914*. New York: Barnes and Noble. 1971. Pp. x, 234. \$8.00.

Carsten Holbraad offers a basically well-documented study of the evolution of German and British attitudes toward the European states system in the nineteenth century, a topic of major importance to diplomatic historians increasingly concerned with changing assumptions about the nature and framework of international relations. From a historical standpoint, however, both the author's substantive presentation and his interpretation of the developments in international relations theory are undermined by methodological limitations, stemming most explicitly from his overly schematic organization and diagrammatic analysis.

The attempt to distinguish rigidly between two procedural stages—first, “collecting, analyzing, and arranging” his data and, second, logical and historical analysis—involves the author in methodological problems from the outset. The initial stage is carried out by the presentation of capsule summaries of successive individuals' ideas about the Concert of Europe, with the first half of the book for Germany (including Austria for the first part of the century) and the second half for Britain. Under each country the strands are separated rigidly into “conservative,” “balance of power,” and “progressive (i.e. federative)” categories, with additional separate sections devoted to the critics of each strand.

These basically successful summaries provide the book with a certain substantive contribution. However, the strict, catalog-type categorization proves too artificial and often prevents the exploration of the complexities and changes in attitudes of given individuals or in the relationship of the ideas to their historical

environment. For instance, Gentz and Metternich fit neatly into the German “conservative” category, which is in fact formulated around their dominant view of the European Restoration, but the same category seems a less appropriate label for the subsections covering their German critics. And the “conservative” strand—both pro and con—proves less distinguishable in England, where poor Castlereagh is left somewhat stranded between categories.

When Holbraad virtually defines the nineteenth-century “balance of power” category in terms of Ranke's thought, he raises interpretive questions he does not really answer. And except in terms of his basic assumption that “ideas themselves are historical factors” (p. 113) he fails to justify his inclusion of the professors and pamphleteers who rub shoulders with diplomats and men of state in his summaries. Thus more serious conceptual difficulties arise on the author's avowed “second stage” where logical and historical analysis are his aim. Had relatively more space been devoted to this process and less to the catalog itself, many of the author's perceptive insights might have been more cogently developed. As it is there is too little concern for the political milieu of the abstracted pronouncements, so that the different strands of thought become not the historical streams the author claims but a series of cataloged ideas related to each other only in facile logical constructs and neat diagrams (pp. 106, 135, 203) rather than in terms of developing historical situations.

This problem makes it difficult for Holbraad to substantiate the underlying themes that emerge from his implicit search for the theoretical roots of the disasters that beset twentieth-century European international relations. Holbraad sees a polarity developing by the First World War between the “organizational-progressive” British attitudes that were to encourage twentieth-century federative efforts and the “imperialistic anti-European” German attitudes that were to thwart such hopes and subvert the European continent. Such themes are provocative, but few will find either convincing or clear, for instance, the charted interrelationships through which Ranke's “balance of power” ideas—“the moderate and noble ideas about European politics”—became subverted by Droysen's “nationalist criticism” and He-

gel's "interstate anarchism," through the medium of Treitschke and Bismarck, so that they end up in Germany "degenerated into selfish and brutal doctrines of anti-European policy." Yet the whole course of this complicated development Holbraad is able to summarize in a brief diagram (pp. 106-07).

At a time when many historians are sympathetically looking to social-science methodology for meaningful analytic frameworks and explanatory generalizations Holbraad's book appears more as a warning about the difficulties of such approaches than incentive to use them.

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M. I. MIKHAILOV. *Istoriia Soiuza Kommunistov* [The History of the Union of Communists]. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 561.

This is M. I. Mikhailov's latest publication in a series of articles and monographs pertaining to the lives, activities, and teaching of Marx and Engels. As in Mikhailov's earlier works, especially in his *Souiz Kommunistov—Per-vaia mezhdunarodnaia organizaciia proletariata* (Moscow, 1960), in this volume the author also discusses, but in greater detail, revolutionary activities and societies in Western Europe from the 1830s to 1852. The book encompasses events preceding the formation of the Communist League in 1847 and its activities up to the dissolution of the union in 1852.

M. I. Mikhailov concentrates on the contributions by Marx and Engels in the formulation of the theory of scientific communism and their endeavors to apply it in practice. He emphasizes that Marxian scientific communism is a unity of theory and practice, and he criticizes Western Marxist writers, such as Franz Mehring and Gustav Mayer, for having failed to recognize this unity.

Organized chronologically, the book consists of three parts. The first part deals with the nature and developments of German emigré societies in Switzerland, Belgium, France, and Britain. It describes the emergence and the activities of the German People's Union, the League of the Rejected, Young Germany, and the League of the Just. The author dwells on

views and activities of their prominent members and devotes a chapter to the discussion of Wilhelm Weitling's views. The second part deals with the League of Communists during the revolution of 1848 and its role and activities in Brussels, Paris, and in German cities, especially in Cologne. The third part relates the activities of the League of Communists after the revolution and its dissolution after the trial of communists in Cologne in 1852.

Written strictly from the viewpoint of Marxism-Leninism, with inevitably abundant references to Marx and Lenin, the monograph is notable for its detailed account of complex and diverse revolutionary trends in West European countries, of the natures and activities of different revolutionary societies, and of important individuals involved in them. The book is heavily documented with both primary and secondary sources. The concluding chapter, "Sources and Historiography," summarizes and gives the author's comments on many works from the 1830s to the present dealing with his subject matter. Needless to say, Marxist historians are viewed positively, while non-Marxian Western scholars or those with differing views are not treated with much tolerance. The works of East German communist writers are especially praised. Such one-sidedness detracts from the value of this comprehensive and thorough work.

The voluminous bibliography attests to the painstaking labor of the author. To the student of Marxism, and especially to those interested in the period covered, the book is valuable for reference and as an example of Soviet thought, methods of research, and evaluation.

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JOHANNES WENZEL. *Jacob Burckhardt in der Krise seiner Zeit*. Berlin: VEB Deutscher Verlag der Wissenschaften. 1967. Pp. 176.

E. M. JANSSEN. *Jacob Burckhardt und die Renaissance*. (Jacob Burckhardt Studien, Number 1. Speculum Historiale: Studien op Geschiedtheoretisch, Geschiedfilosofisch en Ideënhistorisch Gebied, Number 5.) Assen: Van Gorcum & Comp. N.V. 1970. Pp. 260. 35 gls.

Jacob Burckhardt's historical writing subtly reflects its author's attitude toward the course

that Europe was taking in his lifetime. This subjective quality provides much common ground for two books representing opposing ideological spheres.

Wenzel's scholarly, yet polemic monograph, the first comprehensive East German Marxist treatment of Burckhardt's politics, contends correctly that Basel's aloof, highly original historian was really involved if not publicly active in the crisis of the nineteenth century. Burckhardt's reaction, Wenzel argues, was not *apolitisch* but ideological opposition to democracy's threat to traditional Swiss patriarchal society. His efforts to transcend the crisis through asceticism and culture, as well as his pessimism concerning progress, were rooted in a mature, "reactionary" world view that recognized social evils while claiming that nothing could be done about them.

This thesis draws considerable support from Burckhardt's apparent lack of social conscience, but otherwise it limps. From either a Marxist or a liberal viewpoint Wenzel's conception of the nineteenth-century crisis is simplistic. He fails to clarify the key concept, democracy, which he applies indiscriminately to majority rule, socialism, the rise of the masses, and even diverse revolutionary movements condemned by Karl Marx. Wenzel neglects to deal with a fascinating coincidence of viewpoint in Burckhardt's and Marx's critique of the revolution of 1848, the subsequent conservative reaction, and other symptoms of social unrest. Moreover, he exaggerates the sociopolitical reasons for Burckhardt's attitude, which were important but not predominant. Werner Kaegi's monumental biography presents a more credible picture of Burckhardt, concerned mainly with teaching and other duties, engrossed in the beauty of art and nature and in the wisdom of history. Materialistic preconceptions have perhaps led Wenzel to misread the Goethean humanism in Burckhardt's life: it is likely that he pursued the Ideal more as the ultimate goal of existence than as an escape from social strife.

Its shortcomings notwithstanding, the book has scholarly merit. It reflects careful, extensive reading in the primary and secondary published sources, listed in a lengthy bibliography. It is also well written. From his unusual perspective the author sheds new, if harsh light

on previously neglected facets of Burckhardt's political thought.

Where Wenzel's study is doctrinaire and one sided, Janssen's is free from bias and open to many points of view. Nevertheless, their ideas of Burckhardt in the crisis of the nineteenth century are surprisingly similar; a well-informed, reactionary conservative, committed to vigorous, intellectual resistance to the principal historical forces of his time. But, whereas the Marxist would have Burckhardt fighting for a patriarchal social order, the Dutch scholar has him living in the past, devoted to an idealized conception of the Italian Renaissance. They both underestimate the strength and consistency of his secularized Christian humanist convictions.

The first in a projected series of studies on Burckhardt's historical works, Janssen's book is intended to offer a thorough analysis of "the structure and conceptual organization" of the classic essay on the Renaissance. The text treats the more important themes roughly in their original order. A separate section, fully two-thirds of the book, contains detailed, often highly perceptive notes on a wide variety of related topics. This method, however, neglects the essay's structure as a whole. Hence Janssen's masterful analysis has a curiously unfinished quality.

Designed to complement volume 3 of Kaegi's biography of Burckhardt, Janssen's study is unconvincing at the few points where it implicitly contradicts Kaegi. Janssen fails to support, for instance, his contention that Burckhardt is a "verkehrter Pygmalion," a historicist who prefers to love a dead, idealized image of the Renaissance rather than embrace the living reality of his own time. This widely held opinion has been substantially corrected by Kaegi's stress on Burckhardt's basic good sense, as well as by Peter Gay's emphasis on Burckhardt's clear insight into the essential ambivalence of the Renaissance's relationship to modern civilization.

A product of rich erudition and sound craftsmanship, Janssen's study reflects meticulous research into Burckhardt's published writing, careful reading of Kaegi's biography, and thorough familiarity with secondary literature up to 1967 except for Gay's contribution to the *Festschrift* for Hajo Holborn.

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ARNO J. MAYER. *Dynamics of Counterrevolution in Europe, 1870-1956: An Analytic Framework*. New York: Harper and Row. 1971. Pp. 173. \$7.00.

This book is in two respects similar to Professor Mayer's earlier works. He writes it, he says, as "a confirmed leftist critic of those Allied and American policies, both foreign and domestic, that condoned or advanced, intentionally or unintentionally, the counter-revolutionary side in the era of the communist revolution." He writes it also as a man who is aware of the danger of forcing the data to fit the construct, but who fails to guard against that danger. In one respect, however, it is different from his other writings. They are so overstuffed with data that their conclusions, though preformed, appear to result from letting the card indexes speak for themselves. Here the opposite form of self-deception is relied on: he provides no data at all, and the central thesis is no more than an assertion of political belief.

What is this central thesis? It is not that "revolution is more productive of human growth, betterment and dignity than counter-revolution," though this is one of Professor Mayer's convictions. It is not even that, in the age of the communist revolution, only communist revolution can truly be revolution. It is that in such an age of crisis politics all disagreement with or opposition to revolution must by definition be counterrevolution. Counterrevolution is not merely opposition to revolution; it is, antirevolutionary. Counterrevolution is the product of the constellation of world history and not of localized national aberrations. Whether they have been aware of it or not, "those Allied and American policies that condoned or advanced . . . the counter-revolutionary side" have inevitably developed a striking family resemblance to the policies, "both foreign and domestic," of the Nazis.

This argument is not impressive as a statement of political belief. Instead of castigating those establishments which, on his own showing, have had no choice but to become counter-revolutionary, Professor Mayer should either be organizing their liquidation, or be striving to bring to an end the age of crisis politics that has produced them. As an aid to historical understanding the argument is, I believe, worth-

less because it is itself a typical product of that age.

Professor Mayer is to some extent aware of this. He closes the area of crisis politics in 1956 and refrains from extending his "heuristic construct" to the problems of the Third World: so to extend it would, he says, give it "a blunted cutting edge." In fact, it would blow it sky-high. At the other extreme, in a tailpiece entitled "Internal Causes and Purposes of War in Europe, 1870-1956," he urges that the wars of this era were peculiarly the outcome of strained and unstable internal conditions; but he has to admit that his case rests on an argument that Aristotle, Bottero, Montaigne, and Bodin discussed in other ages. In this case, moreover, he has also to admit that the cutting edge cuts both ways. Unstable internal conditions disposed elites to take exceptionally drastic pre-emptive solutions. But they also disposed them to avoid recourse to war—to embrace appeasement. In the neutral vocabulary of the social sciences, which Professor Mayer has adopted in order to keep passion in control, his conclusion is that "the calculus of the internal political effects of . . . war is more likely either to deter or to encourage recourse to war in a revolutionary era and under conditions of internal instability than in times of domestic and international equipoise."

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HARALD VON RIEKHOFF. *German-Polish Relations, 1918-1933*. Baltimore: Johns Hopkins Press. 1971. Pp. xii, 421. \$15.00.

Professor von Riekhoff's scholarly monograph is testimony to the fact that good will or even a recognition of mutual need to stand together against common dangers are not the predominant qualities of statesmen, especially when dealing with problems as complex and emotionally charged as those affecting relations between Germany and Poland from 1918 to 1933. With its corridor cutting through German territory and its inclusion of lands long under German dominion, the new Poland was for Germany nothing less than a national affront, a constant reminder of a lost war and a spur to

revise or abrogate altogether the humiliating conditions imposed on Germany after that war. "Even if the German government had correctly evaluated the potential of Poland as a partner and been willing to sacrifice its revisionist claims on the altar of reconciliation," Professor von Riekhoff says, "it remains open to serious question whether German popular opinion under the democratic process could have been induced to accept a solution of this nature" (p. 385).

It should have been easier and it would certainly have been politic for the Poles to seek good relations with Germany after 1918. They, after all, had "won" as a result of the First World War, and with the menace of Bolshevik Russia on their flank they might have seen their national interests best served in making every effort to reconcile Germany to the new political situation in Europe. Yet the Polish government, too, was swept along by popular fears and resentments in carrying out harsh and often vindictive measures against the German minority in Poland, and, fearing that any concessions to German revisionists would be a prelude to a fourth partition of Poland, the Polish government steadfastly opposed a readjustment of German territorial claims.

Professor von Riekhoff concludes that the history of Germany's relations with Poland during the Weimar period is a study in failure in political conception and practical application. The only redeeming grace he finds in German policy was "the firm commitment on the part of the German government to seek a revisionist solution by peaceful means," whereby it succeeded in preserving peace, however precariously, during the Weimar era (p. 386).

In the light of Professor von Riekhoff's depressing study it would seem that German-Polish tensions would never have been resolved until either or both sides had been exhausted in the conflict or until both were brought under the dominion of a superpower, as is now the case. It is a big price to pay for national pride and bigotry.

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STANISŁAW NAWROCKI. *Policja hitlerowska w tzw. Kraju Warty w latach 1939-1945* [The Nazi

Police in the So-Called Wartheland, 1939-1945]. (Badania nad Okupacją Niemiecką w Polsce, Number 10.) Poznań: Instytut Zachodni. 1970. Pp. 297. Zł. 50.

JAN SZILING. *Polityka okupanta hitlerowskiego wobec Kościoła Katolickiego, 1939-1945: Tzw. Okręgi Rzeszy: Gdańsk-Prusy Zachodnie, Kraj Warty i Regencja Katowicka* [The Nazi Occupant's Policy toward the Catholic Church, 1939-1945: The So-Called *Reichsgauen*: Gdańsk-West Prussia, Warta-Land, and Katowice Regency]. (Badania nad Okupacją Niemiecką w Polsce, Number 11.) Poznań: Instytut Zachodni. 1970. Pp. 306. Zł. 60.

These two books represent volumes 10 and 11 in the series Studies on the German Occupation of Poland, which began in 1946. The first monograph deals with the role of the German police in one of the Polish provinces incorporated into the German Reich, while the other discusses Nazi policy toward the Roman Catholic Church in all Polish provinces that in the aftermath of Poland's defeat in 1939 became an integral part of the Reich.

Both books could be described basically as typical doctoral dissertations not unlike many of the Ph.D. theses written at American universities. They are loaded with lengthy and detailed footnotes; the research appears meticulous and based on original German sources; the final outcome not exactly earthshaking but not uninteresting either. In the latter respect the volume analyzing Church policy is by far more interesting than its companion volume, which adds little to our knowledge of the German rule of terror in Poland.

Since both monographs deal with rather narrow problems of interest to only a small group of historians, I shall limit myself to a few comments of a more general nature. To begin with, both books are refreshingly free of any ideological connotations and devoid of the previously obligatory and frequent references to the "classics of Marxism-Leninism" that used to be cited as ultimate authorities regardless of the subject. Second, one cannot help noticing for the umpteenth time the German thoroughness and determination to put everything down on paper with the result that the history of Nazi Germany is rapidly becoming the best-documented history of all times. Last, the volume dealing with Church policy appears to

confirm once again the highly ambiguous role of the Catholic Church vis-à-vis Nazi Germany. To be sure, about one-third of all the Polish priests in the incorporated provinces perished at the hands of the Germans, yet a large number of those who survived managed without undue difficulty to accept the new reality and to collaborate with their German counterparts who in their turn acted as obedient tools of the Nazi regime in an effort to eradicate everything that was Polish. The rationale in both cases seemed to be the preservation of the Catholic Church at almost any price.

This policy of relative accommodation reappeared with equal strength in the course of Communist takeover of Poland shortly thereafter, and, once again, the Church succeeded by and large in arriving at a *modus vivendi* with the new rulers in the name of preserving the Church's power and influence.

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ERIC ASHBY and MARY ANDERSON. *The Rise of the Student Estate in Britain*. Cambridge, Mass.: Harvard University Press. 1970. Pp. x, 186. \$7.00.

Though the influence of universities upon students is a familiar enough theme in the history of higher education, the influence of students upon universities is little known. In tracing the rise of the student estate in modern Britain the authors depict the ways in which students have exercised their right of membership, inherent in their role as scholars, in the ancient and modern universities of Britain. Greater attention was paid to the student estate in Scotland than in England until the twentieth-century development of the provincial universities: though student unrest, albeit rare, was not unknown in nineteenth-century Oxford and Cambridge, their students possessed no rights paralleling their Scottish counterparts' right of choosing a rector, though this practice had been heavily qualified save at Glasgow. Parliamentary recognition of this franchise was conferred in 1858, and thereafter students in the four universities of Scotland were represented as well—in more than token fashion—on the university courts. Yet little was made of this opportunity by the students' elected representatives.

Of greater importance was the concurrent development in Scotland of the Students Representative Councils, which in 1889 Parliament recognized as embodying the student estate; their most effective work lay in the suggestions addressed to the Scottish academic senates concerning the teaching and discipline of the university. The gradual infiltration of such ideas south of the Tweed culminated in 1921 with the establishment of the National Union of Students, which until the late 1960s contented itself with a moderate approach to the problems raised by its members. Occasionally voices have been raised in criticism of such a vocational approach: "Direct work among the progressive forces in society . . . should be a fundamental principle for all members of the university." Those words, surprisingly, date to 1943, but the sentiment certainly echoes in our own time. Indeed, the authors conclude their historical survey with the unresolved question whether the student estate in Britain deserves representation on university councils and senates while it functions, at the same time, along trade-union lines to protect its interest against other (and possibly conflicting) interests within the university. The student estate, they argue, cannot expect the privileges of both.

With their interest in the present role of the student estate—Sir Eric Ashby was vice-chancellor of the University of Cambridge from 1967 to 1969—they supplement their historical study with what may be characterized as a tract for the times. In a final chapter, which merits inclusion in any university administrator's handbook, the authors outline a taxonomy of the "student conscience." Sympathetic to reasoned dissent, they articulate as well a strategy of "aggressive tolerance" for those who would disrupt in order to destroy. Not everyone, however, will share the authors' implicit assurance that university administrators will be able always to discriminate between legitimate protest and "campaigns of disruption." Yet in calling faculty and administrators to reflect upon the values that it is their unique lot to protect and forward Sir Eric Ashby and Miss Anderson have effectively utilized history to gain a perspective upon the troubled present of the university.

JOHN F. NAYLOR  
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Buffalo

A. R. B. HALDANE. *Three Centuries of Scottish Posts: An Historical Survey to 1836*. Edinburgh: University Press; distrib. by Aldine Publishing Company, Chicago. 1971. Pp. xiv, 336. \$9.75.

Mr. Haldane's volume is a welcome addition to books on the British post office. It carries the record down to 1836 when Sir Francis Freeling ended nearly forty years of service as secretary and virtual controller at the headquarters in London. Mr. Haldane has long been interested in the use of the roads in Scotland; he is a member of the legal profession (a writer to the Signet) with his offices in Edinburgh.

Postal services in Scotland began long before James VI became James I of England on the death of Queen Elizabeth in 1603. About this time the posting arrangements for mail carriage were opened to the public, and the headquarters for Scotland as well as for England were centered in London. The narrative is well organized and attractively written throughout. The chapters in the latter part of the volume following the introduction of the mail-coach services are of special interest as they are based on the abundant material available at the London headquarters but not previously used by historians. There are chapters on the town and city posts and on the country posts, with a fascinating chapter on the work of the village postmasters. There is a chapter on the posts to the Isles, another on budgetary problems, and a diverting account of "Abuses and Evasions of the Post."

Advance had been slow in the eighteenth century; there were 34 post towns at the beginning and 164 by 1800. The quarter century of war against the French Revolution and Napoleon (to 1815) had led to a much greater demand for news. Penny posts were being attached to post towns to give mail service to neighboring villages. The change was dramatic. Mail coaches were reaching speeds of nine miles an hour on the main roads, and the penny post was extending rapidly. By the time Francis Freeling died in 1836, Scotland had 250 town posts, 80 of them with penny posts. The evidence of this surprising change in the postal picture is shown in a large folding map of 1837, to be found in the pocket in the back cover. And in 1840 something like a revolution had come with the extension of the penny rate to letters posted throughout the British Isles.

This volume is well supplied with illustra-

tions, some reprinted documents, a bibliography, and numerous footnotes. The press of the University of Edinburgh has produced an attractive book that is worthy of its contents.

HOWARD ROBINSON  
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LACEY BALDWIN SMITH. *Henry VIII: The Mask of Royalty*. Boston: Houghton Mifflin Company. 1971. Pp. xii, 335. \$8.95.

Lacey Baldwin Smith in this study of Henry VIII provides not a biography but "a portrait of a mind in operation." The portrait is deduced from the king's actions; he left no personal reflections on his life and, indeed, it is argued here, was incapable of original thought. Yet this is not merely a subjective view of Henry VIII: it is based on documentary evidence that in most cases convincingly supports the interpretation. The book is above all a study of the exercise of power and its effect on a highly emotional, insecure man, bound for personal and political reasons to maintain the myth of divine-right monarchy.

The need to preserve his independence dominated the king in the 1540s, the years under scrutiny here. Already the rival factions were forming, beginning the struggle for power that would erupt after his death. Henry VIII asserted his authority by manipulating people, deliberately creating fear and confusion in those nearest to him in order to discover their hidden purposes. Information was the basic instrument of coercion, and he collected it assiduously not only by this means but by the routine sifting and absorbing of the flow of detail put before him by his advisers. This knowledge, retained by an exceptionally good memory, gave Henry the professional competence in government that helped him to keep control to the very end. The legend of an idle king is here refuted. His well-attested reluctance to read and sign documents until he returned from hunting—when fatigue often became the next excuse—is seen as evidence not of slackness but of a fundamental difficulty in decision making. When the duty could no longer be postponed Henry applied to it a "structured and ritualistic" approach that evaded the responsibility of reaching a decision based on real understanding. This approach appeared most clearly in religious matters. To Henry



VIII, as to many of his subjects, religion was essentially a bargain with God, both temporal happiness and eternal salvation being the reward of a scrupulous observance of God's laws. When disaster struck, the failure that had invoked the divine vengeance had to be located and removed. In Henry's case this necessity eventually drove him, fearful though he was of committing himself, to put all his trust in his own judgment. As supreme head of the Church Henry stood alone, sustained only by his high conception of the role of a Christian prince.

War was a relief from tension, an absorbing activity calling for no complex decisions. Its prime purpose was psychological: to enable the participants to achieve honor and glory. Beyond this it was held to improve the moral fiber of the nation and might indeed for a time reduce political and social discord by exporting troublemakers of all ranks. To condemn Henry's wars for their futility and waste is to judge them by modern standards—but perhaps also, one might add, by the standards of the ordinary people. The "despotism of the mind" exercised by Henry VIII over his kingdom was in essence the despotism of a king who reflected the thought patterns of the ruling classes. The new light shed on Henry VIII's personality in this compulsively readable study illuminates also the courtly society in which he lived.

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THOMAS ROGERS FORBES. *Chronicle from Aldgate: Life and Death in Shakespeare's London*. New Haven: Yale University Press. 1971. Pp. xx, 251. \$10.00.

Mr. Forbes, professor of anatomy and teacher of the history of medicine in the Yale University School of Medicine, has, with the support of two grants from the Public Health Service, produced a book very narrowly based on four volumes of parish registers and eight volumes of parish clerks' memoranda books of the parish of St. Botolph without Aldgate, London, for the years 1558–1625; a book in which he gives data, sufficient for an article, on baptisms and burials, stillbirths, illegitimacy, age at death, causes of death by disease, and acciden-

tal and violent deaths. These straightforward demographic data, the essential part of the book, are apparently carefully compiled (in spite of a slight difficulty in transcribing accurately) and are sensibly handled; and because of the extraordinary detail the parish records give over some spans of years (cause of death and age at death, for example) the data are of unusual interest. The rest of the book is chaff. Forbes takes an antiquarian delight in collecting notices of births, sicknesses, accidents, and deaths, which he quotes frequently and at length; quotations constitute about a quarter of the book. Although he has tried to get up the age of Will Shakespeare from W. E. Lunt's *History of England* and A. L. Rowse's *England of Elizabeth* and from more specialized works that are sometimes not those most appropriate to Forbes's needs, the author's attempts to place his parochial findings in a broader social and demographic context and to relate them to public policy fail. The reader is not helped by being told that a "swaddler" in 1600 was "perhaps a Methodist preacher," that a schoolmaster at this time was "also known as an informer," that "piracy was not unusual as late as the sixteenth century," that William Sebright, having subscribed a mayoral precept to the wards in 1594, was lord mayor (as town clerk from 1574 to 1613 he regularly subscribed such precepts), or, at one point, that the parish of St. Botolph without Aldgate was "outside the city." The author generalizes from D. V. Glass's introduction to *London's Inhabitants Within the Walls, 1695* to establish the relative poverty of the parish in the sixteenth century, when he might have put contemporary subsidy assessments to this and other uses. He has apparently made no use of the London Corporation records, which would have shed light on the city's concern with orphans, the poor, the sick, and other matters that he treats with crude sketchiness and unnecessary speculation. It is a pity that Forbes did not publish the demographic kernel of his book in an article, which would, I believe, have introduced to a wider, interested readership these remarkably full parish records and enabled more scholars to relate his data for a London parish to other recent local population studies.

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S. G. CHECKLAND. *The Gladstones: A Family Biography, 1764-1851*. New York: Cambridge University Press. 1971. Pp. xvi, 448. \$16.00.

This is a very rich book, really several books in one. It is, first of all, the history of a family—the story of how John Gladstones, who came to Liverpool from Leith in 1787 at the age of twenty-two in order to set up as a grain merchant (and who dropped the terminal “s” from his name during the same year for reasons of business convenience), raised with his second wife, Anne Robertson, a family of four sons and two daughters. As such, the book is a study in character and personality—not merely of the young William Ewart individually, but of his nurture within the family circle where his moral and intellectual outlook was shaped.

Second, the book is the history of the accumulation of a fortune. When John Gladstone started as a corn trader in Liverpool he invested £1,500 in a partnership and contracted to receive £80 a year as annual salary. When he died, in 1851, he was worth something like £750,000. Shipping and West Indian plantations played a major part in the growth of Gladstone's wealth; and Professor Checkland, a distinguished economic historian, has gone to immense and painstaking lengths to trace the history of that growth from the available business and family records. If only as a detailed study of how merchant (as distinct from industrial) capital could be built up in the early nineteenth century, this book will prove invaluable.

Urban history, social and political, is the third major theme. John Gladstone became one of the leading figures in the great trading port of Liverpool, and, as such, was closely associated with George Canning and William Huskisson. Professor Checkland gives one a vivid sense of how local rather than national issues, primarily economic, went into the making of the elder Gladstone's political stance and thus, to some extent, into the making of that of his more famous youngest son. That stance was decidedly conservative, though John Gladstone had begun his Liverpool career as an ally of the local Whig-Radicals. This book is essential reading for anyone interested in the question of how and why the greatest Liberal statesman of the nineteenth century came to be hailed at the start of his political life by Macaulay as the

rising hope of “those stern and unbending Tories who follow, reluctantly and mutinously, a leader whose experience and eloquence are indispensable to them, but whose cautious temper and moderate opinions they abhor.”

Religion constitutes the fourth strand of this family biography. John Gladstone ceased to attend the Scots Kirk and, along with his second wife, became one of the chief pillars of Liverpool Evangelicalism. How could he reconcile his Evangelicalism with his ever larger slaveholding interests in the West Indies? It was this issue that involved him in a struggle with another great Liverpool family, the Croppers, who were Quaker Abolitionists, but whose own fortune came, in part, from American slave-grown cotton. It was this same issue that increased the psychological tensions within the Gladstone family. For Mrs. Gladstone was unable to reconcile herself to her husband's role as an Evangelical slaveholder.

Her health was frail, in any event. And illness is yet another theme that pervades this book. John Gladstone's first wife had died young. Anne, his eldest daughter, the “saint” of the family, died after protracted suffering at the age of twenty-seven. Her younger sister Helen was a perpetual invalid, sporadically addicted to laudanum. Spas, medicines, treatments, and physicians are constant factors in the history of the Gladstone family. So much so that, at times, one is under the illusion that one is reading a harrowing “medical” novel. The names of some of the characters, such as “Dr. Fallati,” Helen's doctor in Baden-Baden, and “Sporn,” her maid, contribute to that illusion.

Professor Checkland treats William Ewart's brothers—Tom, barrister and M.P.; Robertson, businessman and mayor of Liverpool; and John Neilson, naval officer and country gentleman—with loving attention. But it is the glimpses of the “grand young man” of the family that grip one most strongly: a poem written by him at the age of eight, after a passer-by had saved his life from the threats of an armed madman: “Oh! How would my parents grieve/ If they heard that I was dead/ Oh! How would my parents grieve/ If the black Worms rotted even my head”; a letter to his father, thirteen years later, giving thanks to God for having been beaten up in his rooms at Christ Church

by a party of young men who "were living in sin and had rejected Christ as their Savior"; or the author of *The State in its Relations with the Church* (1838) flinging his hat into the air and shouting "Long Live Liberty!" as he went horseback-riding out of the Papal States in 1839.

To have carried off the ambitious scheme of this book with total success would have required a historian of genius and would have resulted in a classic work. The book falls short of that. The style is, at times, curiously crabbed; the long supporting cast is not always sketched as vividly as it might have been; a piling up of details occasionally threatens the total structure. But, when all is said and done, there is no doubt that this is a major achievement, indispensable for an understanding of the Gladstones and of early nineteenth-century England.

JOHN CLIVE  
Harvard University

JULIA NAMIER. *Lewis Namier: A Biography*. New York: Oxford University Press. 1971. Pp. xvii, 347. \$13.75.

Superlatives in any critical review are always suspect. Yet it is difficult to avoid using such language in commenting on a biography that is in many ways unique. There was nothing conventional about Lewis Namier, and his widow, in writing about him, has heeded his injunction to tell the truth about everything. Whether speaking of Namier's relations with his parents, the women who entered his life, or the academic and political personages who figured in it at one time or other, Julia Namier has succeeded admirably in rendering the complexity and poignancy of many of these relations. The chapters on Namier's East European childhood and youth are remarkable, not least because they so obviously anticipate many of the difficulties that Namier was to experience as an adult. The summoning up of a vanished pre-World War I Polish agricultural society is almost incidental to a larger purpose—the consideration of how a boy, reared in these circumstances, could make England his own and, in time, come to be honored as one of England's most distinguished historians. The route

was never an easy one, nor did it lead straight to the fame that Namier knew mostly in his later years. Whether arguing about the settlement of frontier problems in East Central Europe in 1919 or defending Zionism a decade later, Namier was an isolated figure, always in touch with certain members of what would today be called the establishment, but never entirely a member of the club. The biography documents his passion but also the ambiguity of his position.

This work is remarkable for its candor, not simply about personal relations, which are rarely discussed so openly by a biographer, but also about the inner life of the man and the uncertainties that led him to despair and, on one occasion late in life, to the edge of suicide. Julia Namier knows how to blend the private woes of a spiritually troubled man with the public catastrophes of a time of tyranny without making either the occasion for maudlin observations about a civilization in decline. She knows how to render the health-giving properties of work without losing sight of the situations that even a conscientious commitment to work will not remedy. The man who emerges from this study is only partially recognizable in his histories; the woman who wrote this work has created a standard for biography scarcely less impressive than the one Namier so diligently sought to achieve in his histories. That is no mean accomplishment. Other generations will be grateful for this unique twentieth-century document.

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HAROLD MACMILLAN. *Riding the Storm, 1956–1959*. New York: Harper and Row. 1971. Pp. viii, 786. \$15.00.

HAROLD WILSON. *A Personal Record: The Labour Government, 1964–1970*. Boston: Little, Brown and Company. 1971. Pp. 836. \$15.00.

The dominant feature in these two prime ministerial memoirs is the primacy of foreign policy. Here are two men who ruled their country in periods of considerable social and political change, and they spend most of their time recording their diplomatic experiences. Their choice of emphasis is made all the odder by the fact that neither of them scored any long-term

diplomatic successes, and they suffered some very visible failures.

It has been suggested that Macmillan drifted into a greater and greater concern with foreign policy in the second half of his premiership, and this tendency may in the autobiography have spread back into the earlier half. The demands of cabinet secrecy may also have had something to do with it: presumably prime ministers spend a great deal of their time reconciling ministers who disagree with one another, but they may feel they cannot talk about these disputes unless they have already come into the open, usually through a ministerial resignation. This must be particularly important for Wilson, who is still working with most of his old cabinet colleagues, but probably Macmillan also feels the demands of loyalty.

As an honorable man, Macmillan has a number of problems about Suez: he cannot tell a lie about Anglo-French involvement with Israel, he cannot denounce his old colleagues, and he can hardly pretend it never happened. On the whole he chooses the last approach; he treats the whole crisis as though it were primarily a problem in Anglo-American relations. This solves a number of difficulties: he can show, relatively easily, that United States policy was not as comprehensible or as likely to avert the danger of war as it might have been; he can totally ignore, as though it was an incidental detail, the question of "collusion"; he can dwell on the restoration of good Anglo-American relations, and more or less ignore the weakening of Britain's position in the Middle East that followed the failure of the Suez operations.

His account of the failure of the first (1956-58) round of British negotiations about the Common Market is incomplete in a rather different way. In these negotiations the British government wanted a grouping of countries that would have industrial free trade but no common tariff and no unified agricultural policy. "Plan G," which outlined this position, would have suited Britain's needs very well, but there is no indication that Macmillan ever noticed how little it would have suited the needs of the French, who agreed to industrial free trade only because of the advantages they hoped to gain from a common agricultural policy. Macmillan condemns de Gaulle for rejecting Plan G; it is quite possible that de Gaulle's

tactics were devious, but they were not nearly as inexplicable as Macmillan makes them appear.

At this time his chief success was to lead the unwilling leaders of the United States, the USSR, and France to the Paris conference of 1960. As it turned out, nothing came of the conference, but it was certainly an interesting shift in international affairs. All the same, it is a little surprising that over one-eighth of the whole book is devoted to his visit to Moscow in 1959, especially as very little emerges that was not already known.

While all this was going on, people in Britain were moving from austerity to affluence. Macmillan was well aware of this at the time; he complains that his remark "You've never had it so good" was misapplied by his enemies, but he does not say what he thought about the changes of the late fifties. Perhaps his government simply decided to relax and let everybody enjoy the transformation. Macmillan disclaimed any desire to give moral leadership to the country and said that it was a problem for the archbishops, but it is not certain that the problem can be shrugged off quite so easily.

Fairly clearly Wilson would also like to avoid such difficult and intangible questions. It is quite possible that his premiership will be remembered mainly for the passage of liberalizing legislation on capital punishment, homosexuality, abortion, theatrical censorship, and so on, but practically nothing is said about these issues. Even comprehensive schools, which were part of the official program, get very little attention. Despite the title, this is as much a volume of memoirs as Macmillan's book: a more up-to-date, less histrionic account, but still an account of life seen from 10 Downing Street rather than the story of how the government did its work. There is of course a good deal more self-justification than in Macmillan's book, but then Macmillan made so few mistakes between 1956 and 1959 that he does not have to justify himself. Wilson is aware that he has a good deal to explain, and he makes a fairly good attempt at explanation. He certainly admits he made mistakes: small ones, on the less important issues, but his explanations are interesting and illuminating.

On the big issues, not surprisingly, he is less ready to admit mistakes. He explains why he

made the blunder of saying, after devaluation, "The pound in your pocket has not been devalued"; he does not really face all the problems involved in refusing to devalue for so long. It sounds as though Wilson now believes the pound was overvalued in 1964 (p. 6); the interesting question is whether he believed that in 1964.

Inevitably there is a great deal about the balance of payments. It is presented surprisingly unsystematically; most of the statistics are now public knowledge, and it might have been useful to print some of the tables and provide comments on progress over the whole six-year period. This would have run against the pattern of the book: Wilson tells his story in chapters about each two- or three-month period, which is very effective for showing how a prime minister's life is made up of a diversity of unrelated problems, but carries to an extreme the impression of absence of any continuity of policy for which Wilson's government was sometimes blamed.

Despite the emphasis on the balance of payments, the book clearly concentrates on foreign policy. Some of this may be for tactical reasons—the discussions with Mansholt about the Common Agricultural Policy (p. 343) help justify his opposition to British entry to the Common Market—but most of it seems to be the simple enjoyment of feeling that he was taking part in great events. His meetings with de Gaulle are very entertaining and very well told. His obvious dislike of Ian Smith may be put into higher relief to rally his Labour followers, but seems genuine and entirely justified. His meetings with Kossygin and with Johnson do not really do credit to anyone. It now seems fairly clear that neither Wilson nor Kossygin had much power to do anything about Vietnam, but they went on behaving to each other as if they could control the whole question. It is hard to avoid a faint suspicion that part of the reason was that they wanted to keep up their prestige in front of each other.

But it is also hard to avoid the suspicion that Wilson was being deceived by Johnson: a number of Wilson's best instincts, and also a number of his worst, combined to make him singularly vulnerable to Johnson, and there is every sign that Johnson took perfectly natural advantage of this. An overvalued British

pound was the first line of defense of the dollar; a Labour government that acquiesced in Vietnam was a useful item of diplomatic protection. Macmillan, or even Churchill, might have put up with such a role; the trouble is that friendship on such terms wears out, and the successor to Wilson as prime minister is a man cool enough about the United States to satisfy the discriminating taste of President Pompidou.

TREVOR LLOYD

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MAURICE MANNING. *The Blueshirts*. [Toronto:] University of Toronto Press. 1971. Pp. xi, 276. \$8.50.

MARTIN WALLACE. *Northern Ireland: 50 Years of Self-Government*. New York: Barnes and Noble. 1971. Pp. 192. \$7.25.

J. BOWYER BELL. *The Secret Army: The IRA 1916-1970*. New York: John Day Company. 1971. Pp. ix, 404. \$8.95.

Although Mr. Manning's study of the Blueshirt movement will undoubtedly enjoy its greatest circulation among specialists in twentieth-century Irish history, it deserves a more comprehensive readership. This detailed investigation of the rise and fall of Irish "fascism" in the 1930s should interest all students of the better known and more successful varieties of fascism of about the same period.

At its inception in 1932 the parent organization of the Blueshirts fit the familiar pattern of nascent fascism: ex-army officers, convinced of an alliance between the De Valera government and the "Communist" IRA and disturbed by the threat to free speech posed by government supporters' attacks on opposition speakers, formed a protection organization. Membership was broadened, and some of the more fashionable trappings of Continental fascism (including the colored shirt) were adopted. The author tells us that for two years, as the champion of the more affluent farmer during the economic crisis of the mid-1930s, the Blueshirt movement "dominated" Irish politics. Thus the movement's sudden collapse in 1934 and the disbanding of the remnant two years later make Mr. Manning's explanation of these events an important part of the book. The specialist in European history will certainly note the failure

of the Blueshirts to produce either effective leadership or a generally accepted ideology, for the ideas of the leaders, including the capital "F" fascism of a small group at the top, seem to have had little meaning to the rank and file. The author's claim that the movement failed to meet certain important criteria of interwar fascism and thus was not truly fascist is beyond dispute.

Mr. Manning has written a very good book about a "movement whose main importance lies in its uniqueness." This otherwise impressive study is marred only by a serious flaw in organization—the separation of the chronological narrative from the discussion of the movement's ideological development. The detailed study of ideology and its evolution is reserved for the penultimate chapter; some readers will prefer to begin the book there and then proceed to the opening chapters.

After reading Mr. Wallace's *Northern Ireland*, one is inclined to suggest that the Blue-shirt leaders of 1934, seeking a model to follow, should have paid less attention to Italian *fascismo* and more to Ulster Unionism. During the 1920–70 period covered by the book, Unionists controlled the government at most levels and predominated in the police, the courts, and the civil service. The Unionist formula was very simple: encourage and play upon the fears of the Protestant majority while attacking the Catholic minority as allied with "hostile" outside forces.

Mr. Wallace has produced an informative, well-written, and unpretentious work, a handbook of Ulster politics that can be read in a single sitting. The core of the book is the evolution of Northern Ireland's political parties and institutions, and the changing relations among Stormont, Westminster, and Dublin. Unfortunately since the book's completion in mid-1970, the agony of Ulster has made the modest political and institutional reforms suggested by the author appear sadly inadequate.

Mr. Bell's *The Secret Army* is certain to be a more controversial book than Mr. Wallace's rather bland study of Northern Ireland. Some readers will almost certainly charge *The Secret Army* with being a one-sided glamorization of IRA violence; others may reply that the pro-army bias is never hidden, that the book is exceptionally well researched, and that glamoriz-

ing violence by historians has been going on for centuries. It is to be hoped that this debate and the excessive emotionalism of the author's conclusions will not obscure the one indisputable merit of the book—its detailed evaluation of the IRA as a revolutionary movement. It details one of the longest histories of revolutionary activity available, and "to dig into the history of the IRA is to uncover not only what to avoid but how to persist, to endure, to suffer disaster and to maintain the ideals and the organization" (p. 374).

GALEN BROEKER  
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J. H. WHYTE. *Church and State in Modern Ireland, 1923–1970*. New York: Barnes and Noble, 1971. Pp. xii, 466. \$13.50.

The extent to which the Roman Catholic Church can and does affect governmental policies in the Irish Republic is one of the most hotly debated issues of the current Irish imbroglio. Ulster Unionists charge that the southern government is a theocratic state which, if it ever gained jurisdiction over them, would promptly deprive them of their civil liberties at the bidding of cowed inquisitors. Catholic spokesmen reply that the Irish hierarchy is just another interest group like, say, the trade unions. John Whyte sets himself the task of assessing these divergent claims.

Nearly half of the book is devoted to a careful treatment of the background, course, and aftermath of the 1951 controversy over the so-called Mother and Child Scheme. Dr. Noel Browne, energetic young minister for health in a coalition government, collided head-on with the Catholic hierarchy over regulations to implement a health act of the previous government to which the bishops had expressed misgivings only after its enactment, and then only privately. Though the scheme's proposed state intervention into areas that might involve sexual morality was the most obvious source of friction, Whyte shows that the clash had much wider ramifications. Systematic Catholic social thought had come rather late to Ireland and, in the 1930s, had crystallized into a naively rigid and conservative form of the corporate thinking or "vocationalism" expounded in Pius XI's 1931 encyclical *Quadragesimo Anno*.

Throughout the 1940s Whyte finds trouble brewing between Catholic spokesmen obsessed with finding ways to limit state power and civil servants reacting against repeated suggestions that their really rather modest power ought to be circumscribed by a host of consultative bodies representing "vocational" groups. Dr. Browne took the "bureaucratic" side on a particular issue and found himself opposed not only by a well-defined "vocational" group, the Irish Medical Association, but also by the concerted forces of the Church. His short but brilliant ministerial career was terminated following what looked very much like an ultimatum from the hierarchy.

Though the episode certainly lends credence to the "theocratic" interpretation of Irish politics, Whyte finds considerable evidence for the other interpretation. The Mother and Child crisis represented the apogee of episcopal intervention in the politics of the Irish state. In the 1920s and 1930s, to be sure, the Free State did enact a number of elements of the Catholic moral code into law, but there is little evidence that the bishops had to apply pressure to achieve this end. On the contrary, it resulted from "general agreement in Irish society on the necessity of such measures." Since 1960, moreover, there has been a marked tendency to relax the "integralist" pattern of law and administration which assumed that to be Irish was virtually to be Catholic, and the hierarchy has in general acquiesced in this tendency.

Whyte finds that the Church's position on a continuum between theocratic dictator and just-another-interest-group is affected not only by the circumstances of the time but by the degree of episcopal consensus and the nature of the issue at stake. The state and individual public figures can, indeed, treat the bishops rather cavalierly in cases where a specifically national interest is thought to be involved. In areas such as personal morality and education, however, the hierarchy's opinion carries much more weight. Indeed, deference to the Church's claims in education was so complete until the 1960s that this topic seldom appears in the book. As education usually bulks very large in discussions of Church-state relations elsewhere, this fact deserves perhaps a bit more attention than the author gives it. Whyte concludes that there is no simple answer to the question "How

much influence does the hierarchy possess in Irish politics?" but he provides a highly useful analysis of how that influence varies under the impact of different circumstances. Though denied access to ecclesiastical archives he has made valuable use of interviews and of a wide range of published materials. His judgment is invariably sound and his logic compelling. The book is required reading for anyone who hopes to understand contemporary Ireland and is the most important historical work yet to appear on the post-Reformation Catholic Church in Ireland.

DAVID W. MILLER  
Carnegie-Mellon University

JEAN-PIERRE BARDET *et al.* *Le bâtiment: Enquête d'histoire économique, XIV<sup>e</sup>-XIX<sup>e</sup> siècles. Volume 1, Maisons rurales et urbaines dans la France traditionnelle.* (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Centre de Recherches Historiques. Université de Caen, Centre de Recherches d'Histoire Quantitative. Industrie et artisanat, Number 6.) Paris: Mouton. 1971. Pp. 544. 78 fr.

This story of buildings and, to a lesser extent, the building trades is a series of five scholarly essays by members of the Centre de Recherches d'Histoire Quantitative de l'Université de Caen, whose director, Pierre Chaunu, has contributed an excellent introductory essay. It is a call to action for the current generation of historians. The message itself is not new, but it has become more insistent since the last war. We must attempt to see how the common people—those who were not politically, socially, or economically important in a prestatistical era—lived.

One of the most persistent qualities of these essays is their very tentative nature. Pierre Gouhier, for example, has investigated the cost of constructing the presbytery in eighteenth-century Normandy. He would also like to examine construction costs in general in the eighteenth-century countryside, but he has been hampered by the lack of meaningful data such as written contracts or agreements that clearly spell out the obligations of the various parties. Instead, Gouhier concludes, private contracts often were not recorded or, worse

still, were oral. Furthermore, the building industry in the *ancien régime* was often only one segment of a barter economy. The peasant might pay for all or part of the construction of his house by providing board and room or by giving the artisan part of his crop. On the other hand, many peasant families provided the manual labor themselves, calling in specialists—masons, carpenters, etc.—only for the finishing touches. The construction cost of a presbytery, which Gouhier equates with the house of a *laboureur aisé*, can be determined because the construction contract had to be approved by the intendant and ratified by the *Conseil du Roi*. Using this more dependable although restricted data, Gouhier discovered that construction prices in the Norman countryside doubled between 1750 and 1789, conforming to the general price increase. There is no precise way, however, of correlating prices in Normandy with rural construction in general.

Hugues Neveux has utilized the especially complete *fonds* at Cambrai to examine the cost of construction and maintenance from about 1400 to the beginning of the eighteenth century. The dramatic rise in the price of wood, particularly oak, as well as the diffusion of stone as a building material meant that fewer wooden houses were constructed after 1650. Stone buildings, constructed to last for centuries, were increasingly erected by all segments of society. "Houses of stone became less the exclusive property of the wealthy" (p. 256). Neveux has included a number of leases showing construction costs that date to the late fourteenth century; nevertheless, he estimates that he has examined only about one per cent of the houses of Cambrai. Chaunu compares Neveux's painstaking methods with the reconstitution of families undertaken by demographers.

In addition to Chaunu's introduction, there are also contributions by Gabriel Désert and Jean-Pierre Bardet. Of particular interest to the specialist are extensive original documents that help the reader to appreciate construction procedures, prices, wages, and so forth as far back as the fourteenth century.

Building was the most important nonagricultural economic activity in Europe until the second half of the nineteenth century, yet this subject has been generally neglected by historians and economists until recently. Chaunu and

his colleagues have done us a service by publishing the first results of their research and suggesting the direction such research may take in the near future. We can anticipate more studies in the future, which, one hopes, will allow us some comparison with the rather narrow investigations made in this book.

THOMAS F. SHEPPARD

*College of William and Mary*

J. SHIMIZU. *Conflict of Loyalties: Politics and Religion in the Career of Gaspard de Coligny, Admiral of France, 1519-1572*. (Travaux d'Humanisme et Renaissance, Number 114.) Geneva: Librairie Droz. 1970. Pp. 220.

The struggles between Guise and anti-Guise, Catholic and Huguenot noble factions to gain control of the French Crown in the Wars of Religion have fascinated scholars and led to a range of historical interpretation. Whereas historians in the nineteenth century emphasized the religious basis of the struggle, recently the tendency has been to stress the political issues and personal ambitions hidden under the cloak of religion. Dr. Shimizu and others believe that neither interpretation does justice to the complicated interplay of religion and politics. In this first study of Coligny in almost seventy-five years the author emphasizes the subtle intermingling of religious and political motives and demonstrates the conflicting interests and loyalties that buffeted the French nobility.

Beginning with Coligny's youth and first known contacts with the Reform, Dr. Shimizu focuses on Coligny's role at court under the Guise regime and his gradual emergence as a leader and spokesman for the Huguenot party. She concludes that Coligny's fundamental motives were political; indeed she finds evidence of personal ambition to be the consistent element in Coligny's assuming a leading role in the Huguenot movement. She sees him as a statesman, primarily, and not a man of religion. She convincingly argues that while he loyally worked for the Protestant cause, he remained indifferent to questions of religious dogma because his principle concern with religion was its role in maintaining social order and justice. Coligny's affiliation with the Bourbon-Protestant faction and his public declaration for Protestantism



were thus well-timed acts of political self-preservation.

This study, modest in scope and careful in its scholarship, is not totally convincing in its conclusions. On the question of conflict of loyalties it is difficult to accept Dr. Shimizu's formulation of a conflict between religion and politics. The assertion of a conflict suggests the existence of a tension between Coligny's loyalty to the Crown and to Protestantism. Yet she makes no doctrinal case for Coligny's loyalty to the Reformed religion. The evidence she presents suggests only that Coligny single-mindedly pursued a career aimed at becoming first servant of the Crown and joined the Huguenot movement to advance his political fortune with anti-Guise factions organized to free the Crown from Guise control. Coligny was undoubtedly a religious moderate, as Dr. Shimizu argues, and it is valuable to have his political ambitions brought into focus at last. But there are points at which Coligny remains shadowy and one-dimensional. The picture of conflicting loyalty might have been sharpened if she had also considered Charlotte de Laval's spiritual influence on her husband, Coligny's correspondence with Calvin and Beza, his friendship with Renée de France, and the role of the Protestant pastor in Coligny's household.

Dr. Shimizu's scholarship is impressive; her archival evidence is precise. She has certainly examined every known letter and diplomatic dispatch and uncovered new evidence to support her argument. However, the information she has amassed appears at times to have overwhelmed her. There are several instances where lengthy digressions on questions of interpretation might have been relegated to footnotes. Elsewhere quotations could have been digested and paraphrased. But the questions she raises about political and religious motivation are important and should be applied to other personalities at Catherine de' Medici's court. Certainly Dr. Shimizu's answers help to clarify our understanding of the confused religious and political scene in sixteenth-century France.

CHARMARIE JENKINS BLAISDELL  
Northeastern University

EDMUND H. DICKERMAN. *Bellièvre and Villeroy: Power in France under Henry III and Henry*

IV. Providence: Brown University Press. 1971. Pp. x, 200. \$10.00.

Considering the extensive research upon which it is based, this book makes only a very modest contribution to the monographic literature on the reigns of Henry III and IV. Unfortunately the choice of an artificial, routine subject did less than justice to Professor Dickerman's painstaking perusal of the sources, many of which are unpublished.

The ostensible purpose is to "recreate the Weltanschauung" of these two important royal ministers by a topical presentation of their opinions on various subjects as expressed in their voluminous correspondence. Grouped into three bulky chapters (on royal government and foreign and domestic policies) are assorted nuggets of information on, for example, their reactions to dismissal by Henry III in 1588, their conventional sentiments on the horrors of war, compassion for the peasantry, acceptance of the social structure, or their concern about Henry IV's recklessness in battle. The justification for such an anthology is the assumption that Bellièvre and Villeroy "spoke for an important segment of what we today call the enlightened public." Little attempt is made, however, to keep the opinions firmly rooted in their respective historical contexts, or to compare them with differing or conflicting ideas. Divorced from the detailed, concrete circumstances to which they refer, their utterances tend to sound merely inane.

The tone, moreover, is so adulatory as to border on the hagiographic. Dickerman minimizes the overwhelming evidence that Bellièvre began to sink into illness and senility soon after his appointment as chancellor in 1599. And one would never guess that Villeroy, "the dean in chapter of all the statesmen in Christendom" (as Carew called him in 1609), was noted for his ostentatious scale of living and was rumored to be angling for a cardinal's hat.

Even this *table méthodique* of ministerial commonplaces would have been more useful and accessible to the scholar—the book cannot seriously be intended for any other audience—were it not for the unfortunate practice of tucking the references out of sight in unwieldy omnibus footnotes at the end of the volume, thus presenting the reader with a

series of nasty little "matching tests" (some of which I confess I found insoluble).

Why these two particular royal ministers? Why not Sully, Sillery, Jeannin, Du Perron, and de Thou? They are all virtually eliminated here. I am afraid that Dickerman's book is no substitute for a biographical approach. It adds little to Kierstead's study of Bellièvre or to Sutherland's work on the secretaries of state; and for the last twenty years of Villeroy's remarkable half-century career, we are still left with the old and grossly inadequate biography by Nouaillac.

ALFRED SOMAN  
Carleton College

MOHAMED EL KORDI. *Bayeux au XVII<sup>e</sup> et XVIII<sup>e</sup> siècles: Contribution à l'histoire urbaine de la France.* (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences Économiques et Sociales. Centre de Recherches Historiques. Civilisations et sociétés, Number 17.) Paris: Mouton. 1970. Pp. vii, 369. 48 fr.

Let us welcome eighteenth-century Bayeux into the magic circle of French cities that have inspired more than one solid study. In 1967 Olwen Hufton published a careful description of Bayeux during the last quarter of the eighteenth century; that study contained important information about the city's administration under the Old Regime and the early Revolution but slighted the analysis of population composition, demographic change, and city-hinterland relations. Mohamed El Kordi's *Bayeux* devotes only four or five pages to administrative matters and stops at the beginning of the Revolution; but this study runs back to 1600, deals at once with the city and its region, and provides exquisite detail on demographic structure, prices, wages, agricultural production, plus a dozen more of the items now conventionally quantified in French regional studies. El Kordi does all the analyses we have learned to expect of books issuing from the École Pratique des Hautes Études: family reconstitutions, long series of prices, studies of occupational composition, and so on. As usual, we have to endure stylish substantives like *le dynamique* and *l'historique*. And the design of the whole book is also familiar: (1) sources, (2) composition of the population, (3) demo-

graphic changes, (4) economic structure and, especially, change, (5) appendixes, mainly statistical. Every part is competent and jam-packed with information. El Kordi has followed a classic design and has done his duty by it.

The classic design has its costs. Among other things it tends to diffuse the book's argument. The student of a particular city or region finds himself in something like the position of the nineteenth-century author of a local *monographie*: compelled to fill a series of prescribed blanks whether or not his locale, his evidence, his subject, or his own proclivities lead him in those directions. For most of its way El Kordi's book lurches from description to description, unaided by a general argument. The conclusions El Kordi himself draws from his work are mainly descriptive: that irregularly declining mortality and high levels of immigration (especially of women) accounted for the city's growth in the seventeenth and eighteenth centuries; that people of the region were probably exercising deliberate control over births fairly early in the eighteenth century; that the city and its region alike normally had to import food to survive, a general shift toward stock-raising increased the need for imports, and the urban poor suffered increasingly from the high food prices that resulted from these circumstances; that Bayeux's lace industry, despite its employment of a considerable number of local women, never had great prospects and never played a major part in the region's economy; that at Bayeux, as elsewhere, the "long" seventeenth century (up to 1720 or so) saw relatively little change in general price, wage, and income levels, while the last three quarters of the eighteenth century (especially the period after 1760) brought important rises in prices and rents, thereby depressing the real incomes of the wage-earning segments of the population. For the most part he documents these conclusions amply, carefully, and appropriately. An encyclopedic account of a middle-sized city, of its hinterland, and of its *rentiers*, monks, merchants, lacemakers, and poor folk takes its place on the shelf, awaiting comparison with other cities of the seventeenth and eighteenth centuries.

CHARLES TILLY  
University of Michigan

A. LLOYD MOOTE. *The Revolt of the Judges: The Parlement of Paris and the Fronde, 1643-1652*. Princeton: Princeton University Press. 1971. Pp. xiv, 407. \$15.00.

Lloyd Moote's richly detailed, meticulously documented study of the mid-seventeenth-century "judges' revolt" provides us with one of our first authoritative accounts in English of the Old or Parliamentary Fronde and the succeeding civil wars in France. The title and the initial date of the book are misleading: Moote discusses not only the Parlement of Paris but the rise and decline over a fifty-year period of a broadly based "royal opposition" drawn from the ranks of the established bureaucrats (*officiers*) and of the well-to-do middle classes. Eloquent in their discontent, the older establishment feared the usurpations of newly appointed bureaucrats (*commissaires*), dreaded the loss of income through spiraling taxes, and, after 1643, deplored the bumbling mismanagement of the regency government headed by Anne of Austria and Cardinal Mazarin. Moote is severe in his judgments of the regent and her advisers: Anne "did not have the perseverance, patience, or intelligence" to guide the government (p. 68); Mazarin had "totally lost control due to his ignorance and inattention" (p. 87); Particelli d'Emery's financial schemes were tainted by corruption; and Chancellor Séguier lacked "Richelieu's firmness and tact" (p. 69). After five years of the regency's misrule a storm or revolt broke over Paris in the summer of 1648 at a time when the sovereign courts were sitting in joint conclave to draft petitions for redress of grievances. These grievances, codified in the famous Twenty-Seven Demands, urged, *inter alia*, the suppression of the office of provincial intendant, limitations on the arbitrary legal powers of the Crown, and an adjustment of the tax rates. In the confusion of ensuing civil wars the constitutional-administrative issues were all but lost sight of. Moote insists, however, that the judges did achieve "victory-in-defeat" by slowing down the advance of royal absolutism and by forcing reforms on the government of Louis XIV. "Without the political *via media* [urged by the judges], the Fronde would have been a total failure" (p. 376).

Moote's accomplishments are impressive: he has given order and emphasis to recent interpretations of the Frondes; he has probed, with

insight and learning, the aims and achievements of the officers; and, above all, he has provided the judges with an updated *apologia pro vitis suis*.

Moote elucidates the royalist cause with less deftness than he does that of the *officiers*. It is difficult, for example, to penetrate the various guises taken after 1643 by the so-called *conseil d'état* and by the emerging "inner council," the latter probably being the *Conseil d'en Haut*. Moote might also have discussed roles played by the refurbished *conseil privé* and (after 1648) of the *conseil des dépêches* in their struggle with men of parlement. The portrait of the principal minister is strikingly unflattering: to say that Mazarin lacked "Richelieu's ability [in foreign affairs] to distinguish between desirable goals and attainable objectives" (p. 67) ignores the tangible gains won by Mazarin at the treaties of Westphalia; to say that "his chief weakness . . . was his incredible ignorance about internal affairs" overlooks the minister's considerable abilities as a bureaucrat; and to say that he and Anne "encouraged the upheaval [of the Frondes], just as Charles I of England and . . . Count-Duke Olivares [of Spain] had incited revolution in their countries" (p. 87) strains an analogy. Ultimately Moote might have given us a clearer and more extended discussion of royal absolutism; such a discussion seems essential to our understanding of an age in which maxims of state were hotly debated, in which the fate of a law turned on phrases like *qui veut le Roy si veut la loy* or *Le prince . . . est une puissance publique* and in which the interpretations of *legibus soluta* and *lex regia* formed the background to civil war.

JOHN C. RULE

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MICHEL MORINEAU. *Les faux-semblants d'un démarrage économique: Agriculture et démographie en France au XVIII<sup>e</sup> siècle*. (Cahiers des Annales, Number 30.) Paris: Librairie Armand Colin. 1971. Pp. 387. 45 fr.

This book includes two major articles (140 pages), and a long appendix (240 pages). The latter is a collection of diversely edited, annotated, or interpreted documents: tithe returns, agricultural and commercial inquiries, and a farmer's account. In the first major article, an

expanded version of a piece that appeared earlier in the *Revue Historique* (1968), and was included (in translation) in *Essays in French Economic History* (Rondo Cameron, ed., 1970). Morineau questions the existence of an "agricultural revolution" in France in the eighteenth and early nineteenth centuries. He compares the agricultural census of 1840 with quantitative evidence on yields and productivity for the early eighteenth century. To this is added an abundant but scattered collection of evidence pointing to the same conclusion: that yields of cereals per acre or per bushel of seed stagnated during the period in question. This is excellent work, and the findings are remarkable. But to conclude from this that there was consequently no agricultural revolution underestimates the importance of new crops, particularly maize and the potato. Morineau dismisses this objection by arguing that such new crops did not truly constitute an agricultural revolution since they were not accompanied by rising incomes: the potato was a symptom of poverty rather than prosperity. But is this not begging the question?

Having argued for the absence of an agricultural revolution in France in the period 1700-1840 Morineau then argues that this casts a doubt on the existence of a "demographic revolution," since the former is supposed to have caused the latter, and he sets himself the task of justifying his doubts (the second major article). A long discussion leads him rightly into stressing diversity in the demographic history of French regions. Differences in rates of population growth in the eighteenth century are linked to the different relations between population and means of subsistence existing in the various regions at the start. He finds that, where he estimates there were large food surpluses, population growth was more rapid. On the other hand, he recognizes that rapid population growth could also be sustained on other rural, but nonagricultural sources of income, such as cottage industry. However, Morineau finds two exceptions, Languedoc and Normandy, to the positive long-run association between population growth and rural industrialization.

At this point (p. 333) he throws up his hands, and the discussion ends abruptly with the unexpected conclusion that the sources of

regional demographic differences must be sought on the side of changing patterns of epidemics and contagions (i.e. unexplained exogenous forces). The study ends with a conclusion that there was neither a demographic nor an agricultural "revolution" in France until 1850 or so, only regional variations in the degree of stagnation.

An unfortunate defect of this book is its writing style. The reader is drawn into an interminable monologue with exclamations and interrogations as well as allusions, archaisms, neologisms, and colloquialisms. Going through all of that is thoroughly exhausting. Nonetheless, the primary and secondary material that the book contains will make it indispensable for specialists of French agricultural or demographic history in the period 1700-1850.

FRANKLIN MENDELS

University of Quebec

PATRICE L.-R. HIGONNET. *Pont-de-Montvert: Social Structure and Politics in a French Village, 1700-1914*. (Harvard Historical Studies, Volume 85.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xv, 217. \$8.50.

With a sure but unobtrusive display of factual and statistical erudition based on impressive combing of national, local, and family archives, Professor Higonnet has pieced together some political and social aspects in the modern history of a village in Southern France. In the process he develops interesting generalizations about the some thousand souls inhabiting the cluster of twenty-two hamlets and central bourg, while setting the isolated community in the historical and historiographical context of France as a whole. Stressing a tripartite social structure of rural and urban "lower class," landowning peasants, and rich peasant and bourgeois "notables," the author sees a Porchnev type of late seventeenth-century class conflict insofar as the elite Protestant Montipontins stood aloof from their social inferiors' Camisard wars with Catholic state authority. Using the same social base he then shapes the community-wide support of the French Revolution around a Cobban-like theme of a bourgeois elite strengthening its traditional power. Finally he traces the results of the nineteenth-century "breakup" of the social structure: the petit bourgeois, who stayed on while the top

and bottom elements vanished, proceeded to engage in radical political ideas removed from the concrete issues of their little world while socially clinging somewhat conservatively to their impoverished way of life.

Though Higonnet does balance these bold outlines with careful sifting of details, some basic questions remain. First, the village was untypical insofar as it had no resident nobility, and its pervading Protestantism also made it less typical than the author suggests. The second objection is to the book's general, though not consistent, emphasis on social structure as a key to the events of Pont-de-Montvert's history. For example, it is straining the implications of the social elite's inaction during the Camisard wars to see this as a class conflict between rich and poor. In the case of the French Revolution, the scraps of political records left for the historian do not allow much to be said of political and social interaction. And the postrevolutionary chapters combine the assumption that "attenuation of social distinctions" caused doctrinal unanimity with the quite different stress on the impact of education on society and politics.

A. LLOYD MOOTE \*  
University of Southern  
California

IRA O. WADE. *The Intellectual Origins of the French Enlightenment*. Princeton: Princeton University Press. 1971. Pp. xxi, 678. \$20.00.

This lengthy book is the first of a projected three volumes devoted to "the reality of the Enlightenment: its origin, its development into an organic something, its consequences" (p. x). Professor Wade, well known among students of French literature for his important works on Voltaire, fears that while we know more about many particular aspects of the Enlightenment than we did fifty years ago, we may also know less about "its inner nature, its personality, its internal reality" (p. xiii). His ambition is to synthesize the partial investigations of recent decades in such a way as to reveal the true Enlightenment: the organic spirit of the age. That such an organic spirit exists (or existed), Professor Wade has no doubts. He is therefore perturbed by the fact that historians have apparently foisted their "private Enlightenments"

upon the public as authentic. It may be, however, that the conscious discipline of making a "private enlightenment" public knowledge is essential to good historical writing. For Professor Wade's attempt to analyze the origins of the one true Enlightenment has produced a book that is exasperating in its pretensions, eclectic in its method, and confusing in its logic.

Professor Wade starts his book with an introductory section entitled "Enlightenments We Have Known." The first chapter, devoted to "The Changing Picture of the Enlightenment," deals instead with political and social conditions in eighteenth-century France. The burden of the argument is that historians now regard the period as more prosperous than they once did. But the exact relevance of this interpretation to the French Enlightenment is not made clear, while Professor Wade's conclusion that we have overlooked "the fact that, for Enlightened Man, it is thought which leads the world" (p. xiv) seems largely gratuitous. The second chapter of this introductory section gives a preliminary characterization of the Enlightenment for which Professor Wade is seeking origins in this volume: the Enlightenment as defined by Kant and Cassirer, for whom the problem of knowledge was the critical issue. Following the work of Henri Busson, Professor Wade traces this problem back to the Italian naturalism of the Paduan school and above all to Montaigne, whose "Que sais-je?" was to become the constant watchword of the free-thinkers and the constant challenge to the thinkers (i.e. the philosophers).

Professor Wade then marshals a rich parade of free-thinkers and thinkers in three columns: Renaissance, baroque, and classical. There is much erudition here and much fascination. But there is little that is conclusive. There is, for example, a most interesting series of vignettes of five free-thinkers (Charron, La Mothe Le Vayer, Naudé, Patin, and Sorbière). But when Professor Wade asks exactly what these writers contributed to the making of Enlightenment thought (pp. 201-06), the question gets lost in a discussion of the views of the literary historians upon which he has drawn. More crucial is the lengthy section (the longest in the book) on "The Structuring of Enlightenment Attitudes" (pp. 418-643). In effect this is a discussion of five philosophers (Malebranche,

Leibnitz, Locke, Newton, and Bayle) with a brief excursus on the battle of the Ancients and Moderns and an anomalous coda on movements for reform under Louis XIV. Again, in themselves each of these discussions repays reading, for Professor Wade has read widely and thoughtfully. But taken together they do not amount to an analysis of the "structuring of Enlightenment attitudes" in any sense. Perhaps this is because Professor Wade distinguishes between structure and form, the spirit of the age developing only as "structure" takes on "organic form." But it means that many readers may remain puzzled by this book until Professor Wade's intentions become clearer in its sequel.

K. M. BAKER  
*University of Chicago*

VINCENT W. BEACH. *Charles X of France: His Life and Times*. Boulder, Colo.: Pruett Publishing Company. 1971. Pp. xv, 488. \$16.50.

In this study Beach has brought together research that extends over more than a decade. Some of it has been published previously, but there is much fresh material to help us better understand the contribution of Charles X (1757-1836) to the royalist counterrevolution in France.

Thirteen chapters span his life. After exploring neglected years in the dissipated court of Louis XV the narrative follows the tedium and fantasies of twenty-five years of exile and helps us to understand the problems inherent in Charles's assumption of the Restoration kingship at the age of sixty-seven. New information clarifies developments in three major areas: Charles's role in the Assembly of Notables as opponent of reform, the rationale for the coronation ritual and legislative program of his first three years as monarch, and the program and collapse of the Polignac ministry.

Throughout each of these crises and over a forty-two year period, the author remarks a stubborn consistency in Charles's political behavior. The political ideas spelled out in Charles's defense of aristocratic and monarchical privilege in the Assembly of Notables during the "pre-Revolution" remained his program for the active years of counterrevolutionary insurgency (1789-1804) as well as for

the decade of furtive conspiracy that followed. And, despite expedient and sporadic support for the Charter during the Restoration, these ideas emerged again in the July decrees that precipitated the revolution of 1830. For Beach this consistency is more stubborn blindness than statesmanship, but he does recognize the king's personal involvement in an ambitious foreign policy, a successful resolution of the contested revolutionary land settlement, and the marked economic progress of the Restoration.

One of the reasons why we have not had a scholarly biography of Charles before this has been the difficulty of gathering relevant sources. It is to Beach's credit that he has been able to unearth important unpublished primary materials in special collections at the Public Record Office and the British Museum as well as in important public collections in France. These were supplemented by newspapers, records of parliamentary debate, and a large memoir literature (much of which is unfortunately either hagiographic or, as in the case of de Boigne, maliciously anecdotal). The secondary works drawn on for authority are also substantial, although they are often introduced rather ponderously through lengthy citations offered to the reader without comment. With the exception of the years 1805-13, every period of Charles's life is illuminated in some way by these materials.

A second problem for scholars has been the dullness of the man, projected to liberal critics in his own time and to posterity. Charles was a far cry from his illustrious ancestor the Sun King. Beach has overcome an understandable aversion produced by Charles's early philandering, intellectual mediocrity, and procrastination to present a fuller picture than we have previously had of the last Bourbon monarch of France.

DANIEL P. RESNICK  
*Carnegie-Mellon University*

ROGER PRICE. *The French Second Republic: A Social History*. Ithaca: Cornell University Press. 1972. Pp. vii, 386. \$11.50.

Not since Georges Renard contributed his volume to Jean Jaurès's *Histoire socialiste* almost seventy years ago has there been a serious social

history of the Second Republic. And this despite the best efforts of a historical society specifically devoted to the revolution of 1848, despite a centenary celebrated amidst orgies of retrospective studies and despite a modest stream of substantial publications since 1948. Professor Price's book seeks to fill this gap.

As a social historian Price is chiefly concerned with the linkage of French society to French politics, of social structure to political dynamics. The study divides naturally into three sections of comparable length: an introductory static analysis of French society prior to the February Revolution (pp. 1-94) that distinguishes between basic structure and the disruption peculiar to the period just preceding the revolution; second, a socio-political account of the revolutionary period *par excellence* (February through June 1848) of which over one-third is devoted to the social analysis of the June insurrection (pp. 95-192); third, a much sketchier account of the two and one-half years separating June 1848 from December 1851 (pp. 193-326) with, again, one-third devoted to the social analysis of the coup d'état. This adds up to a highly ambitious agenda. Price has unfortunately insisted on interspersing a running commentary on Marx's views of the Second Republic's social evolution that might have made an excellent article but tends to clutter up the book.

How successful is Price's social history? Very successful in some respects and certainly very useful, particularly to nonspecialists. For one thing the author's focus is on France and not, as has been customary, on Paris. For anyone interested in a scholarly assessment of French society prior to 1848 based on the best recent regional studies, Price's introductory section is authoritative. Furthermore he is genuinely sensitive to the variety of French society and hence to the complexity of its political response. The social significance of key episodes, such as the June Days and the resistance to the coup d'état, are more fully explored than in any other comparable work. Finally, considering its broad scope, the book is remarkably free of errors.

Yet I have misgivings that I find hard to articulate, some of which have to do with style and, by implication, organization. Historians do not spoil each other in this regard, but

Price's style runs from limp to soggy, with an occasional impenetrable sentence thrown in as additional challenge. The result was that I was so busy cutting my way through the underbrush that I was rarely aware how the forest was laid out. I am sure that each chapter does have an underlying outline, but it is not made apparent to the reader. Perhaps it is also the style that gets in the way of Price's overall interpretation. After giving the book two careful readings I remain unconvinced that any definable relationship between social structure and political behavior emerges. The author, following J.-A. Tudesq, begins by presenting France as a traditionalist society led by its notables, leading Price to an essentially determinist verdict on the revolution of 1848: conservatism was bound to prevail where so many had a stake, however modest, in the existing order. Yet in accounting for the radical sweep of the area of small peasant property in 1849 and the triumph of conservatism where large-scale agriculture deprived the rural masses of proprietorship, Price jettisons his earlier explanation. I guess I am worried by what I see as essentially circular reasoning. Because, for example, the Limousin votes socialist in 1849 the author is bound and determined to find reasons why the social conditions of the area make this inevitable. He would do so with equal gusto had it voted conservative. Perhaps social history must develop more sophisticated hypotheses.

One problem may be that Price does not marshal the full range of available evidence against which hypotheses ought to be tested. Price does best in his introductory chapters where he pulls together and displays the findings of some of the major recent French scholars in the field. Where he relies on his own, necessarily cursory archival research—namely for the period of the Second Republic itself—he depends additionally on standard *mémoires* and general histories, completely ignoring a mass of local and regional histories of the Second Republic (admittedly of unequal value). As it is, for his account of regional variations Price relies on the thin diet of the reports of the *procureurs généraux* and a patchwork of massive and recent doctoral theses that leave much of France blank.

Finally, I was disturbed by Price's carelessness in computing social data. Though I am

much impressed by quantification as a tool of social history, the quantifiable is not always what it seems. To generalize, as Price does, about the social composition of the June insurgents from the roster of 11,642 suspects arrested requires assurance that the bulk of them were indeed participants. Even an occupational analysis of the four thousand found guilty must take into account the district-by-district incidence of the repression, the industrial geography of Paris, and similar variables. A breakdown by birth place may prove equally treacherous: Do the 449 arrests of suspects born in Moselle speak for a special Mosellois propensity to rebel? Or were Mosellois in rebellion-prone trades? Or did they just happen to lodge in some neighborhood, like the Faubourg St. Antoine, subject to unusually severe repression? Quantifiable data have to be scrutinized by conventional historical methods just as much as any other findings. It does not, alas, speak for itself.

In summary, Price is successful in providing a cogent summary of what we know about French society at mid-century and in detailing the strains within it. He is less successful in relating social change to the nature and direction of political dynamics, but this is, of course, an extraordinarily difficult task. The publisher, incidentally, should be awarded the Legion of Demerit for inventing the most inconvenient method of footnoting yet devised by the ingenuity of man.

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HELMUT RUMPLER. *Ministerrat und Ministerratsprotokolle 1848-1867: Behördengeschichtliche und aktenkundliche Analyse*. With a foreword by FRIEDRICH ENGEL-JANOSI. (Die Ministerratsprotokolle Österreichs und der österreichisch-ungarischen Monarchie 1848-1918. First Series: Die Protokolle des österreichischen Ministerates 1848-1867. Einleitungsband.) Vienna: Österreichischer Bundesverlag für Unterricht, Wissenschaft und Kunst. 1970. Pp. 131, 4 plates. Sch. 498.

HORST BRETTNER-MESSLER, editor. *Die Protokolle des österreichischen Ministerrates 1848-1867*. Part 6, *Das Ministerium Belcredi*. Volume 1, 29. Juli 1865-26. März 1866. With an introduction by FRIEDRICH ENGEL-JANOSI. (Die Ministeratsprotokolle Österreichs und der österreichisch-

isch-ungarischen Monarchie 1848-1918. First Series, part 6.) Vienna: Österreichischer Bundesverlag für Unterricht, Wissenschaft und Kunst. 1971. Pp. lxxxiv, 351. Sch. 950.

The protocols of the Austrian ministerial councils have long been recognized as an important source for Austrian history and have been profitably used by many scholars. A joint Austrian and Hungarian project is in progress to publish these documents in their entirety. The first series (1848-67) is being compiled by Austrian scholars, the second (1867-1918, excepting the period 1871-83) by Hungarians.

Two volumes have now appeared in print. Helmut Rumpler's slim but valuable introduction to the first series, *Ministerrat und Ministerratsprotokolle*, presents a cogent summary of the role and significance of the Council of Ministers. Rumpler, the general editor of the project, also discusses the organization and the guiding principles adopted for the publication of the documents—as well as some of the peculiarities and limitations of the *Protokolle*. Waltraud Heindl has added useful tables of the personnel of the various ministries, 1848-67. The first volume of documents, compiled by Horst Brettner-Messler, covers the first eight months of the ministry of Richard Belcredi (July 1865-March 1866). The redaction is superb and will enhance both the ease and value with which the documents can be used. The volume is nicely indexed and includes a table of contents for each protocol and a list of all participants in the councils. A bibliography of pertinent secondary works for the period is added (though it is hardly comprehensive). Each protocol is printed in full, and there are explanatory and reference notes by the compiler. Friedrich Engel-Janosi has contributed a detailed introduction.

The fifty-six protocols printed here indicate clearly the limited authority exercised by the *Ministerrat*. The overriding concerns for Austria in these months were the Hungarian question and the contest with Prussia in Germany. Yet the council touched only sporadically and unsystematically upon the Hungarian issue and upon foreign affairs hardly at all; only three times was foreign policy even discussed. On the other hand, a wide range of economic and social concerns were brought before the ministers. The financial plight of the Empire was a



persistent and formidable problem and, as these documents reveal, was an added imperative for the resolution of the Hungarian and Austrian constitutional questions. The loan of 1865, finally negotiated with French bankers, came under such disadvantageous terms that it was openly described in the session of November 7 as "a swindle."

Conversely, this was also a period of economic expansion, and considerable information on the gradual industrialization of the Empire is available in the protocols. The extension of the railway network, the formation of a joint stock company, and numerous discussions concerning taxation policy and the promotion of various industries can be mentioned in this regard. There is also some information of use and interest on social and cultural developments in the monarchy—religious affairs, press relations, and educational questions all received attention.

As historical sources, the protocols of the *Ministerrat* have obvious limitations; the council shared the shadow rather than the substance of power. Nevertheless, anyone interested in the Habsburg Empire should welcome the appearance of further volumes in this project, which has been impressively inaugurated here.

RICHARD B. ELROD

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SAMUEL BERNSTEIN. *Auguste Blanqui and the Art of Insurrection*. London: Lawrence and Wishart. 1971. Pp. 364. £4.00.

A good biography of Blanqui is needed. Unfortunately Dr. Bernstein's effort, while offering some useful new information and interpretation, does not fulfill the need. The author rightly criticizes much existing work on Blanqui, both the calumnious tradition of conservative observers and a tendency among French communist writers to treat Blanquism as essentially a precursor of the Marxist movement. Utilizing an extensive manuscript collection on Blanqui, Bernstein offers a variety of personal details, including material on the huge portion of Blanqui's life that was spent in prison. Bernstein presents new material on Blanqui's judgment of events in which he participated, such as the revolution of 1848,

and the author has new insight into Blanqui's political thought, stressing the conspirator's belief in the motive power of ideas and a hostility to religion and superstition that outweighed his eloquent denunciation of capitalism. Though the overall interpretation of Blanqui is familiar, he is placed more understandably in the French intellectual and political tradition. Blanqui's failure to rouse mass support is also intelligently assessed.

The approach is largely descriptive and chronological. One aside on Blanqui's psychology misfires badly. Even the description is often confused by inserted accounts of general developments in French history—developments quite familiar in outline and based on limited and often dated secondary accounts. This pattern is less pronounced in the section of the book dealing with Blanqui's activities after the 1850s. Here the Blanqui manuscripts are more detailed and revealing, and the author is more content to assume that his readers know something of the general background. Even here, however, we are treated to a number of simplistic judgments of the Second Empire and the French working class.

The descriptive approach involves considerable repetition of interpretive points. We are told and retold of Blanqui's romanticism and his inability to communicate with workers. A few other judgments are repeated without significant proof; thus the claim that Blanqui was a racist and an anti-Semite. Blanqui seems constantly to be held up to a model and found wanting: he lacked a general theory of history, he did not properly understand class structure, he was a French nationalist in the Jacobin petty bourgeois tradition, he was conspiratorial rather than systematic. He was not Marx.

The book presents some information on the formation of the Blanquist movement in the 1860s. Its career under the Third Republic is sketched, but there is no overall assessment of the impact of Blanquism on later protest movements. In sum, the book should be read by anyone interested in political and social agitation in nineteenth-century France. But one must hope that it will not impede a more analytical and broadly researched study of this archetypal revolutionary.

PETER N. STEARNS

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DAVID STAFFORD. *From Anarchism to Reformism: A Study of the Political Activities of Paul Brousse within the First International and the French Socialist Movement, 1870-90*. [Toronto:] University of Toronto Press. 1971. Pp. xiv, 367. \$15.00.

Studies of nineteenth-century French anarchist and socialist movements generally treat Paul Brousse (1844-1912) as a figure of secondary importance and his *Politique des possibilités*, his Possibilism, as a species of political opportunism. In this carefully researched and clearly written account Dr. Stafford has endeavored to give Brousse his due, documenting his "crucial role" within the anarchist Jura Federation, the congresses of the First International, and the post-Commune French socialist movement, as well as seeking to establish a "logical line of development" that links Brousse's early anarchism with his Possibilism and to identify him as a realist and pragmatist to whom "action was the guide to theory."

During the period of his exile in Switzerland (1873-78), Brousse was indeed an intransigent, left-wing anarchist and a prominent exponent of "propaganda by the deed." The increasing isolation and general ineffectiveness of the anarchists, combined with his growing concern with the revival of the socialist movement in France, led Brousse to re-examine his doctrinal commitments. When he returned to France in 1880 he had considerably modified his revolutionary posture and was prepared to make use of legal means of action, the ballot and electoral campaigns, within the existing structure of bourgeois political institutions in order to transform society. Believing that an anarcho-communist society could be achieved in piecemeal fashion through the enactment of significant socialist measures on the local level, in municipalities and communes, Brousse and his followers gained control of the recently formed French Socialist party and, subsequently, replaced its Marxist-inspired Minimum Program with one that emphasized specific, practical, and moderate reforms. During the 1880s the Possibilist party, in contrast to the Guesdist and Blanquist parties, scored at the polls, winning seats on the Paris Municipal Council and in the Chamber of Deputies. Brousse himself became president of that council in 1905 and a deputy the following year, although his party

entered a period of rapid decline after the schism that occurred at the Chatellerault Congress in 1890. This fine study is based on a number of collections of manuscript materials located in Amsterdam, Paris, Bern, Brussels, Imola, and elsewhere, private papers in the possession of the Brousse family, relevant anarchist and socialist newspapers and periodicals, as well as a wealth of other primary and secondary sources.

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JAMES HARDING. *The Astonishing Adventure of General Boulanger*. New York: Charles Scribner's Sons. 1971. Pp. xii, 251. \$8.95.

It would be an act of charity to omit reference to this book from serious journals of history. But Harding's *Boulanger* comes from a reputable old-line publishing house, which touts it as "an authoritative biography," as "a lively, yet scholarly account of the life of the original man on a white horse." Let us overlook that word "yet" and its suggestion that "scholarly" and "lively" are incompatible, and let us even concede that the "white horse" phrase is a figure of speech, inasmuch as Boulanger's black horse is justly famed. What then must we do? Responsibility to even the minimum scholarly tenets of the historical profession requires that note be taken of some of the work's serious flaws.

The total absence of any internal documentation must be deplored. There is not, in this "scholarly" work, a single note citing the author's source of information. He tells of having got from Boulanger's grandson access to the family archives, which yielded "much unpublished material which has been incorporated in this book." There is no way of telling what Harding used from the Boulanger papers and where he used it. There is also a four-page appended bibliography that contains a strange, and unclassified, mixture of primary and secondary items, of ferociously *tendancés* tracts and scholarly studies. The text of the biography itself is liberally sprinkled—without a clue as to their basis—with conjecture as to motives and feelings and with (imaginary?) direct dialogue.

Further, a claim to objectivity with refer-

ence to Boulanger's career cannot be accepted from an author whose total bias against the Third Republic is implicit throughout the book. Concerning prominent Republican figures of the era, unrelievedly lurid and snide characterizations—spiced with elaborate gutter anecdotes—abound. Also there seems to be no institutional or political arrangement or practice of the maligned *Troisième* that is not held up to ridicule. While there can be no argument *ipso facto* with an attempt to revise the generally held unfavorable image of Boulanger, Harding's sympathetic image lacks credibility owing to the warped context in which the general is presented. Even though this biography makes no pose of being in the "life-and-times" genre, elementary decency demands serious effort at judicious and balanced treatment of other actors in the drama and of the setting in which they moved.

Harding is not a historian, and the absence of historical perspective that mars his life of Boulanger demonstrates the hazards that await the amateur when he treats one small segment in a complicated story—in this case, the story of the survival and consolidation of the Third Republic. No serious student of French history denies the sordidness, the shockingly unethical affairs that marked the 1880s in French public life. But with Harding and his smears of Boulanger's opponents these scandals are not adequately fitted into the Third Republic's ongoing development in which these very opponents, unsavory as many were, may nevertheless be seen as having made a genuine contribution to French democracy—to the point, even, where a reputable historian such as Georges Duveau could wish that "the German Social Democrats had shown with reference to Hitler a little of the energy that the Floquets and the Constans used to contain the Boulangist flood."

Any analysis of Boulanger must sooner or later come to revolve around the celebrated episode of the coup—that wasn't and must take into account both how propitious in fact the moment was on January 27, 1889, and why the general behaved as he did. Harding with great dramatic detail builds up a scene of mob enthusiasm with strident voices urging a march on the Elysée. He portrays Boulanger as resisting the summons because to act in an overt

coup went counter to the habits of discipline and obedience engrained in him as a soldier and because of his dedication to legality. Harding's alleged "revision" consists of thus minimizing Boulanger's infatuation with his mistress as the cause of his inaction. But Frederic Seager (see David B. Ralston's review of *The Boulanger Affair*, *AHR*, 75 [1970]: 1138–39) has already shattered this "romantic" version of the tale. Seager, moreover, found no evidence of appeals for going "To the Elysée!" He states categorically that "the source of the legend of a *coup manqué* cannot be traced to contemporary accounts" (p. 204). Surely Harding, before he asks for his study to be taken seriously, should have come to grips with Seager's well-documented contention, if with nothing else.

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H. L. WESSELING. *Soldaat en krijger: Franse opvattingen over leger en oorlog, 1905–1914*. (Speculum Historiale, Number 4.) Assen: Van Gorcum & Comp. N.V.; Dr. H. J. Prakke & H. M. G. Prakke. 1969. Pp. 284. 29 gls.

The author of this balanced and intelligent book confesses that he chose his subject out of a feeling of astonishment. Astonishment at the fact that in the ten years before 1914 French writers and publicists of a much wider circle than just the more traditional rightist and conservative intelligentsia and for a larger public than just the small ordinary reactionary groups started to praise the maintenance and actual use of the French army and to idealize war in general in such a way that the author rightly speaks of a "serious delusion." To describe war, for example, as a "poème indicible du sang et de beauté," as was done by E. Psichari in 1908, requires a great deal of understanding.

The first three chapters try to paint the background against which this idealization of army and war has to be set. In chapter 1 the author sketches the rather ambiguous and awkward position of the French army during the first thirty years of the Third Republic: on the one hand accepted as the inevitable and necessary means by which a defeated nation regained its position of power in the world, on the other hand distrusted and rejected by the republican bourgeoisie, an idealistic intelligent-

sia, and the radical socialists out of antimilitaristic and pacifistic ideals and progressive political aims. From 1900 onward, however, the author discerns a general change of mood (ch. 2) that made acceptance of the French army easy, and former differences seemed to be smoothed over. A kind of "mal du siècle," a feeling of uncertainty and insufficiency, caused a strong reaction against what was seen to be "decadent" and "weak." A younger generation, of bourgeois and labor-class origin, seemed to be exhilarated by the great colonial-imperialistic successes of France and the impressive French victories in sports events and feats of aviation. And finally the international political crises of Tangier and Agadir (ch. 3) seemed to move public opinion in France toward real sympathy for a strong French army and toward a firm interest in theories that idealized war, without, of course, ever being as fully absorbed or as intellectually stimulated by such thoughts as were the more consistent and expressive interpreters of this trend of feeling.

Perhaps one could criticize this rather long introductory part of the book for its somewhat impressionistic approach. Whether the antimilitaristic and pacifist tendencies died, as the author seems to imply by remaining silent about them for the period after 1900, remains doubtful. Whether the mood of the "mal du siècle" was as serious as the author, by an excellent choice of quotations and references, wants to suggest is still open for discussion. Whether Tangier and Agadir were really stronger stimuli for the change of mood in France than some earlier international crises is a question that can also be put with some justification. On the other hand, one is grateful that the author avoids easy monocausality and simple determinism; rather, he attempts to describe shifts of moods embedded in a network of feelings and situations that taken individually did not necessarily cause such change nor inevitably result in a general war.

The main part of the book tends also to be an attempt at understanding by description rather than explaining by analysis. It is a systematic and well-ordered demonstration of the kind of expressed feelings and mentalities, reasonings and argumentations that glorified the army as a real school for the nation (ch. 4) and that idealized war as a blessing and purifica-

tion for that nation (ch. 5). The author offers no quantitative study measuring the change of public opinion—he refers with due praise to Eugen Weber's *The National Revival in France, 1905-1914* (1959) to which this study could be considered to be complementary—but instead he gives a qualitative description of a climate of opinion, carried forward and supported by such well-known and important writers as Péguy, Barrès, Brunetière, Massis, or Psichari.

The last paragraph of the last chapter describes the argument "la guerre pour la guerre" as the culmination of all war idealization in its most irrational style and as the perfection of the human necessity of "vivre dangereusement" (Emile Faguet). Such thoughts in their most consequential radicality are most certainly of the utmost importance to describe and to understand because they did not die after the First World War and were certainly not the monopoly of French prewar ideology.

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ERIKA SPIVAKOVSKY. *Son of the Alhambra: Don Diego Hurtado de Mendoza, 1504-1575*. Austin: University of Texas Press. 1970. Pp. xvi, 450. \$10.00.

Not since the publication thirty years ago of Angel González Palencia's *Vida y obras de Don Diego Hurtado de Mendoza* (and for the first time in English) has there appeared a full-length biography of this important Spanish Renaissance humanist, philosopher, poet, historian, soldier, diplomat, and governor. Mrs. Spivakovsky's sympathetic treatment of Mendoza's active career does not stray far from Don Angel's distinguished work, but she does add a dimension to sixteenth-century history in her perceptive interpretation of Mendoza's role in Habsburg diplomacy.

Don Diego was a complex and sensitive personality, more responsive to the currents of thought transforming Europe in the early sixteenth century than were most servants of the Imperial Spanish crown. Yet he was staunchly traditional in his medieval conceptions of service and honor. His aggressive promotion of Charles V's interests in Italy during his ambassadorships in Venice (1539-44), Trent

(1545-46), and Rome (1547-52) and his vigorous governorship of Siena (1547-52)—resulting in repeated confrontations with the pugnacious Pope Paul III and the eventual revolt of the Sienese—is vividly described by Mrs. Spivakovsky. Much less space is devoted to Mendoza's writings. This may be understandable in view of the anomalous circumstances of their composition and Mendoza's strange reluctance to claim any of his literary creations, but, it is nevertheless unfortunate that Mrs. Spivakovsky did not see fit to give more than two pages to Mendoza's famous *Guerra de Granada* and only the briefest mention of his many poems. From her previous articles we know that Mrs. Spivakovsky attributes the *Lazarillo de Tormes* to Mendoza, but she says almost nothing here about this literary classic in the context of Don Diego's public service and makes no attempt to summarize her conclusions concerning its authorship. She does affirm, however, that Mendoza's intellectual bent was in part responsible for his failings as a man of affairs.

As a study of Mendoza's diplomatic career this is a valuable addition to a fuller understanding of the man and his times. Many insights into the tempestuous struggles among Habsburg, Valois, Farnese, and Medici are revealed. The author's documentation is extensive, especially from the archives at Simancas, Florence, and Madrid, but a rather jaundiced picture of Spanish-French and Spanish-papal relations results from the almost total neglect of French and papal sources. This is doubly surprising since the French and Vatican archives are among the richest and most accessible for this period. A further dimension would be added to Mendoza's portrait and the Habsburg-Valois rivalry would certainly appear more clearly if the observations and opinions of the French ambassadors were used along with those of Mendoza and his correspondents. All of which suggests that even this fine book is not the last word either on Mendoza or on the intricacies of power politics in the sixteenth century.

DE LAMAR JENSEN

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ORLANDO RIBEIRO. *A evolução agrária no Portugal mediterrâneo: Notícia e comentário de uma obra de Albert Silbert*. (Chorographia, Série

Histórica.) Lisbon: Centro de Estudos Geográficos, Universidade de Lisboa. 1970. Pp. 226, 5 maps.

Orlando Ribeiro's little book is a commentary on and a critical evaluation of the massive two-volume (1,216-page) work of Albert Silbert, *Le Portugal Méditerranéen à la fin de l'ancien régime, XVIII<sup>e</sup>—début du XIX<sup>e</sup> siècle. Contribution à l'histoire agraire comparée* (Paris, 1966). Silbert's work deals with a region comprising the province of Alentejo and Lower Beira (*Beira-Baixa*), which together total about one-third of the whole country. This region has several distinctive geographical features, although I have some reservations about the division of Portugal into "Atlantic" and "Mediterranean" regions when the latter are, in fact, situated a long way from the sea of that name. Silbert's point of departure was the manuscript "Dicionário Geográfico" drawn up by order of Pombal after the disastrous earthquake of November 1, 1755, in order to try to ascertain the extent of the damage on a country-wide basis. As the questions were addressed to the parish priests, the replies are of unequal value, but many of them contain reliable statistics and statements concerning the local population and economy. Silbert also used a wide range of archival sources (many of them untapped before) and the relatively limited amount of printed material on Portuguese rural history that was available, supplemented, of course, by lengthy visits to the area. He was thus able to produce a work in the best tradition of the *Annales* school, a work densely documented from primary sources, which is essential reading for anyone concerned with Portugal in the period 1750-1850, and to which (as Ribeiro stresses) this monograph is a guide and commentary, rather than an exhaustive summary.

As the leading Portuguese historical geographer (who himself studied the same region over thirty years ago) Ribeiro is uniquely fitted to explain and evaluate the importance of Silbert's work, which Ribeiro does clearly, concisely, and fairly. Historians who cannot read Portuguese will be grateful for the twenty-five-page review in French that is reprinted at the end; but Ribeiro acknowledges that it is impossible to summarize Silbert's book adequately in this

length. One of the most interesting points that emerges is the persistence of various forms of collective agrarianism in this region. Ribeiro argues cogently that they probably derive from very remote (perhaps pre-Roman) times, whereas Silbert considers them to be of relatively recent origin. Forms of land tenure and of the rotation of crops varied widely, and both authors demonstrate that the large landed estates of the Alentejo differed greatly from the latifundia of Spain and southern Italy with which they have often been compared. The social structure of the towns and countryside is analyzed in meticulous detail. Agricultural, economic, and social historians will find Orlando Ribeiro's monograph an appetizing hors d'oeuvre to Silbert's very substantial banquet.

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J. GÉRARD-LIBOIS and JOSÉ COTOVITCH. *L'an 40: La Belgique occupée*. Brussels: CRISP. [1971.] Pp. 517. 480 fr. B.

The central position of the authors of *L'an 40: La Belgique occupée* is that the German conquest and occupation of their country in 1940 aggravated all of the problems of Belgian society and that the issues faced in the first year of the subjugation continued to be the major issues for the remainder of the war. To make clear what the problems of Belgium were in 1940 the authors have an introductory section in which they sketch in rapid fashion the question of neutrality, the Flemish-Walloon controversy, the multiple-party system, and the military predicament of a small state.

After the "war of eighteen days," the situation within the country became ever more complex because of a variety of issues. First, there was the "royal question," which revolved around the decisions of the king to be commander-in-chief of the armies in the field rather than to remain at the head of his civilian government, to call for a surrender rather than to fight on, to remain in the country and to share the fate of his people rather than to go abroad and lead a resistance government, and to seek concessions from the occupier for his people. Second, there was the knotty question of organizing an autonomous administration

under German military authority and the making of inevitable compromises to keep the "Belgian" administration intact. There was the matter of getting the economy functioning in order to give people employment, even though it was known that much of the industrial production would go to Germany and that the Berlin clearing account would be used to avoid payment in full at market prices. Last, there was "la faim," which seems to have been the major catalyst in turning the people against the occupier.

All of these questions, and many more, are treated with competence and fairness. Yet the story fails to distinguish adequately between the important and the insignificant, contains such a vast amount of detail that a question may be obscured, and often leaves the reader wishing that some idea of the sequel were included in the narrative. Furthermore, the text is not easy reading. In fact, it seems to consist at times of disintegrated notes that were never reduced to a comprehensible form. The volume will, however, find a place in the literature of World War II, for it has a great reference value, is reliable, and has an excellent and extensive bibliography.

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FINN GAD. *The History of Greenland. Volume 1, Earliest Times to 1700*. Translated from the Danish by ERNST DUPONT. Montreal: McGill-Queen's University Press. 1971. Pp. xiii, 350. \$16.50.

Scholars interested in the complex history of Greenland must not only use scant historical sources but rely upon recent archeological and anthropological information found scattered through relatively unfamiliar journals and written in Scandinavian languages with which they are often not familiar. It is thus particularly helpful to find here a brilliant synthesis of such materials written in English, which will certainly remain definitive for years to come.

Its author, Finn Gad, who has adapted this volume from his recent Danish work, which contains even fuller notes, deals with three major themes covering Greenland's history down to 1700 A.D. The first concerns itself with

pre-Eskimo and Eskimo migrations into Greenland from the Northeast, which began as early as 1600 B.C. and continued with certain breaks until the more recent neo-Eskimo colonization of the island from 1200 to 1700 A.D. His second theme deals with Norse settlement from the late tenth century down to its end during the late fifteenth century. His third theme concerns the rediscovery of Greenland and its Eskimo inhabitants by Europeans during the sixteenth and seventeenth centuries. Throughout, in order to explain the nature of Norse and Eskimo economy, society, and culture, the author makes use of a wealth of information derived from archeological and other research that helps illuminate many problems of Greenland's early history perplexing to scholars.

A few of his major points deserve emphasis. First of all he shows that Norse settlers arrived in Greenland during a period when it contained almost no native inhabitants, when it possessed a climate that allowed sheep and cattle to flourish, and when Norse settlers could exploit arctic resources, derived from narwhal and walrus, which were in great demand in Western Europe. Their settlements thus remained prosperous down to the thirteenth century. Only after 1200 did these Norse come in contact with neo-Eskimos, who possessed an arctic culture originating in Alaska and who were advancing down Greenland's east coast.

At first these contacts were peaceful ones and only grew hostile in the mid-fourteenth century when Eskimo pressures may have forced the abandoning of the northern Norse settlements. Whatever caused this abandonment it proved fatal to the colonists by cutting them off from those arctic products that made outside contacts with Europe profitable. Nevertheless, irregular connections with Europe were maintained for more than a century until the weakened survivors died out or were absorbed into a neo-Eskimo population that spread until it occupied the island's entire coastland.

The author believes that climatic change to colder temperatures played a role in all this by making animal husbandry increasingly difficult, by multiplying icebergs (which discouraged European mariners), and by producing a more arctic climate (which was particularly attuned to Eskimo culture). At any rate, it was a new and different Eskimo-inhabited Greenland that Eu-

ropean whalers and explorers encountered during the sixteenth and seventeenth centuries.

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GUNNAR QVIST. *Fredrika Bremer och kvinnans emancipation: Opinionshistoriska studier*. [Fredrika Bremer and Women's Emancipation: Studies in the History of Opinions]. (Kvinnohistoriskt Arkiv, Number 8. Scandinavian University Books.) Göteborg: Akademiförlaget. 1969. Pp. viii, 296. 36 S. kr.

Gunnar Qvist's book is one of those, part historical and part literary, that at worst fall irretrievably between two stools, and at their best produce new perspectives for historians and literary scholars alike. It can be firmly placed in the latter category. It contains a wealth of clear and well-documented material on the changing position of women in Sweden in the nineteenth century, while tracing the evolution of Fredrika Bremer's own views on the subject, principally from the beginning of the 1830s to the publication of her major novel *Hertha* in 1856.

It has long been held that *Hertha* was instrumental in bringing about reforms concerning the legal status of women, in particular the provisions of a government bill automatically allowing them to attain majority without resort to a court of law. Gunnar Qvist, however, demonstrates that Fredrika Bremer's view had little or no effect on this bill, the main lines of which had been decided before the publication of *Hertha*. The great debate resulting from the novel was concerned more with the broader perspective of Bremer's thought, in particular her semimystical idea that woman, being the younger creation, was closer to God and therefore in a special position to bring about an ennobling of the world. It was this utopian perspective that was at the center of the controversy; in this idea Brenner was alone, deserted even by those who supported her more practical demands.

Gunnar Qvist works methodically, giving first a general historical review of the decades into which he divides Bremer's work, then examining the works themselves and comparing their implications with what she says in her private letters, and finally taking a look at the reactions of the critics. After the *Hertha* debate

and a summary of her later views, it is, however, surprising to find a chapter on Bremer and the slave question in America. However interesting, this is not strictly speaking relevant to the rest of the book, as is perhaps implicitly accepted by the author, for he makes no reference to this chapter in his English summary. This summary is adequate, though one wishes the language had been checked by a competent English scholar.

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JOHANNES SZIBORSKY. *Die Germanisierung der Mark Brandenburg in der märkischen Geschichtsschreibung des 16. Jahrhunderts unter dem Einfluss von Humanismus und Reformation*. Breslau: [the author.] 1969. Pp. 272.

This book is a doctoral dissertation completed in 1969 at the University of Giessen. As a result of World War II Sziborsky, who was born in Breslau in 1929, was forced to leave that area, which was to become part of Poland, and he moved to Westphalia in West Germany. Here he completed his university education, which included studies at Münster and Düsseldorf. As with most German doctoral dissertations the documentation for this study is impressive. The text contains 206 typewritten pages and added to this are some 28 pages of notes that contain 626 citations. These are all placed at the back of the book following the text.

The text is divided into four parts. First, there is the unnumbered *Aufgabe* or statement of the problem. Here the author clarifies the use of the term *Germanisierung*, which might be translated Germanization. This is a literary study, not a political history of Brandenburg. As a result Sziborsky assures the reader that the term Germanization must not be associated with the era of National Socialism or with earlier nationalism. From this portion he then turns to the introduction, which is an examination of medieval chronicles relating to the Germanization of Brandenburg. These include bishops' and princely records or chronicles as well as accounts of the Mark itself. From this introduction the author then turns to the main part where he examines the works of a number of writers such as Georg Sabinus, Ernst Brotuff, Wolfgang Jobst, and others. In the fourth and

last part (numbered three in the book since the *Aufgabe* receives no numeral), Sziborsky treats modern research.

For the specialist in the German language and for the student of literary history this is a valuable study. It is easy to read and well documented, and the format and size make it easy to handle. American graduate schools might well consider changing the size and form of American doctoral dissertations and copy some of the features of those in German universities. The  $5\frac{1}{2} \times 8$  size, even though paper bound, is certainly easier to handle and to shelve than the traditional  $8\frac{1}{2} \times 11$  American dissertations. Also, the *Aufgabe* in this particular book presents the problem more clearly than the usual preface, and the brief autobiographical sketch of the doctoral candidate can be very interesting.

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URBAN PIERIUS. *Geschichte der kursächsischen Kirchen- und Schulreformation* (MS germ. quart. 91 der Staatsbibliothek Preussischer Kulturbesitz). Edited and with an introduction by THOMAS KLEIN. Marburg: N. G. Elwert Verlag. 1970. Pp. 718. DM 56.

In the last third of the sixteenth century, Electoral Saxony, the homeland of the Reformation, changed its religious course several times. Although Elector August had sternly suppressed all crypto-Calvinist tendencies and had imposed the Formula of Concord as the confessional norm, his son, Elector Christian (1586-91), accepted the Reformed position and, with the help of his most prominent counselors, Nikolaus Krell and Andreas Paull, gradually opened Saxony to Calvinist influences. Professors and pastors no longer had to sign the Formula of Concord, and reforms along Calvinist lines were initiated: exorcism was eliminated, pictures were removed from churches, liturgy and ceremonies were simplified, and the christological and sacramental definitions of the Formula of Concord were rejected. These changes along Reformed lines have been called a Second Reformation. After all, as historians have already distinguished a Protestant Reformation, a Magisterial Reformation, a Catholic Reformation, a Radical Reformation, and a Third Reformation, we



might as well add a Second Reformation. When Elector Christian died in 1591 the strictly Lutheran faction again seized control of the government. All Calvinist changes were immediately eliminated and the orthodox Lutheran position restored. In a fit of vengeance the most prominent Calvinist politicians were thrown into jail and Krell was executed in 1601 after ten years of imprisonment.

As superintendent at Dresden and afterward as professor of theology and general superintendent at Wittenberg, Urban Pierius (Urban Birnbaum, born in 1546 at Schwedt in Uckermark, died in 1616 at Bremen) was deeply involved in the Calvinist reforms. After the catastrophe of 1591 he occupied high positions in the Calvinist churches at Heidelberg, the Upper Palatinate, and in Bremen. It was at Bremen in or about 1608 that he wrote his *History of the Reformation of Church and School in Electoral Saxony* (covering the years from 1586 to 1591) in an effort to refute the violent accusations and calumnies raised against him and other Reformed theologians by Philipp Nicolai and other Lutheran authors. Pierius's *History* is based both on personal recollections and a considerable number of documents that had been saved before the disaster of 1591. Written in an unusually lively German style, the work is valuable because it presents a great amount of concrete information both on individuals and events, such as the characters and views of various powerful theologians and superintendents, hitherto unknown sermons held by court preachers before the Elector at Dresden, the effectiveness of censorship, or conditions at the university of Wittenberg. Although rich in factual and colorful detail, Pierius's history is strongly biased in favor of the Calvinists. And yet Pierius was such a shrewd observer and eloquent writer that one cannot help being fascinated by his book. In any case, it offers so many new and intimate facts that it is indispensable to our understanding of the fateful events in Saxony during the years from 1586 to 1591.

Lost for 250 years, the manuscript was discovered in 1962 in the collection of the former Prussian State Library by Thomas Klein, who had previously written a study of Saxony's Second Reformation. Klein has edited a slightly shortened version of the bulky manuscript,

adding a fine biographical study of Pierius and an extensive bibliography. Historians of sixteenth-century Protestantism will appreciate the publication of this unusually informative manuscript.

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EDA SAGARRA. *Tradition and Revolution: German Literature and Society, 1830-1890*. (Literature and Society.) New York: Basic Books. 1971. Pp. 348. \$8.95.

Few literatures provide a richer field for investigating the transition from a traditionalistic to a modern social order than nineteenth-century German literature. For the few Western nations which negotiated that transition more smoothly and successfully than Germany did, the documentation of the problems posed by modernization, problems artistic and intellectual as well as political and social, is not so searching, intensive, or dense as for nineteenth-century Germany. There the hopes and expectations, the apprehensions and disappointments, the tensions and turmoil involved in modernization came to the fore time and again at the highest creative level. This creative encounter and its uniqueness are understandable when we consider that Germans were the first in a long line of peoples, in Europe and then throughout the world, to confront modernization as a threatening challenge from without, rather than as an urge born from within. The modern had to be assimilated, selectively and piecemeal, in Germany—grafted on to a system unprepared either to accept or reject it. How leading German writers, during each of Germany's phases of development from the Biedermeier period to Bismarck, came to grips with this situation is the theme of *Tradition and Revolution*.

The result is one of the few comprehensive and comprehensible works on the subject in English. The author's blend of social history and literary interpretation enables the reader at all times to see the creative process not only as a passive reflection of reality, but also as an original response to it. From her discussions of individual writers and their works, many familiar to English-speaking readers, some not, we

learn in each case what the landscape of the writer's vision was, how it came into being, wherein it accurately reproduced reality and wherein it transmuted reality, and what the writer's personal and artistic attitudes toward his material were. True, this book lacks the sociological acumen of a Karl Mannheim or an Ernst Kohn-Bramsted, the interpretive powers of a J. P. Stern or a Hans Mayer, and the philosophical and polemical verve of a George Lukács or a Lucien Goldmann. Still, *Tradition and Revolution* provides both a better understanding of nineteenth-century German history and a valuable contribution to our understanding and implementation of literature as a useful and unique source of historical documentation.

ROBERT ANCHOR

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JOHN R. GILLIS. *The Prussian Bureaucracy in Crisis, 1840-1860: Origins of an Administrative Ethos*. Stanford: Stanford University Press. 1971. Pp. xvi, 269. \$8.75.

German historians and sociologists have long been concerned with the role played by the bureaucracy in Prussia's historical development. Professor Gillis's book directs our attention to the two decades that bracketed the revolution of 1848. He argues that during these critical years conflicts within the bureaucracy combined with profound social and political changes in Prussia to produce a new "administrative ethos," one that forced the bureaucracy to assume a passive and "neutral" posture, allowing it to become a tool of authoritarian governments for the remainder of the century.

During the early decades of the nineteenth century the Prussian bureaucracy was inspired by a spirit of corporatism; defined as a *Beamtenstand* by the Prussian Code of 1794, it was, as Otto Camphausen described it, an "aristocracy of experts." By the 1840s, however, this corporate tradition was breaking under the stress of modernization, which had necessitated the introduction of a younger group of university-trained officials who soon grew frustrated by the intense competition for positions, poor pay, and a seniority system that protected older officials and slowed promotion based on merit. These younger officials gradually abandoned

the corporate image of the profession in favor of a moderately liberal one that demanded a fuller expression of civil liberties. Senior officials, on the other hand, responded to this pressure from below by imposing more difficult examinations for entrance into the civil service, by resorting to secret personnel files (*Konduitenlisten*) for blocking promotions, and by instituting a tighter disciplinary code. The revolution of 1848 brought this conflict into the open. Some officials wrote for liberal journals or became active in the liberal political organizations spawned by the revolution; others, especially older officials, joined conservative groups. These activities resulted in pressure for political conformity from both sides. First the liberal governments and then their conservative successors were willing to take disciplinary action against bureaucrats who endangered their programs. After Olmütz, the Manteuffel regime undertook to eliminate opposition from within the bureaucracy. In 1854 the *Konduitenlisten* were reintroduced, and politics became a prime consideration in promotion. Thus the failure of the revolution and the pressure of the reaction conspired to promote political obedience within the bureaucracy. Moreover, increasing professionalism fostered the expedient doctrine of political neutrality and with it, greater conformity.

Gillis's study, which draws heavily upon archival materials available in West Germany, offers significant insight into the Prussian bureaucracy during an important epoch. His last chapter, a description of the social composition of the bureaucracy from the 1850s, is especially helpful for the information it presents on the new governing class that emerged on the eve of German unification.

ROBERT M. BERDAHL

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WILHELM KLUTENTRETER. *Die Rheinische Zeitung von 1842/43 in der politischen und geistigen Bewegung des Vormärz*. Volume 1; volume 2, *Dokumente*. (Dortmunder Beiträge zur Zeitungsforschung, Number 10, parts 1 and 2.) [Dortmund: Verlag Fr. Wilh. Ruhfus.] 1966; 1967. Pp. 171; 173-266.

This history of *Die Rheinische Zeitung*, a paper that was confidently launched to match other leading journals in format, circulation,

and influence, rests on a thorough knowledge of the political and intellectual milieu of the 1840s, notably in the Prussian Rhine Province. Part 2 (*Dokumente*) includes biographical sketches of the contributors together with a listing of the articles that each wrote, though complete certainty was difficult in some instances because all items appeared anonymously. The statutes of the newspaper company, financial statements, the publication contract, and a number of contemporary appraisals of the nature and impact of the *Zeitung* appear among the documents.

Originally favored by Prussia and financed by loyal bourgeois stockholders, the paper early came under the editorial control of a younger generation of Young Hegelian radicals. Karl Marx was influential from the first from behind the scenes and ultimately became its chief editor. This study should help to dispel the impression that the *Zeitung* experienced a distinctly radical shift under his disciplined editorship. Through a special listing of all articles dealing with press questions the author stresses the role of the paper in the discussion of the current lively issue of freedom of the press. A sample of the less favorable contemporary evaluations of the paper, other than those that came from official sources, might have been included. The journal was perhaps more widely damned than praised and may owe its immortality to the fact that it was suppressed because the "liberalized" Prussian censorship regulations judged a paper by its tendency (*Tendenz*).

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EBERHARD KOLB. *Die Kriegausbruch 1870: Politische Entscheidungsprozesse und Verantwortlichkeiten in der Julikrise 1870*. Göttingen: Vandenhoeck & Ruprecht. 1970. Pp. 150. DM 15.80.

The controversy over responsibilities for World War I has overshadowed another similar debate that has been going on even longer. It concerns the origin of the Franco-Prussian War of 1870-71. Was it France and Napoleon III-Ollivier-Gramont who caused that war, was it Prussia and Bismarck, or was the "war guilt" shared by both sides?

Dr. Kolb acquits Bismarck of having planned the collision and places the blame for the war on the French leaders. There is nothing in the sources, he claims, to indicate that Bismarck wished to maneuver France into a war when he urged that Prince Leopold of Hohenzollern-Sigmaringen accept the candidacy to the Spanish throne. Kolb sees the key event that touched off the crisis in the warning given by the French government that France would never tolerate a Hohenzollern prince on the throne of Spain. This statement, made before a wildly cheering chamber of deputies, roused French emotions to fever pitch and deprived the government of its freedom of action.

When Prince Leopold abandoned his candidacy, Kolb continues his argument, the French leadership could not content itself with this success, but felt compelled for domestic reasons to demonstrate even more clearly France's preponderance over Prussia. This it did by asking King William I of Prussia for a pledge never to allow any Hohenzollern to ascend the Spanish throne. The request was denied, with the known consequences. Given the determination in Paris for a showdown, Kolb concludes, the Ems dispatch played but a minor role in precipitating the crisis.

Kolb argues his case with a great deal of ingenuity, but his analysis fails to convince. Apart from the fact that he passes over material that shows Bismarck's conduct in a much more unfavorable light, the available evidence is ambiguous and lends itself to various interpretations. (Josef Becker, in an essay published in *Historische Zeitschrift* [212 (June 1971): 529-607], examines many of the same sources that Kolb used but arrives at conclusions refuting Kolb's thesis.)

More important, Kolb all but excludes domestic considerations from his investigation. Yet there is a good deal of evidence suggesting that the completion of German unification, if it was to be achieved from above, did not permit any further delay and could be accomplished only amidst a national crisis, such as a war with France. Pressures from business circles, the prospect of a reassertion of the liberal camp, and serious threats to the budgetary arrangements concerning the army most likely also affected the chancellor's actions. In any event these factors cannot simply be brushed

aside, as they are by Kolb. However, given the circumstantial nature of the evidence, the debate will doubtless continue—without ever being settled conclusively.

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WINFRIED SCHÜLER. *Der Bayreuther Kreis von seiner Entstehung bis zum Ausgang der Wilhelminischen Ära: Wagnerkult und Kulturreform im Geiste völkischer Weltanschauung.* (Neue Münstersche Beiträge zur Geschichtsforschung, Number 12.) Münster: Verlag Aschen-dorff. 1971. Pp. viii, 293. DM 48.

The so-called "conservative revolution" of the nineteenth and twentieth centuries attacked the modern forces expressed in the French and Industrial Revolutions and preached a return to an idealized, lost world. One of the most interesting but previously neglected examples of this movement in Germany was the Bayreuth Circle, a loosely structured group of enthusiasts who propagated both Richard Wagner's artistic and national-racial ideas. Winfried Schüler's book is the first critical history of the Bayreuth Circle and is a welcome addition to the literature on the growth of radical conservatism in Imperial Germany. It is based primarily on a thorough examination of hitherto unused archival material in the Richard Wagner Gedenkstätte in Bayreuth, in the Ludwig Schemann *Nachlass* in Freiburg, and on an examination of the literary organ of the Bayreuth Circle, the *Bayreuther Blätter*.

An opening section discusses the development of Wagner's thought and the varied manifestations of the cultural reform movement in Imperial Germany. Schüler then traces the historical background of the Bayreuth Circle and presents short biographies of its leading figures, such as Cosima Wagner, Hans von Wolzogen, and Houston Stewart Chamberlain, as well as a listing of lesser-known men. The third section discusses the *Weltanschauung* of Bayreuth. Wagner's criticism of contemporary theater rapidly developed into a criticism of society in general. The key word in his *Weltanschauung* became "regeneration," and he saw an intimate connection between artistic and cultural regeneration. Schüler indicates that Wagner's theories, especially those of his last writings, were rapidly dogmatized after the composer's death.

For Wagner's disciples Bayreuth became a symbol of longed for artistic and cultural regeneration. In true Romantic fashion the Bayreuth Circle saw art as better able to penetrate to a deeper reality than other modes of knowledge and as best expressing the strivings of the *Volk*. For Bayreuth, key elements of regeneration became racism and anti-Semitism, since the Jews were seen as the agents of modernity in all its forms. Beginning with Wagner's essay on the Jews in music, this racism received its ideological foundation in Gobineau's works and its development into an optimistic and influential viewpoint in Chamberlain's *Grundlagen des neunzehnten Jahrhunderts* (1899). Such racism also led to Bayreuth's championing of a Semitic-free "Germanic Christianity."

Thus, Bayreuth cultivated many of the ideological components that helped prepare the way for National Socialism. As Schüler points out, the esthetic drive is one of the great gifts of mankind, but it is unsuitable as a foundation for metaphysical theories and political doctrines.

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HANS GEORG LEHMANN. *Die Agrarfrage in der Theorie und Praxis der deutschen und internationalen Sozialdemokratie: Vom Marxismus zum Revisionismus und Bolschewismus.* (Tübinger Studien zur Geschichte und Politik, Number 26.) Tübingen: J. C. B. Mohr (Paul Siebeck). 1970. Pp. xvi, 329. Cloth DM 58, paper DM 52.

The author of this seemingly specialized monograph adroitly draws out its many broader ramifications. Narrowly defined, his subject concerns the protracted debate within the German Social Democratic party over the adoption of an agrarian program in the decade 1890-1900. But Lehmann relates this internecine conflict to the broader evolution of the party toward electoral politics, the consequent need to attract new voters in a population still heavily composed of peasants, and the agonizing that resulted over what—in good conscience—the party might promise to a class Marx regarded as doomed. Reformist forces within the party gathered strength largely around this issue, and Bernstein's revisionism derived in no small part from the debate over

whether the law of concentration applied to agriculture. The reformist-inspired draft program was ultimately defeated in 1895 in a triumph of Kautskian orthodoxy, which mobilized strong rank-and-file opposition against any concessions to rural property owners. This triumph, however, left the party without any agrarian program and with a sterile indifference toward peasants that most of the other Second International parties obediently copied from the German *Musterpartei*. It remained for Lenin, who took some part in these West European controversies, to appreciate the full value of peasant support and find the right programmatic formula for winning it. Both revisionism and Leninism thus have important roots in the agrarian debates of 1890-1900.

Originally a 1966 Tübingen dissertation, this study utilizes all the appropriate sources, published and unpublished, and skillfully weaves together several levels of analysis—rank-and-file pressures, regional and national leadership conflicts, and international influences. Lehmann writes dispassionately and well, avoiding the temptation of social-science jargon. Despite the title, his treatment of non-German countries is very meager; essentially he writes SPD party history.

Perhaps because he does not seriously treat rural social conditions, Lehmann leaves the unfortunate impression that the SPD might have turned Lenin's trick in Germany if only it had discovered his magic formula, the "correct" agrarian program. Doubtless Lenin showed great political acumen in riding to power in Russia chiefly on a massive tidal wave of rural discontent, but surely he did not create the tidal wave himself. It did not exist in the West during this period, and no programmatic or tactical legerdemain could have produced it. Thus, the Nantes agrarian program of 1894 profited the French party scarcely more than the absence of such a program did the German, and the belated 1927 statement finally adopted by the SPD was equally barren in the result. No magic formula but only a certain cluster of social conditions can transform the peasantry from an immovable object into an irresistible force.

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URSULA FEIST and GÜNTER FEIST, editors: *Kunst und Künstler: Aus 32 Jahrgängen einer deutschen Kunstzeitschrift*. [Mainz:] Florian Kupferberg. 1971. Pp. 441. DM 36.

BETH IRWIN LEWIS. *George Grosz: Art and Politics in the Weimar Republic*. Madison: University of Wisconsin Press. 1971. Pp. xviii, 328. \$12.50.

From 1902 to 1933 the Berlin periodical *Kunst und Künstler* was an influential voice of modern art—especially of Impressionism—in Germany. Its contributors included critics, historians, and museum officials as well as artists and architects writing on their own work and on that of their peers. As significant as its exploration of modern art throughout the world and of its historical antecedents were the journal's critical discussions of the cultural policies of succeeding German governments—patronage, purchasing practices that often betrayed a dislike of the modern and foreign, appointments to the staffs of museums and academies, and attempts at censoring art. In a volume that is produced with the same distinguished taste that marked the original issues of *Kunst und Künstler*, Ursula and Günter Feist have brought together a selection of articles, reviews, and illustrations that, with their careful notes and bibliography, provide an excellent introduction to the journal. In an epilogue Günter Feist traces the course of *Kunst und Künstler* from its beginnings to its demise when Hitler came to power. Mr. Feist's interpretations show an impressive sensitivity to the past, but his conclusions are repeatedly hobbled by unhistorical Marxist assertions. He offers a knowledgeable analysis of the journal's mission and of the campaigns that it waged; but his admiration for its achievements is tempered by his distaste for its upper-middle-class liberalism and its opposition to social criticism and participation in the class struggle, "which was and remains a necessity of our age" (p. 399). Elsewhere an essay on Van Gogh by Karl Scheffler, who edited *Kunst und Künstler* for twenty-eight years, is judged "unacceptable" on political grounds; and similar pronouncements intrude throughout the essay and notes. No wonder Feist, who welcomes art that supports revolution but rejects art that supports the status quo, experiences some difficulty in coming to terms with a journal that consistently held

that the only judgment in art that mattered was whether it was good or bad.

In its defense of Impressionism *Kunst und Künstler* soon attained an impregnable intellectual position, though the public as a whole was by no means won over. The journal showed some reluctance to extend its sympathies to other modern directions—Expressionism, for instance, or to such more ephemeral movements as Dada. Some of their adherents, moreover, turned aggressively against Impressionism and its leading German representatives, who were among the journal's most valued contributors. Nevertheless Scheffler and his colleagues were sufficiently broad-minded to publish and discuss the work of the new men.

One of the radical artists who appeared in the pages of *Kunst und Künstler* and whom Scheffler defended against right-wing attacks was the painter and cartoonist George Grosz. Beth Irwin Lewis has written an intelligent, well-informed account of Grosz's "experience and work against the background of cultural and political life in Germany before 1933." In view of her judicious treatment of complex events it may be that the author's intentions were unduly modest. For example, disclaiming competence in handling the tools of psychohistory, she scarcely explores Grosz's psychological conflicts, though she recognizes their influence on his political attitudes as well as on his art. Here as elsewhere the important issues of his life are reported by Mrs. Lewis rather than analyzed. We might, in particular, have wished for a fuller discussion of the clash between esthetic independence and party discipline—the classic conflict of the politically engaged artist—which Grosz resolved in the early 1930s by turning away from a communism that he had come to see as basically identical with fascism. Within her self-imposed limits, however, Mrs. Lewis is successful.

Grosz's bitter drawings of officers, profiteers, and exhausted factory workers have long been valued as suggestive of the atmosphere in Berlin during and after the First World War. The pages of *Kunst und Künstler* open the view to another strand in the history of the Empire and the republic. The journal was a preceptor of the cultural attitudes of German—especially north-German—liberalism and was one of its

most representative spokesmen. It reflected the values of a group that was to suffer total eclipse but that possessed the merit—rare in times of frenetic nationalism—of feeling and acting as Europeans as much as Germans.

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*Year Book XV.* (Publications of the Leo Baeck Institute.) [London:] East and West Library for the Institute; distrib. by Leo Baeck Institute, New York. 1970. Pp. xviii, 326. \$9.50.

The Leo Baeck Institute is an organization dedicated to nostalgic research in the history of German Jewry. Since the number of Jews who still speak German with perfection is now shrinking rapidly, the exploration of their cultural past is almost akin to salvaging treasures from a sunken ship. The stories, raised from the bottom—cleaned, polished, and handled with loving care—are assembled into yearbooks, outfitted with bibliographies, indexes, and glossy photographs and given an inspiring theme, such as *Precursors of Integration—Defiance in Destruction*, the subtitle adorning the cover of volume 15.

Actually, the current yearbook is about people, most of them old friends well known to the authors and familiar to prospective readers. We meet the novelist Arnold Zweig as a disappointed emigrant in Palestine, the star of Curt Worman's essay "German Jews in Israel." The nineteenth-century banker Ludwig Bamberger and his contemporary, the publisher Leopold Sonnemann, are the centerpieces of Werner Mosse's article, "The Conflict of Liberalism and Nationalism and its Effect on German Jewry." Karl Marx is presented by Arthur Prinz in "New Perspectives" as a Jew, although some of these findings are based on papers that were previously discovered and analyzed by such investigators as Boris Nikolayevsky and Lewis Feuer. To make Marx a little more Jewish than he was, the author mistranslates Marx's reference to Disraeli as a *Stammgenosse* (one who is descended from the same group) as "our fellow Jew."

Twenty-seven pages are devoted to Ernst Loewenberg's memoir of the writer Jakob Loewenberg. Here we learn that "Loewenberg's childhood in a Westphalian village had

given him a deep love of nature," and we are reassured that "Loewenberg was never heard to make an unfriendly remark about Eastern Jews. They were familiar to him from his early childhood."

An East German researcher, Helmut Eschwege, is featured with a discussion of the resistance of German Jews against the Nazi regime, the "first treatment at any length of the role of Jews in the German resistance movement." Eschwege opens his report with lists of Jewish Social Democrats who died in concentration camps during the 1930s and works up to the small communist Jewish cell of Herbert Baum, which set fire to a Nazi propaganda exhibit in Berlin on May 18, 1942.

Five of the ten authors in this volume are more than seventy years old. The oldest of them, Selma Stern-Taubler, "Grand Old Lady of German-Jewish Historiography," deals extensively with one of the older subjects, "The First Generation of Emancipated Jews," including especially Moses Mendelssohn. The youngest contributor, Rabbi Arthur Hertzberg, concludes the volume with a two-page reminiscence of Ernst Cassirer's last winter at Columbia University. It is a touching note. Here we see how much more can be done with a few sensitive strokes of the pen to portray a man's search for his soul than was managed in all the preceding footnote-laden articles put together.

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GEORGE W. F. HALLGARTEN. *Als die Schatten fielen: Erinnerungen vom Jahrhundertbeginn zur Jahrtausendwende*. [Berlin:] Ullstein. 1969. Pp. 366.

Historians familiar with George Hallgarten's works on imperialism, dictatorships, armaments races, and German National Socialism have been fully aware of his wide learning, sustained intellectual energy, readiness for scholarly debate, and continuous interest in understanding power in its many varieties and applications. He has now undertaken the venturesome and hazardous task of recounting the life story out of which his works emerged. Throughout these memoirs, from the recollections of his childhood and youth in an upper-bourgeois, assimilated Jewish family in Munich

before the First World War, through the narrative of the refugee's travels and searches, to the overly detailed descriptions of lecture tours in the 1960s, the personal characteristics of the man and the scholar appear repeatedly: an awareness of inherited social status, an admiration for and a dependency on his mother, a fascination with the structure and dynamics of power (Machiavelli, Clausewitz, and Max Weber have been his favorite social-scientific authors), a tendency toward difficulties in personal relationships, and a persistent longing, only partially fulfilled, for scholarly recognition and importance.

Although the substance of the book is often intriguing and the style is smooth and even captivating, its worth for students of twentieth-century history is more difficult to evaluate. Throughout his life Hallgarten has known persons of prominence, and some of notoriety, and the pages of his memoirs are sprinkled liberally with their names—the Thomas Mann family (especially Erika and Klaus), Ludwig Ganghofer, Heinrich Himmler, Hans Pfitzner, Erich Marcks, Hermann Oncken, Max and Alfred Weber, Eckert Kehr, Richard Graf Coudenhove-Kalergi, Carlo Mierendorf, and many more. For a few of these, Hallgarten's impressions and characterizations will offer an added perspective and occasionally a new detail for future biographers. But on many others, those he knew only in passing, his comments are cursory and inconsequential. He has added to the information contained in an earlier article concerning his childhood acquaintance, Heinrich Himmler, but his mixture of reminiscence and interpretation must itself be scrutinized carefully to distinguish the subjective from the informative elements. Despite Hallgarten's inclination to comment briefly on many personalities, that is not the aspect of these memoirs which may prove to be of historical interest. It is only when his experiences fit into a broad cultural context or into social-political movements that his personal account assumes more significance. In the early chapters many of the features of the social and cultural milieu in which he grew up come through with vividness and credibility. From the account of his participation in student activities in the 1920s, both in Heidelberg and Munich, one can perceive some of the reasons why the sympathizers with

the Weimar Republic, student socialists, academic republicans, and the advocates of international understanding and pacifism were not more effective. As cultural elitists and political neophytes, they talked ideas and remained socially insulated from the day-to-day realities of the world for which they were so concerned. To his credit Hallgarten does not hide these weaknesses, but he seldom explores their ramifications. It is a curious and revealing oversight, for example, that in his discussions of Weimar politics and elections he does not indicate how he voted or whether indeed he did vote.

Hallgarten has contributed his own chapters to the larger story of the vexations, anxieties, and the successes of the emigrants and exiles from a Germany that rejected them and that they could not fully comprehend. Every emigrant experience had its special perplexities, and Hallgarten's were the frequently frustrated goals of finding an appropriate position for himself and a publisher for his massive manuscript on imperialism. The publishers of this book could have enhanced its usefulness had they included an index in addition to the bibliography of Hallgarten's publications.

VERNON L. LIDTKE

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*Probleme deutscher Zeitgeschichte.* (Lund Studies in International History, Number 2. Scandinavian University Books.) [Stockholm:] Lärmedelsförlagen. 1971. Pp. 266.

*Probleme deutscher Zeitgeschichte* is the second volume in a series of studies in "international history," a concept broadly understood by the publisher, the department of history at the University of Lund, Sweden, to include "not only foreign policy and international relations but also the history of different countries outside Scandinavia." The series encompasses a number of monographic works as well as collections of essays and shorter studies.

This volume falls into the latter category and consists of six chapters by various contributors on topics in twentieth-century German history: the German Monroe Doctrine in the Baltic Sea during the First World War; Raeder and German naval strategy; propaganda on the *Reichstag* fire; the Hossbach Memorandum;

Doenitz's government program in 1945; and Hitler's cultural policy after 1933.

One may, in good conscience, commend the authors in several respects. Their research is quite thorough, both in the secondary literature and the primary sources, notably the archives of the German Foreign Office and the *Bundesarchiv* in Koblenz. The essays are generally well written and the arguments clearly presented, and the translation into German is coherent and lucid.

Unfortunately these basic assets do not compensate for the serious deficiencies of the volume. In the first place, both collectively and individually, the essays reflect an almost antiquarian interest in Germany's recent past. Of the six, two are concerned with problems of naval history that convey a distinct impression of singular insignificance. Germany's so-called Monroe Doctrine during the First World War, for example, was certainly a matter of secondary, or even tertiary, importance at the time and ceased to have any relevance whatsoever when the German army fell apart in 1918. Similarly a study of Raeder's naval strategy and the sources of its inspiration does not add appreciably to our understanding of the more fundamental issues of twentieth-century German history. And even when the subject of discussion is a problem of more serious concern, such as the *Reichstag* fire, the author focuses on various contributions to the journalistic cacophony surrounding the crisis and not on the significance of the crisis itself.

Second, the collection as a whole betrays an unimaginative and almost sterile approach to some of the broader aspects of the historical discipline. In his essay on Doenitz, for example, Lennart Sjöstedt includes a somewhat self-righteous admonition on the dangers of contemporary history in which he contends that proximity in time makes it "generally more difficult to view the subject *objectively*" (my italics) and more likely that the author will "incline sentimentally in the one or another direction." An adequate rejoinder to this contention would transcend the limits of the present review, but for the moment it does not seem unreasonable to point out that Sjöstedt's argument is vulnerable to challenge on several grounds at least.

In conclusion one must necessarily hope that



this volume will not be representative of the Lund series either in terms of the scholarly issues it raises or the more philosophical sense of the historical discipline it projects.

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SHLOMO ARONSON. *Reinhard Heydrich und die Frühgeschichte von Gestapo und SD*. (Studien zur Zeitgeschichte.) Stuttgart: Deutsche Verlags-Anstalt. 1971. Pp. 339. DM 20.

Mr. Aronson's stated objective is to add to our understanding of how the ultimate character of the Nazi state emerged, how certain aspects of the ideology and certain elements of the Nazi movement prevailed, and how the basis of the "final solution" was laid. The vehicles are a short biography of Reinhard Heydrich and a detailed study of the formative years (1931-35) of the organizations he commanded, the Nazi Security Service (SD) and the Gestapo, the political police of the Third Reich.

In fulfilling his objectives Aronson has added refinement and further substantiation to the interpretations of Hans Buchheim and Friedrich Zipfel. His account of the emergence of the SD from the swirl of competing intelligence agencies is a pioneering effort. An important contribution is the thesis that competition between the Gestapo and the SD drove them to propose increasingly radical solutions to the "Jewish problem."

The biography of Heydrich confronts us with another, unheralded objective—a convincing effort to lay to rest the persistent rumor of Heydrich's Jewish ancestry. Aronson's portrayal of Heydrich is satisfying. The man who emerges is a real, but pitiable human being rather than a demonic superman. His role during the period studied is also reduced to more realistic proportions than that usually attributed to him.

The frequently told story of the struggle for control of the police is enhanced by Aronson's analysis of what produced the final product. Traditional German bureaucratic institutions are contrasted with Himmler's innovative SS-police-concentration camp model developed in Bavaria. Both added to the ill-defined and multifaceted character of the final system. The resultant flexibility and ambiguity defeated

Heinrich Himmler's opposition and aided in the transformation of the heterogeneous personnel of the Gestapo and SD into functionaries of mass murder.

Aronson has thoroughly tested the traditional eyewitness versions of SD and Gestapo history, which previous scholars have accepted as the best available information. For this, he interviewed a much wider range of surviving participants and drew upon the core of relevant archival material available to Western scholars.

Overly detailed organizational descriptions and almost forty biographical sketches confuse the narrative. Although the personality profiles were essential to Aronson's characterizations of Gestapo and SD men, much of the detail could have been relegated to appendixes. These characterizations and the explanations of how such men were lured into their future roles are highly plausible. The small elite sample on which they are based nevertheless points to the need for a more thorough quantitative study. Even after this has been done Aronson's account will probably remain unchallenged except on minor details or subtleties of interpretation, barring some very surprising revelations from East European archives.

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JOHN H. BACKER. *Priming the German Economy: American Occupational Policies, 1945-1948*. Durham: Duke University Press. 1971. Pp. 212. \$6.75.

Almost a generation after the event, analysis of U.S. post-World War II occupation policy in Germany survives as at least a minor sport for both Americans and Germans as evidenced by John Gimbel's sober *American Occupation of Germany* (1968) and Caspar Schrenck-Notzing's far-out *Charakterwäsche* (1965). Where *Priming the German Economy* fits into the literature of the occupation is not easy to determine offhand. Although John H. Backer has done substantial research, it is not, strictly speaking, a history even of economic policy; and although Backer worked in the Economics Division, OMGUS, it is also not a memoir. It is, nevertheless, a valuable small book that future

writers on the occupation will do well not to ignore.

In the first three chapters Backer gives sound, if somewhat rambling, observations on U.S. economic policies in general and their execution, particularly the famous JCS 1067, its "disease and unrest" formula, and the level of industry plans. The subsequent chapters (4 through 6) are an extended essay on the U.S. export-import program and the U.S.-British Joint Export-Import Agency. There Backer is on his home ground. As a former official of the export-import program for Bavaria he writes from experience, but he has not neglected the documents, in this case the OMGUS records in the National Archives. The result is as lucid an account of one of the least comprehensible features of the occupation as is likely to be written.

Backer is one of the few writers on the occupation who have successfully resisted the subject's built-in temptations to blanketed, and hence often pointless, negative judgments. From time to time in fact he leans too far the other way, for instance, when he writes that JCS 1067 was "no impediment to the early efforts of the American Military Government toward the reconstruction of the German economy." While it was certainly not a practical impediment, especially since the German economy was already as badly off as it would have been under JCS 1067 stringently construed, it was a psychological impediment in that it created an apparent divergence between the stated U.S. policy and the manner of its execution.

Backer is also, in my opinion, a little too willing to equate good performance with general success. True, both the food and the related export-import programs for the Germans were enlightened and constructive, but the simple fact was that both were largely exercises in futility without currency reform and superfluous after currency reform was finally achieved two or three years late.

EARL F. ZIEMKE

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In this slender volume the contributions of František Palacký to historiography receive their fullest appreciation in English since Francis Count Lützow wrote some seventy years ago. Those readers already acquainted with Joseph F. Zacek's articles on the nineteenth-century historian and public figure will find here more than a convenient compilation, and those not acquainted will be admirably introduced to the pre-eminent personality in the Czech national revival. An updated version of the author's doctoral dissertation, it is more elegantly written than most such endeavors.

Palacký's jousts with Habsburg censors, which prompted him vigorously to defend freedom of scholarly inquiry, exemplified the burdens of the liberal intellectual in pre-March Austria. His devastating rebuttals to German detractors of his monumental *History of the Czech Nation* (1836-67) established a model of combative yet objective scholarship in a national cause. His treatment of fifteenth-century Central Europe shattered hoary myths and elevated Hussitism to European and even transcendent importance. This pioneering role was imposed upon Palacký by the necessities of locating and assembling the pertinent historical sources and of creating upon that foundation the first scientific account and philosophical interpretation of Czech history.

The author views his monograph as "a partial, critical synthesis," the prelude to a full-scale biography that would draw intensively upon archival materials, but the work can stand on its own merits. It is not merely an essay in historiography and biography but also a revealing glance into a pivotal arena of awakening nationalisms. One wishes, however, that Professor Zacek had refrained from repetitively designating almost all citations of Czech scholarship since 1948 as "Marxist" or "Marxist-oriented." He could have made this obvious point more effectively in the preface or the bibliographical essay. And surely it is carrying respect for a dead hero too far when he asserts that Palacký's "broad modern historical program . . . remains the basis of contemporary Czechoslovak historiography." For better and for worse today's Czech historians have incorporated the problems, themes, and methods bequeathed them by Palacký into a different and more relevant perspective; and if Slovak histo-

JOSEPH FREDERICK ZACEK. *Palacký: The Historian as Scholar and Nationalist*. (Studies in European History, Number 5.) The Hague: Mouton. 1970. Pp. xiv, 137. 28 gls.

rians also supposedly follow Palacký, then perhaps the author has interpreted the great historian's "program" so broadly that it acquires well-nigh universal validity.

STANLEY B. WINTERS

Newark College of Engineering

ANTON VANTUCH and L'UDOVÍT HOLOŤÍK, editors. *Der österreichisch-ungarische Ausgleich 1867: Materialien (Referate und Diskussion) der internationalen Konferenz in Bratislava 28.8.-1.9. 1967.* (Slovenská Akadémia Vied, Historický Ústav.) Bratislava: Verlag der Slowakischen Akademie der Wissenschaften. 1971. Pp. 1076. 120 K.

The Habsburg monarchy died in 1918, but as yet no certifiable cause of death has been established. At first death was attributed to malignant nationalism aggravated by the First World War, but further dissection of the corpse cast doubt upon this conclusion. New evidence suggested that nationalism was not causal but part of wider processes of historical change that became compelling in the nineteenth century. This suggests that the insoluble nationality conflicts were themselves only a symptom of a systemic dysfunction brought on by the limited adaptability of the monarchy's sociopolitical structure to these processes. In the period between the two world wars research concentrated mainly on the political, diplomatic, legal, and cultural dimensions of the problem, while after World War II, Marxist-oriented historians in East Central Europe stressed long-run economic and social forces associated with the processes of modernization. The latter analysis was valuable but it remained separate until recent years. As a way of exploring the interconnection between the findings of Marxist and non-Marxist historiography, international meetings of historians were held in Budapest (1964), Bloomington, Indiana (1966), Bratislava (1967), and Vienna (1968)—to name the major ones. Each conference was held on the anniversary of what might be labeled a predisposing event, that is, an event that predisposed the Habsburg monarchy to disintegration. This volume contains the papers of the Bratislava Conference held in the summer of 1967 on the hundredth anniversary of the Compromise of 1867, which created the dual monarchy of Austria-Hungary.

The Bratislava Conference, which was sponsored by the Historical Institute of the Slovakian Academy of Sciences, brought together 140 Marxist and non-Marxist historians from 15 countries. The 42 published papers and 155 pages of discussion contain a wealth of analyses, data, bibliography, and research suggestions. On the whole the papers and discussions of this conference are free from the East-West division of earlier conferences. There are disagreements among both Marxist and non-Marxist historians, and methodological and conceptual differences tend to be of the kind about which historians generally argue. That is, they differ in their predilections for interpretations based on long-run factors as opposed to relativistic ones and the significance of that which happened as opposed to that which might have happened but did not. Progress toward integration of different lines of historical research, mutual jettisoning of outworn ideological baggage, and less apologetic stances by East Central European and Austrian historians—which are all, one hopes, irreversible trends—are attested by concluding remarks of several of the participants. For example, Jiří Kořálka, a Czech, remarks that "black-white" historical interpretations of the Habsburg monarchy have declined greatly, and he calls upon his fellow Czech historians to concern themselves with political and constitutional history more than they have in the past (p. 1041). Hans Lentze, an Austrian, calls on his compatriots to pay more attention to social and economic history (p. 1038).

The forty-two papers are grouped together under four headings according to their content. In introductory papers Josef Polišený (Prague) and Fran Zwitter (Ljubljana) maintain that the revolutions of 1848-49, which represent the beginning of the end for the Habsburg monarchy, are far more important than the Compromise of 1867, which they see as a last and negative echo of the revolution. R. A. Kann (Rutgers), on the other hand, sees little of the ideas or plans of 1848-49 in the Compromise of 1867, which he asserts was never intended to solve the nationalities problem but was meant to preserve the Habsburg monarchy and its great power position with the absolute minimum of social change necessary. Several of the participants took issue with Kann's conclu-

sion that the compromise ought to be judged "only according to the purpose of those who created it" (p. 25).

The first group of papers examines Austro-Hungarian foreign policy from 1866 to 1870, the implications of the Compromise of 1867 for the Eastern Question, the reaction of Austria-Hungary's small southeastern neighbors, as well as the attitudes of the respective foreign offices and major newspapers of the great powers toward the compromise. In general the essays show the connection between foreign policy and domestic structure and how, for different reasons, the reorganization brought about by the compromise accorded with the interests of all of the great powers.

The second group of papers deals with social, economic, and constitutional aspects of the compromise. Walter Goldinger (Vienna) and Hans Mommsen (Heidelberg) both stress the continuation of absolutism as a logical extension of the compromise. Mommsen attributes the insolubility of the nationalities question in Austria to the quasi-constitutional and unrepresentative parliamentary system established by the constitution of December 1867, with the agreement of Austrian liberals. In separate essays Peter Hának and György Ránki (Budapest) utilize quantitative data and the concept of economic backwardness to maintain that the Compromise of 1867 was a variant of the bourgeois-capitalist transformation of state and society, which, though not the best, nevertheless was conducive to economic growth. Both historians reject the idea of a direct connection between economic growth and social and national development, a position that brought them into sharp conflict with the Romanian historian, Miron Constantinescu. George Macesich (Georgia) attempts, with the help of modern market theory (but with no empirical economic data), to confirm Oscar Jaszi's judgment that the compromise prevented the growth of an integrated economic unit. Though Macesich is less persuasive than Hának and Ránki, all three demonstrate how much can be learned from the application of modern economic analysis.

The third group of papers treats the response of the non-German and non-Magyar nationalities to the compromise and includes papers on small nationalities, such as Slovenes

and Slovaks, whose efforts to protect their national identities have been ignored until very recently. Erwin Melichar (Vienna) and Július Mésároš (Bratislava) get down to the nitty-gritty of the nationalities question. Melichar, a specialist in constitutional and administrative law, uses the decisions of the Austrian *Reichsgericht* from 1869 to 1918 to examine the implementation of Article 19 of the Austrian Constitution of 1867, which guaranteed nationality rights. The attempts of the court to specify what constituted a denial of nationality rights reveal the strengths and weaknesses of Article 19 and of the *Reichsgericht* itself. In his article, which is studded with demographic tables, Mésároš analyzes the nationalities question at its most sensitive point—the growth and diffusion of educational institutions. In an innovative comparative article, Peter Sugar (Washington) compares and contrasts the reactions of the Croats, Romanians, and Slovaks in the period from 1867 to 1875. He shows that the Croats, with an "independent political history," were far better off in the cultural manifestations of their lives than the Romanians and the Slovaks, neither of whom had such a history. Paradoxically, however, the Croats, despite having "a political history," were not able to wrest much more of a substantive political nature from the Magyars than were the Slovaks and Romanians.

The papers in the fourth section concern the problem of federalism in Austria-Hungary. Hans Lentze (Vienna) concludes that a real federalization of Austria would not have been possible without depriving the Crown, the ruling classes, and the German-directed bureaucracy of their power, and that this would not have been possible without a struggle. Ferenc Pecze (Budapest) develops the thesis that the generally held view that Czech trialistic aspirations in the 1870s were frustrated by the Magyars is an oversimplification. In a paper that sparked a lively debate, Joachim Remak (Santa Barbara) argues that the monarchy in 1914 was a viable state that might have survived and transformed itself from within had it not been "killed" by bad diplomacy and the war. Set against the papers of, among others, Lentze, Pecze, Mommsen, Goldinger, and Hának, as well as the critical remarks of several discussants, Remak's argument is unconvincing.

If there is disagreement among the participants on particulars, there is some agreement on larger issues. Until the late nineteenth century the Habsburg monarchy provided a framework for economic growth, expansion of the social structure, and cultural growth that benefited all of East Central Europe. After that time, under conditions created by forces that the monarchy helped to generate and organize, in part by the Compromise of 1867, the traditional sociopolitical foundations of the Habsburg monarchy and the compromise itself became obstacles to a more equitable diffusion of economic growth as well as to fully differentiated modern social structures. Most of the participants agree that the dissolution of the monarchy was inevitable, not according to any abstract notion of historical inevitability, but only within the context of comparative and systematic studies of European social-historical development. In this context the question of the inevitability of the monarchy's dissolution is shorn of its recriminatory overtones.

We are indebted to the editors and to the Slovakian Academy of Sciences for making the papers of the conference available to a larger readership, by which I mean not only historians of the Habsburg monarchy and East Central Europe, but all historians and social scientists interested in the process of "national awakening" among small nationalities, the transition from economically underdeveloped to economically developed societies, and the problem of coordinating national societies with international or transnational organizations. All of these are now global problems, for the investigation of which these papers provide comparative instances. The value of the volume is enhanced by a list of newspapers and periodicals mentioned in the papers and discussions and by an index of names of historical personages and authors. In view of its merits, the large number of typographical errors and typographically garbled sentences may be regarded as simply annoying. The editors and the printers faced a formidable task in producing a book of essays in four languages: German (21), English (10), French (8), and Russian (3).

Robert Musil observed that "the mysteries of dualism are at least as difficult to understand as those of the Trinity." The essays in this volume leave the mysteries of the Trinity un-

touched, but they do illumine those of the Compromise of 1867.

SOLOMON WANK

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YVES COLLART. *Le parti socialiste suisse et l'Internationale, 1914-1915: De l'Union nationale à Zimmerwald*. (Publications de l'Institut Universitaire de Hautes Études Internationales, Number 49.) Geneva: [the Institut;] distrib. by Librairie Droz, Geneva. 1969. Pp. xi, 373. 60 fr. S.

Collart's study asks why the truce of 1914 was broken only a year later, especially by people who had supported it with full conviction in 1914. He carefully analyzes all previous research theses, checks them on the basis of any and all available sources, rejects previous theses, and carefully justifies his reasons for new interpretations. The most important point is the meaning that Collart attributes to the Conference of Lugano, which was neglected in previous research. In this conference he perceives the indispensable connection for the transition from the truce to Zimmerwald. Collart's inquiry proceeds in three stages: an analysis of the truce, an assessment of the motives and the consequences of the Conference of Lugano, and the question why the change of climate took place in 1915, manifesting itself in the Zimmerwald Conference. The author attempts to show to what extent such decisions are determined by historical constants (constants of national and international development) and where and why certain variables are decisive for the choice of a specific course.

According to Collart the *salto mortale* of the Swiss truce of 1914 differs from that executed by the Social Democrats of the belligerent countries. It was justified more rationally than emotionally. It was dictated by the concern for the food supplies of a country largely cut off from international supply lines. With respect to Lugano Collart considers the duty of a small, neutral state, spared by the war, to work for peace and restitution of the International to have been the main motive for the turnabout that led from the truce to Zimmerwald. Collart is correct in concentrating not merely on the fact of this change but in inquiring after the tortuous ways and the various tempi by which it took place. The author is undoubt-

edly justified in selecting Robert Grimm as the key figure involved in this change of climate and in attempting to throw light on his career, hitherto not at all well known. Collart nails Grimm on the formula that the struggle for international peace is identical with forcing the national class struggle. I rather doubt that the change can be explained in so monocausal a fashion. Insofar as the truce was motivated by the material safeguards of the Swiss workers, it must have lost its *raison d'être* to the same degree that the working masses were pauperized and proletarianized by the unsolved distribution problem.

E. GRUNER

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BRIAN PULLAN. *Rich and Poor in Renaissance Venice: The Social Institutions of a Catholic State, to 1620*. Cambridge, Mass.: Harvard University Press. 1971. Pp. x, 689. \$13.50.

Drawing upon a staggering amount of archival material Pullan organizes his book around three institutions that dealt with poor relief in Renaissance Venice—lay religious societies, hospitals, and moneylending. First he traces the development of the Scuole Grandi from their thirteenth-century beginnings as flagellant societies to their prestigious status as quasi-public charitable institutions. By the fourteenth century each Scuola consisted of two orders, the poor brethren who received charity and performed the devotional acts of the society, including ceremonial scourging, and the rich nonflagellating brothers who dispensed charity and took part in the pageantry of public festivals. Typically Venetian, the Scuole offered something for almost everyone—religious merit and social prestige for the rich non-nobles who ran them, charity for the respectable poor who did the work, and, as always, support for the state, for the Scuole loaned money to the treasury and provided rowers for the galleys. Only the truly unfortunate were overlooked—prostitutes, beggars, vagrants, and needy foreigners who were, literally, outcasts, for they had no access to the all-important charitable societies. Hospital care, the subject of Pullan's second section, was likewise unavailable to them, since the responsibility for institutional care was left to private societies, which natu-

rally favored their own members. By the late fifteenth century, however, the swelling numbers of war refugees, syphilitics, and sufferers of plague and famine were forcing the attention of state officials and high churchmen. Pressed by her enemies Venice was anxious to earn divine protection by charitable acts, while Franciscan preachers and Christian humanists alike were bringing the claims of the poor and the unfortunate to public consideration. Besides, there was a perceived threat of public disorder if the poor were not regulated and provided for. On the whole the effort was to direct the poor into socially useful occupations. This led to a distinction between the deserving poor and the incorrigibly idle. In some ways the new poor laws of the sixteenth century replaced the older benign neglect with harsh treatment—Tawney's "new medicine for poverty" was not exclusively a Puritan discovery.

In the third section Pullan studies the Venetian experience with that typically late medieval remedy for poverty, the provision of cheap credit. While most of her neighbors were establishing public loan funds, the famous *Monti di Pietà*, Venice continued to favor another common medieval expedient, the licensing of Jewish moneylenders. Jews could also be tithed, taxed, and intermittently shaken down for contributions to the fisc. This inevitably caused them to pass the cost of doing business on to the consumer in the form of high interest rates; but after 1573, apparently to celebrate the victory of Lepanto, the Venetians hit upon the expedient of forcing the Jews to maintain a non-profit loan bank for needy borrowers, a kind of Jewish *Monte di Pietà*. This was not a sign of greater Venetian sympathy for the poor but rather that the state's relentless squeezing of the Jews had so dried up Jewish capital that it was no longer a significant factor to the treasury, so what was left could be diverted to loans for the poor.

Pullan's book suffers from lack of focus and excessive detail but it contributes much new material not only for the subject of poor relief but also for the history of Venetian piety, social and political organization, and the treatment of the Jews. With respect to its place in the current lively discussion of the origins of early modern philanthropy, it comes down on the side of continuity. Pullan shows that in

Venice many sixteenth-century "innovations" were really efforts to revive earlier practices, that the tendency to question the spiritual value of voluntary poverty and to replace it with the new social and moral value of work was not limited to Protestant societies nor was it entirely new in the sixteenth century, and that social demands as well as evangelical doctrine could explain the formulation of a new work ethic. At the same time Pullan sees no thoroughly secular, social approach to poverty emerging among the Venetians, who continued to regard charity as a spiritual act that earned divine favor both for individuals and the state. Preservation of the existing order was ever the foremost consideration, into which the new spiritual and social values were absorbed. Was Venice unique in this? Pullan is willing to allow that it may have been, but one school of thought suggests that the subordination of welfare policy to the interests of the ruling class and "the national interest" has been and continues to be the dominant pattern in Western society.

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VITTOR IVO COMPARATO. *Giuseppe Valletta: Un intellettuale napoletano della fine del Seicento*. Naples: Istituto Italiano per gli Studi Storici. 1970. Pp. 360. L. 5,000.

The seventeenth century in Italy between the condemnation of Galileo and the arrival of Vico has been a *mare sconosciuto* until recent studies of Badaloni, Mastellone, and others have shown that a good deal was going on, especially in Naples. Here a group of lawyers, including Giuseppe Valletta (1636-1714), keenly debated the issues of the day. The son of a poor tailor, Valletta became a wealthy lawyer and merchant with a keen mind and great erudition, which he applied in a number of treatises.

Comparato successfully argues that at the center of Valletta's thought was a conception of natural law derived primarily from Grotius and the late sixteenth-century French jurists, above all Bodin. In all of his thought Valletta was a man of European culture who transcended purely Italian concerns. In his politi-

cal thought Valletta moved beyond traditional Italian theory, still focused on how to acquire and conserve states, to the European base of seeking the balance between the sovereign's authority and the subject's rights. Valletta argued against a monetary devaluation proposed by the Spanish government on the principle that the economy was founded on natural laws of exchange and should be allowed to function freely. In a critique of the Inquisition Valletta argued that it violated procedurally the natural right of defense and betrayed historically its roots. Sarpi had argued that the state rather than the Church should conduct it; Valletta took the critique a step further by balancing the principle of free inquiry against the duty of the public authority to maintain unity. This led logically to Valletta's defense of the right of philosophical inquiry, with its distinction between theological truth based on authority and philosophical truth founded on investigation of nature. In summary, Valletta appears as a transitional figure, anchored in Renaissance legal humanism and historical erudition while only a step away from the Enlightenment.

Comparato has combed the sources for Valletta's printed and manuscript sources, printing two of the latter in appendixes. In each chapter Comparato sets the stage by copious reference to other contemporary treatises, and he finishes by explaining the sources that went into Valletta's analysis. This is a useful and competent study in an area increasingly valued as important in the history of European thought.

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DENIS MACK SMITH. *Victor Emanuel, Cavour, and the Risorgimento*. New York: Oxford University Press. 1971. Pp. xv, 381. \$22.50.

The history of the Risorgimento has never been the same since Denis Mack Smith began revisiting it about twenty years ago. The Risorgimento was still in the midst of the neo-Marxist revisionist storm raised by the publication of Antonio Gramsci's *Quaderni del Carcere* (1947) when Mack Smith's disturbing monographic study in political conflict entitled *Cavour and Garibaldi, 1860* (1954) and then, four years later, his general history of modern Italy

brought a fresh whirlwind of historiographical controversy. Though it would be far from irrelevant to write a full critical review of Mack Smith's present book on Victor Emmanuel and Cavour, unfortunately not even a brief summary of the highlights of that controversy can be attempted here.

Following his fruitful detour into Sicilian regional history, which I feel relatively certain was both cause and effect of his immersion in Garibaldi's liberation movement of the island in 1860, Mack Smith has now returned to the high road of Italian national history running from the dead end of the revolutions of 1848-49 to the Piedmontese march on Rome in 1870. On the basis of part of the older documentation enlivened by research in hitherto unexplored or unavailable source materials from Italian, British, French, and Austrian archival collections, Mack Smith presents his findings and views, old and new, concerning the two culminating decades of the Risorgimento. This time the Italian national revolution is analyzed from a variety of aspects that, on the whole, are subsumed under "the personal contribution which two very different individuals [Victor Emmanuel and Cavour]—the two most politically powerful men of the Risorgimento—made to the way that Italy became a nation" (p. xi). Had Mack Smith been fortunate, as he had long hoped, in having access to the archives of the former Italian royal house that now lie buried at Cascais in Portugal where they were mysteriously transferred after Italy became a republic in 1946, he would undoubtedly have written a much-needed critical biography of Victor Emmanuel II. Whether or not one agrees with one of Mack Smith's major historical assumptions (and, for a variety of reasons, I do only in part), for him Victor Emmanuel ranks higher in influence as well as in position than Garibaldi and Mazzini and merely on a par with, not below, Cavour as a maker of united Italy. The impact of Victor Emmanuel's personal and dynastic legacy, almost wholly negative, persisted beyond the fall of fascism. The first king of Italy is thus studied by Mack Smith as a primal molder of policies, practices, and institutions (chiefly a powerful and "irresponsible" executive) that left an inescapable impress on subsequent national developments and that were almost deterministi-

cally crystallized into a royal "residual power," which allowed Victor Emmanuel's son and grandson to intervene decisively at crucial moments in later Italian history in support of such "strong men" as Crispi, Salandra, Mussolini, and Marshal Badoglio.

After an introductory overview of Risorgimento history from 1840 to 1870 and through fourteen self-contained but more or less interrelated essays, Mack Smith engages in a sort of clinical dissection of various aspects of his fundamental thesis on the centrality of Victor Emmanuel's negative "contribution" to the making, structuring, and activities of the Italian unitary state. In a series of masterly chapters Mack Smith re-envelops the diplomacy of the Risorgimento within the contradictions of its multiple, or at least dualistic, political leadership—Villafranca in July 1859 and Marsala in May 1860 proved only the most dramatic revelations of the nemesis that hounded the unresolved conflict between Victor Emmanuel and Cavour—and reveals that the sources of the erosive disaccord lay infinitely less in the much publicized clash of strong personalities than in recurring confrontations between the constitutional prerogatives of the king and the institutional and parliamentary responsibilities of the prime minister. For, in essence, they were engaged in the Italian version of the ancient struggle between monarchy and representative government, old dynastic ambition and emerging national interest. Thus Mack Smith subtly succeeds in stripping down some of the most resistant hagiographic veils with which a prominent sector of "traditional" Italian historiography has tended to cover up, ignore, minimize, or obviate with almost equal generosity the personal flaws and the grave, sometimes tragic, official errors of the *Re Galantuomo*.

In this volume practically none of the major historical viewpoints on the Risorgimento that have come to be associated with Mack Smith is either substantively or radically changed. Nevertheless, the total effect of this fascinating prismatic study of the politics of the Risorgimento has proved to be, at least for one reader, much more positive, paradoxically even in its negative emphases, than its author's general history of modern Italy. Whatever his larger intellectual and cultural interests, there can hardly be any question that Mack Smith is at



his unequaled best with monographic political history, even if, as may perhaps be unavoidable for all professionals, he sometimes appears to become too enchanted and, therefore, entrapped by the very sources he can so deftly exploit. Yet even this judgment needs drastic modification, at least in the light of his superb two-volume history of modern Sicily, a miniature model of which is reproduced in the present volume with the publication, I believe for the first time in English, of his 1950 article in Italian on the peasants' revolt in Sicily in 1860. Interestingly enough, it is in this essay, which at first sight does not seem to be directly connected with the main themes of a volume on Victor Emmanuel and Cavour, that Mack Smith reiterates the core of his views on the Risorgimento. For him, the Italian national revolution is still essentially a multiple civil war, which, appropriated as it was by the forces of law and order represented by the Savoyard monarchy and the liberal middle classes, excluded the masses of the Italian people, urban and peasant alike, from "the fruits of their own success" in bringing down the institutional, social, and moral pillars of the various *anciens régimes* in Italy.

Mack Smith quite rightly believes that "there is no necessary reason why truth should be beautiful or simple" (p. 176). The studies in this volume make it amply evident that the "truth" about Victor Emmanuel II is almost absolutely harsher than some "traditional" Italian historians ever acknowledged—was the king perhaps "half mad," as Sir James Hudson reported from Turin to the Foreign Office at a moment of exasperation and frustration in 1862? On the other hand, when the truth about Cavour's diplomatic, political, and constitutional thought and action is pitted, as it essentially is in this volume, against the king's anachronistic and often dangerous and irresponsible activities, it shows something finer, richer, more human and historically fertile than that almost demonic face of the Machiavellian "fox" that the relatively isolated confrontation between Cavour and Garibaldi in 1860 had tended to reveal. In this volume Mack Smith has neither simplified nor beautified historical truth as he sees it. Nevertheless, by placing the "truth" within a larger context of historiographical perspectives he has tended to render

his reinterpretation of the Risorgimento no less original and brilliant, but certainly mellowed and perhaps wiser than his almost single-mindedly iconoclastic earlier work in modern Italian history.

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GIORGIO SPINI. *L'Evangelo e il berretto frigio: Storia della Chiesa Cristiana Libera in Italia, 1870-1904*. (Storia del movimento evangelico in Italia, Number 1.) Turin: Editrice Claudiana. [1971.] Pp. 265. Cloth L. 3,500, paper L. 2,800.

This volume dwells upon an often forgotten aspect of the Risorgimento—the Protestant awakening that accompanied the political reorganization. Specifically, it examines the development of the Free Christian Church in Italy from its appearance in 1870 to its dissolution in 1904. Identified with the Left and committed to the independence of each congregation, its ministers, as Spini shows, found themselves not only at odds with the Catholics but with the Waldenses as well, and hence determined to march their separate way.

The Free Church, according to Spini, received its inspiration from two sources: the Anglo-Saxon religious revival and the Risorgimento. Its fatal flaw was the inability to reconcile the two. Superficially the problem seemed resolved, for the foreign sponsors of the institution joined nationals in pressing for a close association with the state. The church achieved its respectability by sacrificing its vitality, for its preoccupation to please its foreign paymasters and the political establishment repelled its most devout members. Thus, though the church could boast of some two thousand communicants in the 1880s, a doubling of its membership since unification, it was no longer free or Italian.

The specialist will appreciate the detailed descriptions of the thirty communities in the church, the comprehensive coverage of the activities of their pastors, the reports of their general assemblies, and the analysis of their abortive attempt to merge with the Waldensian Church. These, when combined with the writings of a number of church members, create the picture of a dedicated group of men working against clerical pressure and widespread ignorance to effect a religious transformation.

Unfortunately Spini relies almost exclusively upon evangelical sources, and thus has produced a restricted and one-sided study of the Free Church, which after 1890 came to be called the Italian Evangelical Church. We are given but a glimpse of the manner in which the political elite, the various classes, and the vast majority of Italians reacted to that institution. Little is said of the means by which this church proposed to draw the masses away from the traditional faith and into its own ranks. What is presented, instead, is a survey of the maneuvers of its leaders and a catalog of the activities of its various congregations.

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*Atti del XLIV Congresso di storia del Risorgimento italiano (Trieste, 31 ottobre-4 novembre 1968) (La fine della prima guerra mondiale e i problemi relativi).* (Istituto per la Storia del Risorgimento Italiano. Biblioteca scientifica. Atti dei Congressi, Volume 12.) Rome: the Istituto. 1970. Pp. 269.

How much valuable work of able scholars is lost when it appears in volumes with bland serial titles such as this? Historians of Italy are doubtless attentive to publications of the Istituto per la Storia del Risorgimento, but the proceedings of its Forty-fourth Congress have a much broader appeal and suggest once more the crisis of information retrieval in this age of proliferating knowledge.

These seven papers were presented in Trieste in the autumn of 1968 when the Istituto commemorated the sesquicentennial of that Adriatic city's unification with Italy. The theme of the meetings, "La fine della prima guerra mondiale e i problemi relativi," was quite appropriate and opportune, but it becomes almost antiseptic as the subtitle of such a rich collection of information and observations. Hence one initially has the impression that the congress undertook a panoramic consideration of European events in 1918 and gave the expected emphasis to Italian affairs at the close of the war. But closer examination shows that the participants produced an excellent résumé of numerous aspects of World War I that was based on recent research. The résumé

has also taken into account the significance of the forgotten, maligned, or misunderstood Italian front.

As in any such collection, the essays vary in quality, but unlike most such assemblages, each of these works has something of unusual value. Aldo Garosci, in his "Mutazioni di equilibri e ideali politici nel corso della prima guerra mondiale," has presented a truly stimulating, even seminal work, which treats anew the causes of the conflict and the alteration or adaptation of each belligerent's war aims. He has drawn most imaginatively upon Fritz Fischer and Arno Mayer, as well as Croce, and goes beyond all. Yet potentially more controversial are the assertions by the French historian Henry Contamine, who asserts that the events of the Italian front shortened the war and perhaps held the key to its outcome. His evidence and arguments will not be easily refuted.

Some vital and easily neglected aspects of the dissolution of the Austro-Hungarian Empire (for example, issues deriving from transmission of administrative responsibilities) are treated by Fritz Fellner, whose remarks spurred one listener to warn against excessive criticism of Austrian relations with the successor states of the Empire.

Luigi De Rosa has provided a sweeping view of the tragic and even cataclysmic economic consequences of the Great War. If he may be faulted for giving less attention to Russia, England, and France, De Rosa has nevertheless provided unusual information on such countries as Finland, Spain, Bulgaria, and Sweden. His conclusion on the Continental situation is most candid: Europe should have given more attention to helping itself by creating its own markets after the war. Manlio Udina, a jurist, has undertaken a similar but less penetrating analysis of the drastically altered and wrenched situation of international law and organizations in the postwar world.

Leo Valiani's lively essay on the stormy course of Italian domestic politics and foreign policy in 1918 demonstrates the fragility and uncertainty of the country's commitment to the Treaty of London long before the controversy over that pact erupted and threatened Allied solidarity at Versailles. This account ought well to be considered by any future historians of the peace conference, especially those who

would reappraise America's policies toward Italy in 1919.

The late Carlo Schiffrer, in "L'attesa di Trieste," evokes the enthusiasm of Trieste on the eve of its capture by the Italian army. Still, he has not overlooked the history of the city with its Slavic minority, its vital links to its non-Italian economic hinterland, and its romantic ties to the Risorgimento, questions that have continued to make news long since 1918.

Not long ago Jean-Baptiste Duroselle remarked that a new generation of works on World War I, with a different perspective, could soon be expected. Here is some evidence of his accuracy; and it is therefore to be hoped that at least one, if not more, of these essays may become the basis for new monographs.

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STANISŁAW GRODZISKI. *Historia ustroju społeczno politycznego Galicji, 1772-1848* [A History of the Social and Political Organization of Galicia, 1772-1848]. (Polska Akademia Nauk—Oddział w Krakowie. Prace Komisji Nauk Historycznych, Number 28.) Cracow: Zakład Narodowy im. Ossolińskich, Wydawnictwo Polskiej Akademii Nauk. 1971. Pp. 303. Zł. 60.

This book on Galicia pertains to Poland and to Austria, which acquired Galicia in the first and third partitions of Poland. It provides a thorough study of the historical processes in that province with a pluralistic society and diverse nationalities, religions, and economic conditions. The author, being a historian of law (a group of scholars particularly prominent in Europe) concentrates on the legal aspect of the subject.

A detailed survey of sources of law and a geographical description of the territory open this study. Then the author analyzes social relations, including the nobility; cities and burghers and their professional and occupational problems; the clergy as a social group; and peasants and the attempted reforms at their emancipation, their self-government, and their rebellion of 1846. Colonization and Germanization of Galicia and the place and fate of Jews complement this part of the book. The next part brings forth a description of religious relations followed by an analysis of political and administrative organization of Galicia in

the framework of the Austrian monarchy and empire. The organization and functioning of the judicial system is a new major part of the book. The fiscal order and problems, taxation, and other economic matters are treated in the next chapter. Separate consideration is given to the organization of armed forces. One chapter is entirely devoted to an analysis of education and political thought in Galicia. The study is ended with concise conclusions and is supplied with a summary in German. All problems examined in this monograph are treated with respect to their historical development in the period from 1772 until 1848.

The study is firmly set in a broad source basis. The author fully uses archival materials as well as broad secondary sources. A fresh insight is often provided by the memoirs of contemporaries. This solid scholarly apparatus is reflected in numerous pertinent references.

This is the most up-to-date study of the problem. Together with K. Grzybowski's *Galicja 1848-1914* (1959), this book completes our knowledge of that part of Poland in the period of partitions until World War I. It also contributes to a better knowledge of the Austrian Empire as well as to a deeper understanding of European history in the period of expansion of some states and in the areas of struggle of conquered nations for autonomy and their own organized way of life.

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LOUIS FITZGIBBON. *Katyn*. Introduction by CONSTANTINE FITZGIBBON. New York: Charles Scribner's Sons. 1971. Pp. 285. \$10.00.

During the invasion of Poland in 1939 the Russians took prisoner about fifteen thousand Polish officers and placed them in the prisoner of war camps at Kozielsk, Starobielsk, and Ostashkov. After the spring of 1940 all but a few hundred of these officers disappeared and nothing was heard of them. More than a year later, after the German invasion of the Soviet Union, the Polish exile government in London concluded an agreement with Moscow that provided for an "amnesty" for the Poles in the Soviet Union and for the formation of a Polish army there. When most of the Polish officers

who had fallen into Soviet hands in 1939 failed to report to the Polish army, the Poles, naturally, inquired about them only to be told by the Soviet authorities that they knew nothing of their whereabouts. Then, on April 13, 1943, the Germans announced that they found the bodies of some forty-three hundred Polish officers buried in Katyn wood near Smolensk. Each man had been shot in the back of the head. The Germans at once accused their Soviet enemy of perpetrating this monstrous crime. The Russians immediately counter-charged that the Germans had done it. When the Polish government asked the International Red Cross to investigate the Katyn massacre, Moscow broke diplomatic relations with that government.

Louis Fitzgibbon states that he has written the book to "clear once and for all the obscurity which has shrouded for over thirty years the worst crime against prisoners-of-war ever committed, and perhaps the worst single unpunished crime in history." He has, to be sure, uncovered no new evidence, and it is doubtful that his book will have a greater impact than the books on Katyn published previously. There has been little doubt from the beginning that the Soviets perpetrated the Katyn crime. Nonetheless, this horrible deed needs to be exposed again and again. The book consists mostly of extensive quotations from the testimonies of the former Polish prisoners of war and other documents. It also contains twenty pages of most gruesome photographs and the list of the 4,143 victims identified at Katyn. This is a thorough and well-written book.

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FERENC A. VÁLI. *Bridge across the Bosphorus: The Foreign Policy of Turkey*. Baltimore: Johns Hopkins Press. 1971. Pp. xiv, 410. \$12.50.

Váli's thesis is that modern Turkey pursues three fundamental goals: national security, economic development, and "Europeanization." Policies adopted in their pursuit have changed, but the goals (of which Váli approves) remain unaltered since Atatürk.

This analysis permits Váli to applaud much of current Turkish foreign policy: cordial but

restrained relations with Russia, close but dignified relations with the United States, NATO membership, Common Market affiliation, limited pro-Arab support against Israel, and advocacy of peace in the Middle East. Váli supports the "magnificent scheme" to bridge the Bosphorus, to him symbolic of Turkey's third goal, to be assimilated into European civilization. Váli's research in these areas seems competent, informative, and well documented, if marred by repetition.

Váli is weaker in his attempt to provide "the proper historical setting"—fourteen pages for six hundred years of Ottoman foreign policy. Such brief coverage results from his belief that the Ottoman legacy is minimal. Ottoman policies were "hesitating, haphazard, and even irrational," dependent on "concessions, 'appeasement,' or humiliating surrender," whereas those of modern Turkey supposedly represent a near-total discontinuity. Where Váli has provided additional Ottoman historical background the results are commendable, as are his discussions of Soviet-Turkish relations and of the Cyprus issue. Where he has not, as in his sketch of economic development, his conclusions go astray. Two examples: Ottoman leaders purportedly attempted to develop military technology while ignoring overall economic and industrial development. In actuality low import duties imposed upon the Porte in 1838 frustrated an extensive Ottoman effort to initiate its own industrial revolution in the 1840s. Economic development was thereafter impossible until Turkey regained control of tariff rates in 1929. Second, in attributing the origins of *étatisme* exclusively to Soviet models Váli ignores traditional Ottoman state workshops and heavy industry, including shipyards, foundries, smelters and mines, and several state factories that the Republic inherited. Similarly, by arguing that Turkey's current mixed economy is Western he again slights Ottoman precedents.

Two other disappointing features deserve comment. First, the index is an economy-model list of proper nouns capitalized in the text. One looks in vain for general references to agriculture, education, industry, political parties, trade, and so on. Second, the one map is insufficient—it includes no roads, railroads, population symbols, foreign military bases, or former political boundaries, and but one river.

Important regional and place names referred to in the text are omitted. These drawbacks, while annoying, are not critical. Váli has produced a valuable work.

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V. A. PETROV. *Ocherki po istorii revoliutsionnogo dvizheniia v russkoi armii v 1905 g.* [Essays on the History of the Revolutionary Movement in the Russian Army in 1905]. (Akademiia Nauk SSSR, Leningradskoe Otdelenie, Instituta Istorii.) Leningrad: Izdatel'stvo "Nauka." 1964. Pp. 426.

The modern period has never attracted the best Soviet historians, and in this arid field no subject has suffered more from lack of standards than the history of the army. Trained historians leave the subject to military men who regard themselves as historians. As a consequence, with the major exception of P. A. Zaionchkovskii's work on the military reforms of the 1860s and 1870s, there are no worthwhile Soviet studies on any aspect of the history of the Russian army.

Under the circumstances one opens every new book on the army with restrained hope. Petrov's book, however, disappoints even the most modest expectations. Let us take for granted the obligatory distortions (for example, the Mensheviks did nothing but undermine the revolutionary work of the Bolsheviks); even so, Petrov's inadequacy as a historian is distressing. He has no sense of what is important and what is trivial; he does not connect his little facts with larger issues; instead of constructing well-supported generalizations he paraphrases documents. Worst of all he fails to ask those questions that would make his collection of facts meaningful: did the revolutionary movement among the soldiers contribute to Russian defeat, what kind of soldiers were likely to participate in revolutionary action, how widespread was revolutionary sentiment, and why did the revolutionary movement ultimately collapse? The description of "revolutionary acts"—surely a vague concept—in regiment after regiment makes excruciatingly boring reading (for hundreds of pages).

There is, however, one fascinating chapter in the book that alone justifies the investment of

two rubles and five kopeks. In great detail Petrov paraphrases eighty-four petitions prepared by soldier groups. We have so few sources on the basis of which the world view of the Russian peasant (or in this case, soldier) could be reconstructed that this contribution is very valuable. The soldiers' demands were varied: some groups protested against being used as police in putting down disturbances, while others merely wanted to receive extra pay for this duty. Some regiments wanted libraries, while others asked for free soap or higher salaries for army musicians. Reading these petitions one inevitably gains sympathy for the Russian soldier, living in terrible misery, receiving forty-five kopeks every two months, constantly humiliated and abused by his officers. One catches a glimpse of the faces of those peasants and soldiers who twelve years later would bring down the Imperial regime.

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#### NEAR EAST

RICHARD G. HOVANNISIAN. *The Republic of Armenia. Volume 1, The First Year, 1918-1919.* (Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1971. Pp. xxiii, 547. \$15.00.

The present work is the sequel to Professor Hovannisian's previous study, *Armenia on the Road to Independence, 1918* (1967). Heavily documenting his book, the author presents in this the first of a projected three-volume analysis a detailed description of the trials and tribulations of the ill-fated Armenian Republic's first year (1918-19). Not only does he cover the republic's many domestic crises, but he also gives considerable treatment of its foreign relations. One can only sympathize with the enormous internal problems—lack of food and transport, disease and malnutrition, destruction and poverty on every hand—facing the republic's provisional government. The author also gives much attention to the republic's tortuous foreign relations, which only add to the complexities of its internal problems. He explores in considerable depth the republic's relations with Georgia and Azerbaidzhan, its Transcau-

casian neighbors, with the pro-Muslim British forces, with the White Russian troops of General Anton Denikin, and with the hostile nationalist Turkish forces of Mustafa Kemal. Attention is given the efforts of the republic's delegation to the Paris Peace Conference, where, in conjunction with the delegation representing the Turkish Armenians, it petitioned the powers to create an integrated Armenian state consisting of the Armenian Republic, located in Russian Armenia, and Turkish Armenia, located in the Ottoman Empire. The student of the Armenian mandate question and of the endeavors of American relief workers will also find ample space devoted to their particular interests.

No filiopietistic writing pervades this study. The Armenophile will, indeed, find the author's narrative both penetrating and objective as he tries to unfold the story of the republic's efforts to survive the first crucial year. In fact, one is at times inclined to feel the author is overly critical of the republic's officials. Presented in a chronological narrative, the author, in what will undoubtedly become the definitive study of the Armenian Republic, seeks to explore and evaluate the tragic turn of events that made independence for Armenia little more than a pipe dream.

Thoroughly researched on a multiarchival scale, this study is based on the author's search in primary sources in Russian, Turkish, German, English, and Armenian archival material. This book makes it apparent that the diplomatic historian must seek answers to his questions in more than one archival source in order to make a completely objective analysis.

While the author has accomplished his purpose of presenting "a detailed, if not involved, history of the Republic of Armenia," one is inclined to wonder whether a shorter treatment would not have sufficed to cover that ill-fated republic's first year of existence. In any event, this work will answer many questions for the scholar whose interest lies in Russian and Middle Eastern studies.

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MEHDI HERAVI. *Iranian-American Diplomacy*. Brooklyn: Theo. Gaus' Sons. 1969. Pp. 161. \$6.50.

MICHAEL KAHL SHEEHAN. *Iran: The Impact of United States Interests and Policies 1941-1954*. Brooklyn: Theo. Gaus' Sons. 1968. Pp. xiv, 88. \$5.00.

Dr. Heravi's book, while providing an adequate general survey of United States relations with Iran, is both too brief and too lacking in guiding generalizations to be recommended with enthusiasm. It lacks an overall view of the changing nature of United States motives in its relations with Iran beyond speaking of the period before World War II as one of isolationism. The book opens with a discussion of missionary activity in Iran, which states misleadingly that "the most drastic change in the Iranian mind and attitude was brought about by the establishment of the first American missionary school in 1835. . . . The first formal school for girls, which was established by the American missionaries, also greatly improved the social standards of Iran."

The book lacks analytical statements of the bases of American policies over the long period it discusses. Many of the generalizations it includes are taken from the sources used by the author. A more comprehensive approach should have come out of studying the period. Minor errors occur, such as seeing Nasir ad-Din Shah as a frustrated reformer, or speaking in the plural of tobacco concessions to British corporations. Transliteration errors are also frequent, so that we get "Valihad" for (in simplified transliteration) Valiahd, and "Gas-Ri-Shirin" for Qasr-i Shirin. The author has, however, used a great deal of documentation to present the main facts about United States-Iranian relations; and except for the period better covered by Abraham Yeselson's *United States-Persian Diplomatic Relations 1883-1921* (1956), the book may be profitably referred to by students of the area as a useful general survey of the long period it covers.

Mr. Sheehan's book wisely concentrates on a far shorter period than Dr. Heravi's. Although based almost exclusively on published sources and secondary works and presenting little that is new to students of the area, the book may be recommended for its bringing together of materials that have not been systematically presented before. The major criticism to be made of it is that it generally takes U.S. policy at its own word and presents it as an increasing com-

mitment to Iran as a bastion of the "free world," which the United States has defended against Soviet aggression. It is correctly stated that the United States took over a position formerly occupied by Great Britain in Iran, but U.S. motivations are scarcely interpreted in terms other than those used by the American authorities themselves. The expansion of U.S. commitments abroad during and after World War II cannot be adequately understood by quoting the rhetoric of American self-justification. Both military and economic expansion were in the interest of establishing the dominant power and economic interests of the United States. Aside from these caveats Mr. Sheehan's book can be recommended as a very brief and readable overview of the development of the American interest from the period of open door policies, which had long characterized U.S. policy in Iran, to the year when American control became paramount. Mr. Sheehan does not hesitate to note that the Shah's coup in 1953 against Mossadeq was engineered by the CIA, even quoting Alan Dulles as implicitly admitting this. The book's documentation of the rapid growth of U.S. influence in Iran is cogent and welcome, and its generally accurate presentation of at least the external facts of United States relations with Iran make it an appropriate introduction to the questions it discusses.

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#### AFRICA

HUBERT DESCHAMPS, editor. *Histoire générale de l'Afrique noire, de Madagascar et des archipels*. Volume 2, *De 1800 à nos jours*. Paris: Presses Universitaires de France. 1971. Pp. 720.

With this volume Professor Deschamps and his colleagues complete the most significant French entry into the new generation of multi-authored, multivolumed histories of Africa. Though several of the projected entries (like the *Cambridge History of Africa* or the UNESCO history) are still to come, the task they face collectively and individually is that of assimilating the vast body of new research produced since the early 1950s, when Africa first

emerged as a "new" field of history. The effort itself raises a whole series of crucial questions—questions that should be asked, perhaps, of more traditional fields as well, but are raised here more acutely because these new histories of continental scope are appearing on a clean slate. Who, for example, are the intended readers? What background do they have? What criteria should govern the chronological and geographical distribution of space within the book? What kind of balance can be struck between the main lines of generalization and the detailed narrative of a local scene?

Deschamps's solution for this, his second volume, was to accept the conventional periodization for recent African history—1800–80 for the last phase of precolonial Africa, 1880–1945 for the high colonial period, 1945–70 for the decolonization and the early post colonial period. Within these three periods, he briefly outlines the broader aspects of African history as a whole, leaving individual regional specialists to deal each with his own region in his own way—and all too often without reference to what was going on elsewhere.

This relative lack of uniformity between chapters makes for even more unevenness than cooperative works usually show. Some authors see their task as an ordinary textbook presentation summarizing the current literature. Others take the opportunity to write original and sometimes brilliant synthetic accounts that go beyond anything now available in print. Yves Person's treatment of the region that centered on the upper Niger basin (essentially the Manding culture area) is a contribution of that kind. So, too, is Pierre Alexandre's treatment of the equatorial forest, which traces the broad sweep of nineteenth-century history for a region usually left out of textbooks. Claude Tardits and Pierre Kalk deal with the north central savanna from Cameroon and Lake Chad east to the Republic of the Sudan, another region badly neglected in anglophone histories of Africa. Jan Vansina's survey of the southern savanna is new in another way; it is used to update the version he presented in *Kingdoms of the Savanna* (1966). The history of Madagascar and the Mascarene Islands is not so clear an advance on the existing literature, but it is welcome simply because it is so often neglected.

Other regional chapters are far less adequate, and here the weakness of writing without a

common framework comes into its own. At worst synthesis is not even maintained at the regional level, and the regional accounts become a confusing jumble of unfamiliar names presented without explanation in the framework of a rigid narrative. The overabundance of unexplained data is especially striking in Pierre Mercier's treatment of the Guinea coast from Nigeria west to the Gold Coast, since Mercier is a sociologist; yet his account reads like an old-fashioned history text. Here and elsewhere authors who are not historians appear unfamiliar with the latest historical literature in English, as though the double barrier of language and discipline was too high a hurdle. The section on East Africa even has a remnant trace of the "Hamite" myth in the form of "caucasoid" cultural traits (pp. 265-66).

With the colonial period the organization changes. The twelve regional articles of the precolonial section are replaced by a new series centering on the colonial policy of the various European powers. This, unfortunately, shifts the emphasis to European aims and means, which must be dealt with before the authors even get around to the African reality. Most of them never reach that point, and their problem is heightened by Deschamps's allocation of space. More than half the volume is assigned to 1800-80, but only sixteen per cent to 1880-1945. In terms of pages per year, this distribution gives 4.6 for the period 1800-80, 7.4 for the period 1945-70, but only 1.7 for 1880-1945. This distribution seems to imply that the colonial period was not very important, but that assessment is never explicit, and it seems inexplicable for a book with a former colonial governor as editor and former colonial officials as at least a fifth of the other authors.

This idiosyncratic assignment of space continues in the allocations to the various colonial powers. Measured by the ratio of pages to present-day population, Liberia and the Spanish and Portuguese territories have twice the space of francophone Africa, while francophone territories in turn have double the space per capita assigned to anglophone Africa. I doubt that this was an intentional emphasis; it simply follows from the fact that it takes so much space to describe a colonial policy and administration, regardless of the number of people administered—and regardless of its actual influence on African his-

tory. The African response to these policies is seriously neglected.

The overemphasis on administrative and political history continues into the postcolonial period, joined now by occasional paragraphs on economic change. But social change, new tendencies in thought, religion, or art, and even economic change in the traditional sector are all underplayed. Instead, the emphasis of the section on 1945-70 falls very heavily on the 1960s and especially on the second half of that decade, so that history gives way to current events. More than half the discussion of Nigeria between 1945 and 1970 is devoted to the period 1966-70, explicable perhaps by the importance of the civil war, but more than half the space allotted to Ghana also deals with the period after 1960. Other spatial allocations for the colonial period are simply puzzling. It is possible that a francophone reader might want more space devoted to Madagascar than to South Africa—however difficult to justify in terms of the past or present place of either in African affairs. But why should the former British East Africa have more space than French West Africa and twice as much as British West Africa?

No reviewer has a right to ask for a book different from the one the author intended, but here is a general work, beautifully bound, printed, and illustrated. The physical setting suggests a work intended to be read now and used for reference over the years. Some of the precolonial sections have a lasting quality of the kind suggested, but the interpretation of the colonial period is just now being drastically revised by historians exploring the African side of the colonial experience. Five years from now the account given here will not be wrong, only beside the point. As for the postcolonial period, this version can hardly last beyond the time when the recent crises of the late 1960s have given way to still more recent crises of the early 1970s.

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ROBERT O. COLLINS. *Land beyond the Rivers: The Southern Sudan, 1898-1918*. New Haven: Yale University Press. 1971. Pp. xvi, 368. \$12.50.

This is the third volume of Professor Collins's



comprehensive history of the Southern Sudan, a subject that he has studied in some depth for the years 1883-1918. The present work deals with the years 1898-1918. Students of African and African-Arab history will welcome this contribution to an area that is nearly unmatched in its complexity and that remains little understood despite its importance in the dynamic of Middle-Eastern and African relations. Professor Collins's interest lies primarily in diplomatic and administrative history. He notes in his preface that early in his work he came to the conclusion that "the fundamental decisions affecting the conquest and administration of the Southern Sudan during the first decade of this century were largely the result of the Anglo-Congolese dispute over the Upper Nile." This interest in diplomatic history is reflected in the latest volume of this collection, although it is balanced by a just concern with the internal administrative policies of the British in the Southern Sudan. There is little sentimentality wasted over the motivation of British officers and consequently little effort to romanticize the laborious efforts to create a sensible and orderly administration. There is, on the other hand, a consistent attempt to understand the objectives of the administrators as well as the conditions they found, in a human and material sense. "British administrators in the Southern Sudan," writes Professor Collins, "regarded their role as fundamentally political. Modernization meant the more efficient operation of the administration rather than economic growth or raising the standard of living of the Southern Sudanese. The British were rulers first and developers second." Gentlemen remained gentlemen, and basing his observations on the class background of the British administrators, Professor Collins finds that "the paternalism of the squirearchy, not the incentives of the urban merchants, guided their actions."

I have selected one passage and limited my remarks, as is obvious, with the result that readers ought to feel deprived without reading more of Professor Collins for themselves. There is much here that deserves further comment and that will doubtless be variously interpreted. It seems highly regrettable to me that a work dealing with the Southern Sudan tells us so little of that internal history which the author was also interested in. Where are the Su-

danese? And what is this polite, discreet, imperialism that differs so markedly from the indifference and cruelty attributed to the others engaged in the same game—namely France and Belgium? Diplomatic and administrative histories are perhaps meant to whet the appetite particularly for those who are curious to know who the happy masses were that were being so efficiently administered.

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D. A. LOW. *Buganda in Modern History*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xii, 265. \$8.75.

D. A. LOW. *The Mind of Buganda: Documents of the Modern History of an African Kingdom*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xxvii, 234. \$8.50.

Professor Anthony Low has given us a volume of essays and an essential collection of documents centered on the political history of Buganda. These two volumes build upon his study of the making of the Uganda Agreement (*Buganda and British Overrule* [1960]) and the establishment of the Uganda Protectorate (in volume 2 of *Oxford History of East Africa* [1965]). In the scholarly tradition of Terence Ranger, Elizabeth Chilver, Yves Person, and Jacques Berque, Low demonstrates that detailed knowledge of the internal, precolonial history of an African society provides the most powerful and coherent explanation of colonial relationships and their aftermath. The essays, *Buganda in Modern History*, review and gather the political continuities in the history of Buganda between the 1880s and 1960s. They serve as a commentary upon *The Mind of Buganda*, a collection of sixty-two documents from the past century of Buganda history, put into context by a skillful fourteen-page introduction.

At this moment, when the Eurocentric view of the "discovery of the Nile" is again being given publicity on television, Professor Low's documents forcibly call to our attention the African view of imperial rivalry and colonial rule. The view from inside looking out reveals the complexities of interaction with intrusive British values and the cost of accommodation to political and cultural hegemony. Although

the Kabaka wrote in 1935 that "while boasting of having acquired Western education and civilization in an amazingly short period, we have entirely and completely ignored our native traditional customs," the documents also make clear that the Baganda achieved and wished to sustain a special political position in Uganda. They valued the symbolic, self-conscious unity provided by the "living and functioning form of the Kabakaship and Lukiiko" (the Lukiiko Memorandum, 1960), and they sensed great continuity in their four-hundred-year history.

There is a certain amount of perhaps unnecessary repetition among the seven essays, but the major insights cannot be faulted, and the total effect is cumulative. Just as in his selection of documents Professor Low sets out "the attitudes of mind which the Baganda have displayed on issues of major importance to them," so in his essays he keeps in focus the continuously complicated relationships between the Kingdom of Buganda and the larger protectorate, now the nation, of Uganda. The first essay shows clearly the political sophistication and adaptability of the Baganda in the final decades of the nineteenth century as they turn and use foreign religious elements for internal political purposes. The third essay argues that the British relied upon the "new men," the adaptive radicals, in the 1890s, but that in the changes following the Second World War the British relied upon the established leaders. Thus, at the very time when the hierarchy of senior chiefs was becoming more distant from the people, more bureaucratized, when "the personal nexus" between chiefs and people was atrophying, the British, too, failed to recognize the new generation. The fifth and sixth essays spell out the consequences: neotraditionalist dominance of the Lukiiko in 1957, the rise of new-style nationalist political parties, and the storming of the Kabaka's palace by Ugandan government troops in 1966.

Perhaps the most self-contained essay is the fourth. It deals with the Hancock achievement at the Namirembe Conference of 1954. Through a study of the personalities involved and the successive positions taken, Professor Low reveals the thought and the logic used to open the possibility of a unitary constitution for Uganda. But because the Kabaka did not understand what was at stake and preferred to

think in the political metaphors of the past, "the carefully contrived opportunity was never seized, and a decade later nemesis followed."

PROSSER GIFFORD

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NORMAN ROBERT BENNETT. *Mirambo of Tanzania, 1840?-1884*. New York: Oxford University Press. 1971. Pp. xiii, 191. Cloth \$6.50, paper \$1.95.

A recent popular approach to the study of African history has been through biography. "Every nation needs to build its history around the exploits of great men," Mr. Bennett maintains. He has here refined and added to previously published work to present an account of the life of Mirambo, a Nyamwezi leader from western Tanzania who was a prominent military figure in the 1870s and 1880s. Mirambo has been principally known as a warrior and empire-builder who was a major political force in his own day but whose empire collapsed on his death; to this picture Mr. Bennett adds material on his diplomatic dealings and his relations with African, Arab, and European personalities. Mr. Bennett's command of the European source materials, principally those from British official and missionary archives, is admirable, and there seems little doubt that he has uncovered almost all available evidence. The problem is that it is European evidence; we see Mirambo almost invariably through European eyes and in terms of his contacts with Europeans. Very little African material is used. As a contribution to nineteenth-century Tanzanian history the work is useful but as biography it is inadequate, and Mirambo remains a shadow.

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#### ASIA AND THE EAST

CHANG KUO-T'AO. *The Rise of the Chinese Communist Party, 1921-1927*. (The Autobiography of Chang Kuo-t'ao, Volume 1.) Lawrence: University Press of Kansas. 1971. Pp. xxi, 756. \$25.00.

If China has a Trotsky, it is not Liu Shao-ch'i but Chang Kuo-t'ao. Chang broke with Mao Tse-tung and left the Chinese Communist

party (CCP) in 1938. His subsequent obscurity—and Mao's pre-eminence—has frustrated assessment of an outstanding revolutionist. The long-awaited publication of Chang's memoirs is an important event.

An organizer of the epoch-making student demonstration of May 4, 1919, Chang played a prominent role in the formation of the CCP. He pawned clothing to support the embryonic party organization and was a delegate to its first congress in 1921. After visiting the USSR and meeting Lenin, Chang narrowly escaped Wu P'ei-fu's brutal suppression of the Peking-Hankow railroad strike of 1923. On the evening of the historic incident of May 30, 1925, he arrived in Shanghai in time to organize the city's General Labor Union. By the conclusion of this volume he had shared in the hopes, disillusionments, fleeting victories, and bloody disasters that marked the first years of the CCP. At the age of thirty he was an established party leader.

Despite reservations, Chang welcomed Leninist organizational techniques but was shocked by the Comintern's heavy-handed intervention in the policies of the Chinese party. He recognized the fatal contradictions in Moscow's decision to integrate China's Communists into the Kuomintang. Though one may question his judgment that a bilateral united front might have led to "a normal multi-party democracy," his premonition of disaster was well founded. Yet how the Comintern developed its strangle hold over the CCP remains a mystery. Chang was not the only CCP leader to recognize the drawbacks of Moscow's stewardship. Why, then, did the party repeatedly capitulate to ill-conceived directives? In return for economic aid? (Chang says little on this.) Because the party was spellbound by the mystique of the Russian Revolution? (Chang and others who visited the USSR were disenchanted.) Was it the charisma of Soviet advisers? (Many of these men displayed abysmal ignorance of China, and even the formidable Borodin was no Rasputin.) Central to the problem is the prestigious Ch'en Tu-hsiu who became a scapegoat for Comintern failures after doggedly defending its distasteful policies. Yet after Ch'en's deposition a neophyte Comintern adviser was able to pick his successor. If anyone was in a position to understand why these things hap-

pened it was Chang Kuo-t'ao, yet his book leaves the mystery unsolved.

Chang's contributions heavily outweigh his omissions. Historians of the CCP have heretofore drawn upon party and Comintern documents. Painstaking dissection of doctrinal controversies has been highly abstract, and even Harold Isaacs's vivid *Tragedy of the Chinese Revolution* submerges individual protagonists in tides of mass movements. Chang has now written the most vital account of early CCP history since Isaacs's book appeared in 1938. This volume is, as A. Doak Barnett has observed, "a remarkable combination of involvement and dispassionate observation." Chang breathes life into fellow participants who have hitherto been little more than pasteboard mannequins—the imperious Comintern agent Maring, the party patriarch Ch'en Tu-hsiu, and the playboy revolutionist Ch'en Kung-po. Yet, in his guise of dispassionate observer, Chang fails to do justice to one key figure: himself. He moves through the battle-scarred landscape of the Chinese revolution noting others' foibles while his own record remains virtually unblemished. His thoughts and deeds are dutifully recorded but he remains a one-dimensional man. To bring Chang to life we will need the observations of others, including, of course, Mao Tse-tung. We eagerly await Chang Kuo-t'ao's second volume.

JOHN ISRAEL

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ARTHUR N. YOUNG. *China's Nation-Building Effort, 1927-1937: The Financial and Economic Record*. (Hoover Institution Publications 104.) [Stanford:] Hoover Institution Press, Stanford University. 1971. Pp. xx, 553. \$19.50.

This is Arthur Young's third large book on the economy of China during the period of Kuomintang (Nationalist party) rule. Where his earlier books dealt with foreign economic aid to China from 1937 to 1945 and with wartime finance and inflation, the present volume is a memoir and a history of the prewar efforts of the Nationalists to transform the system of public finance inherited in 1927 from the warlord Peking governments into an instrument for achieving national unity and (it was hoped) economic development. As financial adviser to the Republic of China from 1929 to 1947, the

author was more or less closely involved in the formulation of the financial policies of the Nanking and Chungking governments, so that he writes with some authority on the more technical aspects of fiscal and monetary affairs. From the records in his possession he occasionally offers valuable data not available elsewhere, for example, statements of the receipts and expenditures of the Nanking government for the fiscal years 1936 and 1937. Mr. Young, an honest reporter, acknowledges that his discussion of modernization and development (pp. 287-401) does "not have the depth of the treatment of fiscal and monetary matters with which I was concerned at first-hand and for which I have primary and often unique data."

The essential issue raised by this book is the classical one of the glass with some water in it: is what we have before us a vessel that is more accurately described as half empty or as half full? Mr. Young, in the summary passages that appear at several places in the text, agrees that the political and social development of China during the years 1927-37—which are not the main subject of his book—lagged behind the ability of the Nationalist government to rehabilitate and reorganize substantial parts of the modern sector of the economy. Thus central government revenues were increased from almost nil to one billion dollars Chinese currency, a modern budgetary and administrative system was organized, the market for internal borrowing was developed, most foreign debts in arrears were settled, the currency system was reformed and unified, a central bank was established, transport and communications were improved, and a modest program of development was begun with both domestic and foreign capital and technical aid. All were promising beginnings when compared to the period of warlordism that preceded. But, the author also notes, the rural areas were largely unchanged and their resources never tapped by the center, no substantial agrarian reform was undertaken, provincial and local governments were dominated by militarists, administration was frequently corrupt, the central government wasted its resources in civil wars, its political base was a narrowly circumscribed social elite, and the populace at large was inert and had no voice in the government.

After looking at the glass with some care Mr.

Young has decided that it was half full. Had war with Japan not come in 1937 the Kuomintang would not have been overthrown by its Communist party opponent. The prospect was for continued "evolutionary change and growth" on the basis of the relatively successful fiscal and monetary policies of the decade here considered. For many—indeed most—students of Kuomintang China, who are more skeptical about the efficacy of fiscal and monetary policy in coping with political and social crisis, the glass in question would be described as more than half empty in 1937 and leaking dangerously through numerous cracks.

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#### AMERICAS

GEORGE ATHAN BILLIAS and GERALD N. GROB, editors. *American History: Retrospect and Prospect*. New York: Free Press. 1971. Pp. viii, 471. Cloth \$7.95, paper \$4.95.

In the frequency and thoroughness of their self-scrutiny American historians must surely surpass all other practitioners of their craft. Theirs is the dedicated priesthood of a cult agreed upon only one dogma, "Know thyself." This proceeds in large part no doubt from phenomena which, to a European eye, appear distinctively American—the sheer volume of academic output and the number, diversity, and wide geographical scatter of the institutions that promote it. In such a situation the provision of formal channels of communication and their maintenance by regular dredging is a convenience almost amounting to a necessity. (Which should not, incidentally, diminish anyone's gratitude, least of all that of the foreign scholar, at finding this task so conscientiously and ably done.)

But behind the bibliographer's urge to order there is also the philosophic impulse to assess and evaluate. (No accident perhaps that "clerk," in its original and derived meanings, may cover both.) That the American historian sees himself—and is seen by many, perhaps most, of his readers—as the unacknowledged legislator of his times is almost as evident as that he is deemed to be the Rhadamanthus of

the national past. The pages of the *Newsletter* testify as vehemently to the first role as the pages of the *AHR* do to the second.

Moreover, the flexibility of the discipline is greater in the United States than elsewhere. It is more open to innovatory techniques and borrowed insights. There is therefore not only the social but, so to say, the technological, impact to be recorded. The American *Clio* is not only ready to sing songs of social significance; she is also prepared to try them on the twelve-tone scale or the Moog synthesizer. This may stimulate (it certainly does not impede) the rapid turnover of interpretations. Certainly the corpus of American history appears unique in its quick responsiveness to every shock wave that reaches it from the nation itself, so that today's national crisis becomes tomorrow's *nouvelle vague* of revisionism.

The authors of *American History* are in varying degrees responding to all these developments. They are seeking to update, in many fields, our swiftly obsolescing bibliographies. They are trying to record and relate the fresh evaluations of the American past that have been emerging in the sixties. And where relevant and practicable, they have been trying to do a historian's job on historiography, to enquire and explain where the new trends have come from, what they signify, where they may point. The result is a diversified, rich, and informative volume.

Two general impressions, irrespective of period or approach, remain from a reading of these very different essays. The first is the decline of great names. Certain figures, formative teachers, and interpreters do, of course, recur in these pages. But the age of the giants is over. There are no Turners or Beards. With the tragic losses of Hofstadter and Potter, even the ranks of the near-giants are sadly thinned. But a second impression is almost equally strong, of the great range and liveliness of the profession as a whole, of its impressive output, its restless energy, and its Oedipal addiction to patricide as a form of occupational therapy. Just as there are no giants, so there are no idols.

Less strong than an impression but more powerful than a whiff is a certain emanation from these pages of what an unkind critic would call insularity. The rich internal resources of the American past have encouraged

a certain exclusive concentration, even in our ecumenical present, on the American experience. There is rather too little evidence in these pages that the American historian's curiosity follows where his civic conscience or his vacation travel might lead him, to ask how far his findings about the American past might be enriched or clarified by being brought into comparison with the experience of other peoples at other times. We all wear blinders here; no one has panoramic vision. But because the American horizon is so broad it sometimes lends itself more readily to the illusion of being the totality of the perceptible.

The ten essays that make up *American History* fall into two categories—the chronological and the topical. All, that is, save Lee Benson's on "Middle Period Historiography: What Is To Be Done?" This, as its Leninesque interrogative might suggest, is as much an indictment as an inquiry. The research on 1816 to 1860 has, he charges, yielded only "unsystematic and trivial" information. But when one discovers the reason for this, namely that historians have obstinately declined to become social scientists, one realizes that the indictment is general. What we have here is a hard-hitting if rather old-fashioned restatement of the case for a scientific history, which could as reasonably be applied to any other period of the American past. Which is not to deny that Benson scores some very palpable hits, especially when he directs attention to our profession's sloppy use of terms and our inveterate addiction to "proof by haphazard quotation."

Next to Benson's, the widest-ranging essays are those by Samuel P. Hays on social history and James P. Baughman on economic and business history. Hays makes the large claim that the distinctiveness of social history lies not in its subject matter but in its way of looking at the past, concerning itself with "human interaction." What? Have we all been writing social history without knowing it? Not at least in its fullest realization, for that, as we are reminded apropos of immigration history, "requires that it be freed from the particular circumstances of time and place." If that seems beyond the reach of most of us poor prisoners of the here and now, we can nevertheless endorse much of what Hays has to say about getting social history away from its emphasis on

problem solving and its subservience to ideology, as also his warnings against the tendency to dichotomize.

Baughman's essay is a most illuminating survey of the ground recently cultivated by the economic historians with the new tools that they have derived from contemporary economic theory and, even more, with the new data that econometrics has put at their disposal. Here, it can hardly be disputed, there has been real progress. In 1944 the Committee on Research in Economic History of the Social Science Research Council identified three "target areas"—the roles of entrepreneurship and government in economic growth and the history of the firm. In all these, three solid advances have been made. It is not often that scholarship thus complies with its agenda.

Urban history, as a growth topic, receives appropriate scrutiny from Richard Wade, who writes not only a survey of past work and an agenda for future researchers but also a very perceptive and succinct history of American urbanization itself. For a declining trade, diplomatic history, Ernest May provides a comprehensive and far from tearful obituary, hopeful of the phoenix of international history that is rising from the ashes of the old documentation.

For many readers, however, the greatest practical utility may well be found in those essays that survey in a preponderantly bibliographical form recent work in separate periods. All these are well done, some in conspicuously comprehensive detail—like Jacob E. Cooke on the Federalist age—some with a greater freedom of choice and a more explicit commitment to one style of approach—like Gerald Grob, who hails the exhaustion of moralism in Reconstruction historiography and sees the future as belonging to a neo-David Donaldism. Thomas Barrow, surveying the colonial scene, is especially helpful in relating the significant periodical literature to the more familiar book-length studies. George Billias follows with a freshly categorized analysis of the large outpourings of the Revolutionary era, which constitute such an impressive run-up to 1976 and All That.

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HERMAN E. KROOSS and MARTIN R. BLYN. *A History of Financial Intermediaries*. (Random House Books in Finance.) New York: Random House. 1971. Pp. xi, 254. \$7.50.

In all fields of scholarly endeavor there has recently been a growing demand for the work of the synthesizer. Specialists of all types have found it almost impossible to keep abreast of the flow of articles and books without the help of summaries and syntheses. Market demand is fortunately being met in some areas of activity, and this history of financial intermediaries provides an example. In 246 pages the reader is offered a sweeping survey of the creation, the response to environmental changes (internal and external), innovative actions, variations in growth rates, and the general performance of financial intermediaries in the history of the United States. The book deals not only with commercial banks and mutual savings banks, as had earlier similar books, but also with insurance companies, general insurance companies, investment banks, credit unions, trust companies, savings and loan associations, noninsured pension funds, and investment companies. In comprehensiveness of coverage it is unique among histories of financial institutions. Those whose activities are analyzed now hold one-half of the total wealth of the nation, a fact that indicates their importance in the economy. As measures of growth of financial intermediaries, assets of banks and insurance companies approximated ten dollars per capita in 1800, and assets of the ten discussed now amount to about five thousand dollars per capita.

Although the reader must peruse carefully rather than breezily, the authors strive successfully to keep the narrative clear and straightforward. In the preface they explain why they concentrate on participants in this segment of economic life—on borrowers and lenders and the processes by which they were brought together. The introduction contains definitions of financial intermediary, the theoretical framework utilized, what financial intermediaries do, and their historical growth. Of particular concern is innovation in three areas—those encouraging saving, those easing the borrowing of funds, and those narrowing "the gap between the saver and the investor by improving liquidity and by adding to geographic mobility" (p. 4). In eight time periods from colonial times to

1970 the extent of and variations in innovations are analyzed—among others, the surges in innovative response occurring in 1816–36 and 1945–70. Twenty-one tables and three “figures” elucidate the text. The presence of a few typographical errors does not impair effectiveness of presentation, but both the index and the bibliography are so brief that they have limited utility. Minor limitations notwithstanding, this synthesis serves to remind the economic historian that assets of financial intermediaries have grown more rapidly than those of any other sector of the American economy. From it the noneconomic historian can learn where the financial power is and the process by which it got there.

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CONVERSE D. CLOWSE. *Economic Beginnings in Colonial South Carolina, 1670–1730*. (Tricentennial Studies, Number 3.) Columbia: University of South Carolina Press for the South Carolina Tricentennial Commission. 1971. Pp. ix, 283. \$6.95.

PAUL S. TAYLOR. *Georgia Plan: 1732–1752*. Berkeley: Institute of Business and Economic Research, Graduate School of Business Administration, University of California. 1972. Pp. xviii, 322. \$6.50.

During the third quarter of the eighteenth century the rice colonies of South Carolina and Georgia experienced an economic boom unparalleled among the British-American colonies since the emergence of the Caribbean sugar islands at the end of the seventeenth century. So great was the boom that on the eve of the Revolution the rice-producing regions of those colonies were almost certainly the most prosperous area on the British North American mainland and probably also the most promising area of agricultural investment in the whole of Britain's overseas dominions. Clearly written and thoroughly researched, these two volumes provide the fullest and most authoritative account ever published of the beginnings of this remarkable development in the socioeconomic foundations of South Carolina and Georgia.

For South Carolina those foundations were slowly laid. The central themes of its first two decades, Clowse makes clear, were the twin failures of the proprietors' elaborate schemes for

the development of the colony and of the search for a commercial crop. Trade in deer-skins and in surprisingly large numbers of Indian slaves with neighboring tribes generated some profits. But as late as 1690 the economic base of the colony was extremely precarious and the population correspondingly sparse. Only after 1690 did rice gradually emerge as a primary staple that provided, together with subsidized naval stores and the Indian trade, the profits necessary for the substantial expansion of the colony between 1705 and 1715. That decade was decisive in casting the socioeconomic life of the colony into a 'West Indian mold characterized by a small, slowly growing European population; a rapidly increasing number of African slaves, who accounted for a continuing upsurge in productivity as each slave produced approximately one ton of rice annually in addition to other subsidiary products; the concentration of good rice lands into the hands of a small white elite; and the emergence of the supporting mercantile groups necessary to perform the functions required by a plantation economy. Over the next fifteen years this expansion was slowed somewhat by a series of internal problems and adverse external conditions, but the heavy importation of African slaves continued, and rice received ever greater emphasis. Though economic troubles continued to hinder the colony for another decade, by 1730 it was poised on the threshold of the great boom that began in the 1740s as Carolinians gained access to larger international rice markets and developed indigo as a profitable second staple whose production was heavily subsidized by Britain.

There are several minor problems with the Clowse volume. Unfortunately it stops a full decade before the boom got underway, once again calling into question the wisdom of using political events, in this case the end of the proprietary, to determine the limits of social or economic studies. Moreover, Clowse's explanation for the introduction of slavery (the unsuitability of the climate for white labor) and his account of where Carolinians acquired the technical knowledge to produce rice (he does not consider the likely possibility that they learned it from their African slaves) are unsatisfactory, and the hard economic data on which the study is based are disappointingly

thin. Given the nature of the extant sources, however, it is doubtful that any future scholar will improve upon this study, and the author compensates for the lack of data with a series of responsible and informed projections.

Whereas the problem of the early South Carolinians was to find a viable commercial crop, the difficulty of the enterprising spirits among the first Georgians was to overcome the Trustees' opposition to their efforts to follow the Carolina example. In their attempt to create a colony of small, working farmers, the Trustees steadfastly clung to their original plan to exclude slavery, limit the size of landholdings, encourage white immigrants and the use of servant labor, and rely on public support for financing. The first twenty years of the colony's history, Taylor shows in an exhaustive and detailed narrative, were a perpetual tug of war between the Trustees and their supporters in the colony who were determined to keep Georgia from falling into the West Indian model and those other settlers who wanted desperately to emulate that model. A chronicle of the Trustees' gradual defeat on one point after another until their final relinquishment of the last important provisions of their plan in 1749, Taylor's volume is frankly revisionist in tone. He shows that the plan was more practical than earlier historians have suggested, that it worked quite well for over a decade, and that the Salzburgers, Scottish Highlanders, Dutch settlers, and other small farmers consistently supported the Trustees.

But the point is not that the plan was impractical but that it was out of harmony with the central animating spirit of the mid-eighteenth-century British Empire. For what both of these volumes illustrate so profusely is the extraordinary extent to which the momentum for colonial economic development was supplied not by proprietors or trustees in Britain but by the settlers on the spot and to which the primary impulse underlying that momentum "was not altruism or trial of a political system, but profits"—in Taylor's words, "the search for private gain." As many recent studies have implied, everywhere in the British-American colonies men's material appetites were continually whetted by the economic opportunities offered by an environment of abundance. Living next to the colony that at the very moment of the

founding of Georgia was on the verge of a dizzying economic boom and in an environment that was supremely suitable for following the Carolina example, Georgians would indeed have had to be Spartan to have resisted the temptation, in Egmont's disconsolate phrase, to become "rich in the ways of other provinces."

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SARAH MCCULLOH LEMMON, editor. *The Pettigrew Papers*. Volume 1, 1685-1818. Raleigh: State Department of Archives and History. 1971. Pp. xl, 699. \$15.00.

Volume 1 of the *Pettigrew Papers* contains the records of the early members of the Pettigrew family of the Albemarle Sound region of North Carolina. The principal items are the correspondence of the Reverend Charles Pettigrew, North Carolina's first elected (although not consecrated) bishop of the Episcopal Church and a successful colonial planter, and his only surviving son Ebenezer, an imaginative and prosperous planter himself.

After a brief but informative introduction by Dr. Sarah McCulloh Lemmon, biographer of Charles Pettigrew and the last of a series of editors who have worked on the papers, the records begin with wills dated 1685 and 1753; then follows the late eighteenth-century correspondence, which comprises the first section of the *Papers*. The records tell the story of an educated Philadelphian who came to eastern North Carolina and with devotion to the church and to his own business affairs rose quickly to a place of prominence in the Edenton area. The death of Charles Pettigrew's first wife, his remarriage, and the establishment of an estate on the southern part of the Albemarle Sound are recounted in a series of letters that includes a wealth of materials for the social historian. Matters relating to colonial religion, health and medicine, plantation management, travel, and postal customs emerge from the letters as does the story of strong men and women wrestling the best life possible in a difficult land.

The after-effects of the American Revolution reflected in the *Papers* include a movement toward internal improvements (including canal building), the problems encountered as the Anglican Church became the Protestant Episcopal



Church, the political struggles of the new nation—with the Pettigrews predictably Federalist—and the flourishing of educational institutions. The Pettigrew sons went to the newly founded University of North Carolina, where the father's concern for his sons' morals was reinforced by their reports of life in Chapel Hill in the 1790s. "Cursing & swearing is carried on here to the greatest perfection" with the students preferring Thomas Paine's deistic *Age of Reason* "to all the books that were ever wrote since the creation of the World." Other impediments to learning included a dancing school, six or more boys to a room, a scarcity of books and beds, poor food, and "innumerable" chinchies.

Pettigrew shortly withdrew his sons from the university only to have his oldest son die, perhaps of one of the innumerable fever epidemics that regularly swept the region. Ebenezer then joined his father on his increased holdings in Tyrrel County south of the Albemarle Sound, and they engaged in the growing of rice and wheat. Ebenezer applied himself to correspondence with his friends and the development of machinery and buildings to handle the culture and harvest of the plantation products. Ebenezer's marriage to his cousin provided him with an opportunity to build a large new home and furnish it with the best that could be provided by his New York factor. His correspondence with his wife, who spent the winters with her family in New Bern, shows him to be a kindly master, husband, and father. His devotion to his lands and his family frequently tore him emotionally between his desires to have his loved ones with him and his concern for their health and isolation at his home near Lake Phelps.

Above all, the excellently edited and indexed *Pettigrew Papers* reveal the difficulties that had to be surmounted by even the most prosperous Americans of the early national period. In addition to the intimate details of life in the Albemarle Sound region, student life at the early University of North Carolina and at Princeton is related in the letters of children, relatives, and friends. Views are provided of Haiti, New York, Philadelphia, Baltimore, and Norfolk. The difficulties of transacting business at long distance, with poor mail service, no dependable way to transmit money or goods, and inordi-

nate time consumed by even the most simple of transactions, make one marvel at the determination and abilities of the Pettigrews and of those like them who not only survived but prospered. Volume 1 ends in December 1818, significantly, with the purchase of a slave. Succeeding volumes from the North Carolina Department of Archives and History (which, incidentally, has provided this monumental volume for "the actual cost of printing only") should provide further valuable insight into the Pettigrew family and the region, state, and nation in which they lived.

J. EDWIN HENDRICKS

Wake Forest University

DOUGLAS SLOAN. *The Scottish Enlightenment and the American College Ideal*. (Teachers College Studies in Education.) [New York:] Teachers College Press, Teachers College, Columbia University. 1971. Pp. xi, 298. \$10.00.

In this mistitled collection of seven essays Douglas Sloan has limited himself to the "thoughts and careers of representative individual educators who were related to the [Presbyterian] academy and early Princeton traditions" instead of appraising the "full impact of Scotland upon the American college." His book is the weaker for it, since most of his subjects are well known (as his excellent bibliographical essay shows), and his presentation of them is seriously flawed.

The basic problem is that he has tried to demonstrate that eighteenth-century American higher education was influenced by the universities of Scotland and the ideas of Scottish intellectuals—a proposition whose meaning is far from clear (if not nebulous) and the logical conditions for proving which are exacting (if not impossible). (See Quentin Skinner, "The Limits of Historical Explanation," *Philosophy*, 58 [July 1966]: 199–215.) Mr. Sloan has neither clarified the one nor satisfied the other, and accordingly his text is fraught with "maybe," "probably," and "seems to have been." When evidence for the influence of or upon his subject peters out, he retreats to lists of the man's former students, who in later life "seem to have" borne the intellectual characteristics chosen as "representative." Because of the slippery

nature of what he is trying to prove, he is effectively forced to accept any and all suggestions of connection between his subjects to such an extent that he never dismisses or discounts evidence even after warning of its possible bias or weakness. In short, the author would have been well advised to heed Hume's warning that mere contiguity in time and space (or similarity) is not sufficient evidence of cause and effect (or "influence").

The book is further marred by an egregious Whiggism. According to Mr. Sloan the Scottish universities were consistently "modern" and "progressive"—which is usually equated with "public service," "utilitarianism," or "reform"—and this in spite of three "watershed" reforms that were never executed. Mr. Sloan seems to have written from the liberal-progressive belief that the burden of proof for change is on conservatives, not reformers, a position that is least valid in education whose very responsibility is the preservation of social ideals.

JAMES AXTELL  
Yale University

ROBERT ZEMSKY. *Merchants, Farmers, and River Gods: An Essay on Eighteenth-Century American Politics*. Boston: Gambit. 1971. Pp. xiii, 361. \$10.00.

A blend of political history and historical political science, this book discusses Massachusetts politics between 1730 and 1755, using an analysis of legislative decision making as a focal point. Zemsky finds that by comparison with its modern counterparts the Massachusetts Assembly's organization was primitive, with no standing committees, no seniority system, few rules, and debates that actually influenced how members voted. Decision making in this body was orderly not because it was institutionalized, but because the committees that drafted legislation and the debates preceding enactment were dominated by a stable group of leaders—distinguished by social status rather than legislative experience and by the related ability to acquire the contacts, the calculated mode of behavior, the understanding of patronage politics, and the pragmatic approach that united professional politicians. Potential leaders, usually men from distinguished fami-

lies, representatives of coastal towns, or graduates of colonial colleges, emerged upon first appearance in the house, while others, once identified as back-benchers, remained without substantial power throughout their legislative careers.

Such a system worked because the central government concerned itself with defense, justice, taxation, currency manipulation, and little else. The agrarian majority in Massachusetts tolerated the political dominance and officeholding monopoly of upper-class leaders in return for a narrowly defined public policy that served its interests. Although deference to social status influenced electoral behavior as well as legislative leadership, politics remained responsive because professional politicians recognized the limits of deference and took popular positions on sensitive issues in order to preserve their autonomy on other measures. By analyzing the relationships among legislators in the house and between legislators and their constituencies this study adds much to our understanding of colonial political processes.

Zemsky is penetrating as a historical political scientist, but he is less successful at more traditional political history. His interpretations are conventional, rely heavily on standard sources, and, moreover, are flawed with minor errors. Many of the book's weaknesses, however, stem from a major organizational problem. Like many historians who use extensive quantitative sources, the author felt constrained to restrict his presentation of statistical evidence in the interest of historical artistry and has relegated the numbers to an appendix. This decision is counterproductive because statistical analyses provide the most original and suggestive material in the study and the most concise means of supporting major interpretations. A series of biographical sketches provides a partial substitute for the absent quantitative evidence, but they are lengthy, repetitious, and digressive, creating an impression of padding. In marked contrast, the statistical appendix is clear and forceful. Historians should profit from Zemsky's conclusions about political behavior and his methodological discussions, but it is hoped that they will avoid his method of presentation.

EDWARD M. COOK, JR.  
University of Chicago

RICHARD HOFSTADTER. *America at 1750: A Social Portrait*. New York: Alfred A. Knopf. 1971. Pp. xvi, 293, xiii. \$6.95.

This book was not complete when Richard Hofstadter died in October 1970. What appears under the title *America at 1750* is in fact an unfinished section of a volume intended to be the first of three on the history of the United States from around 1750 to the recent past. In a prospectus written for his publisher Hofstadter explained that each volume would run to around 500,000 words and would cover a period of around seventy-five years. The decision to publish this fragment, apparently made after Hofstadter's death, was sound, for although the work is incomplete it has unity, and it has what one would expect of its author—fresh insights and literary grace.

In substance the book is a social history of the thirteen continental colonies at 1750: there are chapters on population and immigration, white servitude, the slave trade, black slavery, the middle class, the churches, the Great Awakening, and the effect of the Awakening on the churches and the larger society. These are all familiar subjects in the social history of early America, and much of what is said about them is also familiar. For example, in his discussion of the colonial population, Hofstadter emphasized the connection of immigration to economic growth. In his description of the organization of the slave trade he repeated Philip Curtin's findings that in the long history of the trade the territory of the United States received slightly less than five per cent of all slaves brought to the New World. And, to give one other example of familiar material in the book, the discussion of the Great Awakening makes a good deal of lay-clerical conflict, itineracy, and the separation of churches under revivalistic strains.

Yet, the book has its own freshness, in part because of Hofstadter's conception of social history. There is throughout the book a clear, though largely unstated, concern for social psychology. The social history that the book tells is the history of attitudes, feelings, values, and the psychology of groups as much as it is the description of the "objective" conditions of society, such as numbers of immigrants, rates of economic growth, amounts of property held, and class organization. Consider, for example,

the long-standing assumption of colonial historians that middle-class values pervaded all classes of white society. Hofstadter accepted that assumption—both upper and lower classes looked to the middle, he wrote, because the middle class was large, assertive, and energetic. If this is one of the "facts" of social history Hofstadter's insight into the way the upper class was recruited from the middle, which carried its bourgeois standards upward, is important and suggestive.

The emphasis on church history as a means of revealing social development is also interesting. The methods of analysis in the book are not new here—much of the recent writing on churches and revivalism is replete with a sense of the connections between religion and politics and the coming of the American Revolution. And indeed, Hofstadter in the last three chapters of *America at 1750* resorted to biographical and narrative techniques that are reminiscent of those in his *The American Political Tradition* (1948). There are, for example, shrewd assessments of the revivalists—the portraits of Jonathan Edwards and Gilbert Tennent are vivid, and the accounts of George Whitefield and James Davenport are remarkably sensitive and gentle.

In these vignettes, and in the narrative, the book provides a valuable analysis of the effects of the Awakening on society and in particular the meaning of the event for civic life. Here Hofstadter looked forward to the American Revolution. What he wrote was fragmentary, but provocative in the best sense. One hopes that other scholars will follow his lead, amplifying and perhaps qualifying his insights. In the process the full value of *America at 1750* should become obvious.

ROBERT MIDDLEKAUFF  
University of California,  
Berkeley

RICHARD BAUMAN. *For the Reputation of Truth: Politics, Religion, and Conflict among the Pennsylvania Quakers, 1750-1800*. Baltimore: Johns Hopkins Press. 1971. Pp. xviii, 258. \$10.00.

Richard Bauman is the most recent in a long line of historians and sociologists who have sought to understand what happens when a group of people who have foresworn worldly

involvement, power, and material gain—in this case the Quakers of Pennsylvania—are obliged to assume power, employ it, and reconcile its uses with their religious ideology. By studying the leaders of Quaker society in eighteenth-century Pennsylvania Bauman is able to define three types of behavioral response “among the politically relevant Quakers”: (1) the “politicians,” who embraced the world, enthusiastically engaged in politics, and accommodated their religious principles accordingly; (2) the “reformers,” who called for a withdrawal from politics and worldly affairs and a return to the inward-looking, self-abnegating life of the First Publishers of the Truth; and (3) the “politiques,” who sought the *via media*, hoping to maintain the faith of the founders by using political power to convert the surrounding society to the Quaker ideal. Bauman follows two generations of historians in arguing that for the first half of the eighteenth century Quaker leaders in Pennsylvania mastered the world of business and politics and accommodated their religious views to this worldly success, but that in the second half of the century, with the pressure of war and internal dissension increasing, the “reformers” began to dominate the Quaker leadership and brought a revitalization of the movement through their re-emphasis of the doctrine of meekness and social service rather than the exercise of political power. Others—such as Rufus Jones, Frederick Tolles, and Sydney James—have been at pains to point this out for many years. Bauman’s contribution consists in clarifying the different responses of Quaker leaders to the time of troubles after 1750, in re-emphasizing that the reformers who counseled a withdrawal from politics were still acting politically since they too were bent on influencing the “setting and pursuit of public goals,” and in reminding us that, as Quakers struggled during and after the American Revolution to find an outlet for their political energies that would not conflict with their religious testimony, they discovered a commitment to the oppressed and disinherited members of their society that has ever since given them an influence in moral stewardship out of all proportion to their numbers.

The author’s attempt to use anthropological theory to explain the several reactions of Quaker leaders to political responsibilities is

not especially fruitful. Interdisciplinary analysis requires more than the frequent use of terms such as “role strain,” “processual analysis,” and “adaptations of belief systems”; and in the end this work gains little by reference to the work of political anthropologists. Historians have long known what Friends themselves so clearly expressed—that for Quakers it was extraordinarily difficult in the strife-filled second half of the eighteenth century to exercise political power while remaining true to the original religious commitment of the Society of Friends.

GARY B. NASH  
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THOMAS FLEMING. *The Man Who Dared the Lightning: A New Look at Benjamin Franklin*. New York: William Morrow and Company. 1971. Pp. x, 532. \$12.50.

In the early 1960s Thomas Fleming’s *Now We Are Enemies* came to my attention; a treatment of the battle of Bunker and Breed’s Hills. It was evident that the author had the ability to write attractively and to hold his readers’ attention. In *The Man Who Dared the Lightning* Fleming gives further indication of his talents as a historical popularizer. Both a novelist and historical interpreter, Fleming has not bound himself by some of the rules of writing and documentation followed by others. On the first page of the text, for example, he sets the tone by describing what happens in all thunderstorms as if he were observing the particular squall during which Franklin performed his electrical experiment with the kite. “From bright June sunshine, the sky above hot, muggy Philadelphia began changing to a sour gloomy gray. . . . Windows slammed, tradesmen shut doors . . . , mothers hastily called children indoors, and idlers vanished from street corners.” As the storm broke, “Franklin asked if everything was ready. William nodded” (p. 3). Or (p. 53), “and then with a small shrewd smile [Franklin] noted. . . .” These points are obviously incapable of documentation, but Fleming uses such devices with surprising effect. Beyond cavi, Fleming’s story is the best written of the many available Franklin books. His novelistic style

gives a good mixture of interest, lightness, humor, and serious, well-documented reporting. Fleming is aware of both recent and older treatments of Franklin's career and uses them well. Fleming occasionally relies on inadequate materials; his coverage of Pennsylvania politics would have been vastly improved by using William S. Hanna, *Benjamin Franklin and Pennsylvania Politics* (1964) rather than the source listed in Fleming's bibliography.

There are a few minor errors, as where Fleming refers (p. 14) to Paxton and Donegal "townships" in Pennsylvania. According to James Hutson, a former assistant editor of the *Franklin Papers*, they were not "townships" but counties. One finds an occasional questionable emphasis, as when Fleming indicates that by the time of the Declaration of Independence, Franklin had turned his back on "founding a Western colony" (p. 329). Then later Fleming tells us that Franklin continued his speculative interests well into the war years, and the records indicate that as late as 1788, in a letter to Charles Thomson, Franklin still sought a western tract.

Since it appeared, Carl Van Doren's *Benjamin Franklin* (1964) has been the standard, broad treatment. Less encyclopedic by design, Fleming's study omits much of Franklin's early life and lays less stress on his subject's multifaceted interests, which Van Doren was at such pains to cover thoroughly. Save in a few areas, *The Man Who Dared the Lightning* may now well supersede Van Doren's work as the most widely used biography of Franklin's career.

CECIL B. CURREY  
University of South Florida

this disappointing volume were not able to devote as much time to the editing of the work as they did to its compilation, for the former does not match the latter. The introduction is too brief to be satisfactory, and it contains inconsistencies that could have been resolved as well as a glaring typographical error on page xvii that certainly should have been caught and corrected. A seventeen-page section of identifications of persons and places at the end of the book is some help. In the absence of a substantial narrative and analytical introduction, this publication might at least have been the occasion for a comprehensive bibliography of the North Carolina Regulator movement but instead only a very inadequate two-page section on sources is offered. Fortunately, there is an index. All in all, though, this work is an inferior piece of scholarly editing. But why should this be so when both the publisher and the editors, who are well qualified for the task, are capable of much better? The answer—undue haste—is revealed in the foreword. The volume was rushed through to completion in less than a year and a half in order to be published "to coincide with the observance of the two-hundredth anniversary of the Battle of Alamance" (May 16, 1771), the climactic event of the Regulator movement. The objective is a laudable one, but Alamance and the Regulator movement as a whole would have been better memorialized by delaying publication beyond the anniversary in order to produce a scholarly work worthy of the importance of the historical event.

RICHARD MAXWELL BROWN  
College of William and Mary

WILLIAM S. POWELL *et al.*, editors. *The Regulators in North Carolina: A Documentary History, 1759-1776*. Raleigh: State Department of Archives and History. 1971. Pp. xxxix, 626. \$12.50.

The chief virtue of this bulky volume is that it brings together for convenient reference in one book many significant documents on the North Carolina Regulator movement that were hitherto accessible in various volumes of *The Colonial Records of North Carolina* (1886-90), in the British Public Record Office, and elsewhere. It is unfortunate that the compilers of

LOUIS DE VORSEY, JR., edited and with an introduction by. *De Brahm's Report of the General Survey in the Southern District of North America*. (Tricentennial Edition, Number 3.) Columbia: University of South Carolina Press for the South Carolina Tricentennial Commission. 1971. Pp. xvi, 325. \$12.95.

This is a splendid work: a little-known report by a remarkable cartographer and observer, edited with scholarly depth and sensitivity and printed in a handsome and sturdy form. The author of the report, William Gérard De Brahm, was a German immigrant who became

surveyor-general of South Carolina, Georgia, and finally, following the Peace of Paris in 1763, surveyor-general for the entire Southern District of North America. He was erudite to the point of pedantry, self-assured to the point of arrogance. He quarreled with colonial governors over his authority; he built forts in the wilderness and fortifications in the cities; he proposed grandiose engineering works that would have bankrupted the tiny English colonies. Nevertheless, he was a superior man who deserved the confidence placed in him by King George III. His contributions are several. He was, as one expert has put it, "the first strictly scientific cartographical expert to practice his art in the Carolinas." His maps, plans, and surveys of the little-known region of Florida acquired by the English Crown in 1763 provided the detailed knowledge necessary for intelligent planning for the area by the English government. Second, De Brahm's description of the natural history of the region and the potential uses to which its resources could be put were a needed corrective to the harsh and unscientific judgments of previous observers. One wonders today at the inability of early travelers in Florida (and in the Southwest, for that matter) to see through the least attractive features of these areas to their potential values. De Brahm was one of those able to look beyond the barren sandy beaches and swarming mosquitoes to the rich storehouse that Florida was. Yet even De Brahm concluded that "I cannot expect my eight years Experience on sandy Soil should be sufficient to out-do the . . . many thousand years entertained Notion" against such a soil. Third, De Brahm was an acute observer of Indian life and not only wrote perceptive comments about the Indians of the Southeast but included a "Compendium of the Cherokee Tongue in English." His surveys of South Carolina, Georgia, and East Florida published here omit only his "Ephemeris" or daily record of weather conditions during certain years, and certain mathematical computations and tables. Twenty-nine maps and sketches demonstrate his skill as a draftsman and cartographer. A solid fifty-seven-page introduction, based on exhaustively mined manuscript sources in England and America, makes the book not merely an edited document but an account of De Brahm in America. However unlike his fellow Ameri-

cans, whether for his specialized calling or even for his mystical writings, De Brahm's story is a fascinating one never before told in such detail. De Vorsey has himself realized his modestly stated hope that the book will lead to a fuller discovery and appreciation of De Brahm's career in America.

WILCOMB E. WASHBURN  
Smithsonian Institution

DAVID JOHN JEREMY, editor. *Henry Wansey and His American Journal, 1794*. (Memoirs of the American Philosophical Society, Volume 82.) Philadelphia: the Society. 1970. Pp. xvii, 186. \$7.00.

Progressive Wiltshire clothier, rational Dissenter, Whig, antiquarian, and avid traveler, Henry Wansey was, as a friend said, "always on the fidget." He made the most of a two-month visit to the United States in the summer of 1794, reading about the country beforehand, posing questions to answer, and keeping a careful diary as he traveled from Halifax to Philadelphia. His powers of observation were aided by a fair and open mind. His findings, in the form of an unpolished diary, were published in 1796, followed by a second edition in 1798.

Wansey's business interests prompted his trip, and the principal value of his *Journal* lies in its description of the American textile industry. He concluded about Americans that, "with all their improvements, they must yet for a long time come to John Bull for his cloth, for at least half a century to come." The *Journal* also includes information and commentary on, among other things, ocean travel, the American landscape, commodity and land prices, pronunciation, climate, public figures, and prospects for the country.

Although often cited, the *Journal* was not reprinted until David Jeremy, curator of the Merrimack Valley Textile Museum, produced this incontestably definitive edition. The lengthy introduction acquaints the reader thoroughly with the man through whose eyes he sees the country. The editor has endeavored, with considerable success, to trace every reference in the text. His footnotes correct, explain, and amplify, sometimes unnecessarily. The text, containing perhaps 60,000 words, appears crowded out by the footnotes. There are also

maps, illustrations, a genealogy, bibliography, and index. The reader may feel he has to fight his way through all these aids to find the *Journal*. Nonetheless, by his exhaustive efforts, Mr. Jeremy, with the American Philosophical Society, has made this valuable account not only available, but presented it in a form immensely more useful than the original edition.

JOHN BORDEN ARMSTRONG  
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PETER P. HILL. *William Vans Murray, Federalist Diplomat: The Shaping of Peace with France, 1797-1801*. [Syracuse:] Syracuse University Press. 1971. Pp. ix, 241. \$8.50.

This book serves a dual purpose: it provides the first biographical study of William Vans Murray, a Federalist congressman and diplomat from Maryland's Eastern Shore, and a re-examination of the politics and diplomacy of the Quasi-War with France. Murray, who left insufficient material for a full-scale biography, played an important role in President John Adams's search for an honorable reconciliation with France. Peter P. Hill has used Murray's career as a vehicle for an account of that search.

As the American minister to the Netherlands, Murray became the key figure in the secret diplomacy that gave Adams the assurances he required before he would send his second diplomatic mission to Paris, this time to negotiate successfully an end to hostilities that had erupted into undeclared naval war after the XYZ Affair. Murray also served effectively as a member of the three-man commission that made the peace convention with the new French government of Napoleon Bonaparte and, alone, ably represented his country in the difficult exchange of ratifications at Paris before his recall by President Jefferson in 1801.

In a volume published in 1966 Alexander DeConde considered the politics and diplomacy of the Quasi-War, including Murray's role. Although Hill's work does not supplant DeConde's more extensive study of that subject, it is a useful supplement. By Hill's careful examination of Adams's motives and conduct, in which Hill draws upon Stephen G. Kurtz's observations about naval preparedness as an adjunct to Adams's diplomacy, the author adds

convincing touches to DeConde's portrayal of the president. By Hill's analysis of Murray's life and career, including his association with the Adams family, before President Washington appointed him minister to The Hague in 1797, Hill also helps to explain why Adams had such confidence in Murray as an agent of his policy of peace. In sum, in his biographical treatment of Murray, Hill has furnished a well-researched and well-written case study of one of those "Adams Federalists," as Manning J. Dauer called them, upon whom the president relied as he doggedly pursued his "middle course" amid partisan passions that could have driven him to the side of Britain or of France.

ARTHUR A. RICHMOND III  
U.S. Naval Academy

HASKELL M. MONROE, JR. and JAMES T. MCINTOSH, editors. *The Papers of Jefferson Davis*. Volume 1, 1808-1840. (Supported by the William Stamps Farish Fund. Sponsored by William Marsh Rice University and the Jefferson Davis Association.) Baton Rouge: Louisiana State University Press. 1971. Pp. xvi, 594. \$15.00.

To students of the Civil War era, few of the major editorial enterprises now under way are of as much interest as the papers of Jefferson Davis. Hitherto the only comprehensive edition of the works of the Confederate president has been *Jefferson Davis: Constitutionalist*, the ten-volume compilation edited by Dunbar Rowland in 1923. Though still valuable, Rowland's work does not measure up to modern standards of historical editing, and since its publication large numbers of new Davis letters have been discovered, notably those edited by Hudson Strode in *Jefferson Davis: Intimate Letters* (1966). Increasingly, historians have felt the need for a complete, accurate compilation that will permit them to study the complex mind and the troubled heart of one of the most puzzling figures in American history.

The publication of the first volume of *The Papers of Jefferson Davis* gives reason to hope that the need will be met. Though it covers only the thinly documented period of Davis's childhood, his education at Transylvania and West Point, his military experiences in the West, his tragic first marriage, and his resignation from the army, this installment of what is

projected as a twenty to twenty-five volume series includes hundreds of hitherto unpublished Davis documents. Just how this new material will affect interpretations of Davis's career is not easy to predict. The hortatory introductory remarks in this volume by Bruce Catton and Frank E. Vandiver make no attempt to evaluate the documents; and the editors, Haskell M. Monroe, Jr. and James T. McIntosh, have limited themselves to a brief discussion of technical problems of editorial method.

Even without a proper introduction, the documents themselves call for rethinking of problems in Davis's early career. Why was this Mississippi youth at the age of nine sent away to a Catholic school in Kentucky "without my mother's knowledge or consent"? Why did this boy, educated in the North at the nation's expense, so early espouse Southern and sectional sentiments? As a student at West Point he spoke scornfully of "the Yankee part of the corps," who were not "such associates as I would at present select." During the Nullification crisis he was prepared to resign his army commission "rather than be employed in the subjugation or coercion of a State of the Union." How much of Davis's success or failure as Confederate president was determined by the friends and enemies he made as a young officer in the West? As early as 1835 he became strongly attached to Lucius B. Northrop, whom Davis praised as "a very active and Efficient Officer"; thirty years later he was still trying to defend the incompetent Northrop, whom he had insisted upon naming commissary-general of the Confederacy.

If the documents in this first volume illuminate important facets of Davis's personality, the elaborate editorial apparatus that accompanies them resolves a number of disputed questions about Davis's early life. For instance, the editors seem to have settled the long controversy about the date of Davis's birth, which the Confederate president himself remembered incorrectly. Similarly they establish that, contrary to popular tradition, it was "most unlikely" that Davis had any part in the fighting during the Black Hawk War. In a courteous, unobtrusive way they have corrected dozens of errors, large and small, in the previously published versions of Davis's letters. Giving the source of each document reproduced, they have wherever

possible identified every person, place, and incident mentioned. A full chronology of Davis's early life and an elaborate Davis genealogy prepared by Kirk Bentley Barb add to the usefulness of this volume.

Just because this first installment of Davis's papers is so valuable, one could wish that it were more so. Unfortunately most of the 535 documents here reproduced with such loving care have not the slightest biographical or historical significance. Hundreds of the entries consist of monthly reports on the conduct of West Point cadets, of post returns from all the forts where Davis was stationed, of pay vouchers and registers of payments to officers, and of other similar, formal documents that should, at most, have been calendared. Most of the letters addressed to Davis in this volume deserve the same treatment; they consist largely of routine acknowledgments of correspondence by war department officials. Indeed, it would probably have been advisable to calendar most of the eighty letters by Davis himself, about two-thirds of which are brief routine notes transacting army business. It is hard to see how any future historian or biographer will ever need such documents as Davis's letter to William B. Lewis, dated September 27, 1833, which reads in full: "Herewith I have the honor to transmit to you an account current with vouchers covering the amount of funds acknowledged to be the U. States in my last recruitg. account."

The editorial apparatus attached to these letters is also unnecessarily burdensome. Of course it is useful to have every person mentioned in these pages identified, but is it necessary to have a four-hundred-word biographical sketch of Winfield Scott or a two-hundred-word précis of Martin Van Buren's career? The editors of *The Papers of Jefferson Davis* surely do not need to summarize the *Dictionary of American Biography*. Full identification of minor and obscure persons is more defensible, but too often Monroe and McIntosh are excessively detailed. For instance, the Indian trader Peter Pauquette, incidentally mentioned only once in this volume, rates a three-hundred-word footnote, bolstered by the citation of no fewer than nine sources.

Furthermore, the pages of this volume are cluttered by excessive notes. Not content with identifying each person when he is first men-



tioned, Monroe and McIntosh drop a footnote referring back to this biographical sketch when the same name appears in any subsequent document. Thus Peter Hagner, the third auditor of the treasury who acknowledged many of Davis's reports as quartermaster, is identified in footnotes no fewer than twenty-two times. Since this volume has an admirably complete and accurate index, there is no reason for such a superfluity of cross-references.

Behind these criticisms lies a sense of urgency on the part of the practicing historian, who sees all too many of our major editorial enterprises bogged down in trivia and enmeshed in technicalities. If publication of scholarly editions of the papers of notable Americans is to be completed within the lifetime of any subscriber to this issue of the *American Historical Review*, the editors must begin to move with greater speed and with greater selectivity.

DAVID HERBERT DONALD  
*Johns Hopkins University*

MICHAEL P. CONZEN. *Frontier Farming in an Urban Shadow: The Influence of Madison's Proximity on the Agricultural Development of Blooming Grove, Wisconsin*. (Logmark Editions.) Madison: State Historical Society of Wisconsin for the Department of History, University of Wisconsin. 1971. Pp. xviii, 235. \$10.00.

Blooming Grove, Wisconsin, is the township immediately east of Madison, and this book assesses certain aspects of the township's agricultural history between 1835 and 1880. As the title indicates, the author investigates the growing impact of the capital city on the farming township. He does so through a quantitative analysis of major economic factors from the time the federal government first sold the land until the last manuscript census was available. He considers such problems as land ownership and use, the types of people who settled and stayed on the land, the size and value of farms, kinds of production, and marketing. Not surprisingly Conzen finds that Madison consistently exerted significant influence on Blooming Grove.

The book reflects meticulous research, with numerous maps, tables, and graphs amplifying the text. Throughout the book the author's research design dominates the material, and the

mechanics of presentation protrude unnecessarily. There is no question that the study proves its points. It tests various theses related to the data and makes appropriate comparisons with agricultural developments in neighboring areas. In this way it makes a contribution in establishing the particularities of economic growth in a limited region. Against such concrete information further comparisons may be made.

My reservations about the book are two. The author assumes that the reader is completely familiar with the extensive technical jargon associated with Conzen's quantitative approach to historical analysis, and he does not bother to make logical explanations where they are needed. For instance, only on the next-to-last page does he explain "cohort analysis," a model he uses frequently in the book. More fundamental is the fact that for all the specificity of data and scientific treatment of economic development, the reader comes away with scant sense of the human beings who labored the earth of Blooming Grove. The study has scientific sterility, to be sure, but it evokes too little of the past.

WALTER RUNDELL, JR.  
*University of Maryland*

ERLING A. ERICKSON. *Banking in Frontier Iowa, 1836-1865*. Ames: Iowa State University Press. 1971. Pp. x, 183.

For almost three decades the people of Iowa struggled to create a safe banking system, defined by the author as "a sound paper currency." From the beginning of white settlement the attitude prevailed that credit was bad for farmers and honest men in general; banks, the agencies of credit, should not be tolerated. Four recognizably distinct periods with differing systems followed, including a period of eleven years in which all "banks of issue" were prohibited. The soundest paper currency was obtained after the establishment of the national bank system in 1863. Failure of earlier systems and the necessity for change resulted largely from the increasing complexity of the Iowa economy, a widespread demand for more rapid development, and the growth of a national rather than local market for both agricultural produce and purchased commodities.

In a detailed, clearly organized, and well-

written monograph Erickson presents the story in six chapters, followed by a brief, well-considered "overview" in which he relates the changes to state party politics: "Old school" Democrats (by one of the paradoxes of American historical vocabulary they are the "radicals") opposed all banks; Whigs favored an elastic currency regulated by a national bank; conservative Democrats stood between them. His general approach and conclusions are for the most part in line with the work of Bray Hammond rather than A. M. Schlesinger, Jr. and other pro-Jackson historians.

The study is based on extensive research in published sources and secondary works and an astonishing amount of manuscript material. The author suggests that his generalizations "are probably true for a number of other frontier states." This may well be so, but the scene is far different from that shown, for example, by William H. Brantley for frontier Alabama. The genre of state banking history, although well established and perhaps somewhat rigid, is far from exhausted. Interesting illustrations, excellent footnotes, a series of appendixes, a bibliography of over three hundred items, and a dependable index complete this valuable work.

HARRY R. STEVENS  
Ohio University

DONALD JACKSON and MARY LEE SPENCE, editors. *The Expeditions of John Charles Frémont. Volume 1, Travels from 1838 to 1844*, and map portfolio. Urbana: University of Illinois Press. 1970. Pp. xlv, 854; map portfolio, pp. 16, 5 maps. \$22.50 the set; map portfolio only, \$10.00.

Anyone who ever read an American history textbook should be remotely familiar with the glamorous John Charles Frémont, and most of those who have gone beyond the survey level can appreciate his contributions as an explorer and map maker at a time when thousands of migrants, promoters, and politicians were hungry for information about the West. With the possible exception of the president of the United States, few Americans were better known in the early 1840s. But like most men dramatically elevated to the status of a hero Frémont turned out to be a mere mortal with feet of clay. His rise to fame following the 1842

expedition to South Pass and the one a year later to Oregon and California was meteoric, but almost everything attempted after 1847 in the fields of finance, exploration, politics, and military leadership seemed to end in disappointment, failure, or outright disaster.

The first of the projected three volumes of Frémont's expeditions probably will not make the best-seller list, but any collection of Western Americana will be incomplete without this very attractive publication. Even more handsome is the accompanying *Portfolio*, which contains the famous 1843 map by Joseph N. Nicollet of the hydrographical basin of the upper Missouri. The most significant of the remaining four facsimile reproductions is the Frémont map of 1845. This detailed work was compiled with the help of Charles Pruess and soon became the base for subsequent explorers to expand the boundaries of cartographic knowledge of the Far West.

Volume 1 in the current series presents a total of 137 thoroughly annotated documents, most of which are hitherto unpublished letters. Interspersed with these are extensive selections from Frémont's *Memoirs*, which only carry the story of his life to 1847 and his ill-fated appointment as governor of California by Commodore Robert F. Stockton. The principal events of his remaining forty-three years were faithfully chronicled by his adoring Jessie, but the sequel was never published. The well-written biographical essay included by the editors in the introduction to *The Expeditions* is disappointingly sketchy. For however Frémont's character was flawed by vanity, his monumental successes and failures have never made dull reading.

The various documents in the first volume reveal the explorer's wide range of scientific knowledge, and the carefully selected excerpts from the *Memoirs* are still fascinating to read. In comparison with the turgid style that characterized much exploration literature of the early nineteenth century Frémont's writing remains a model of clarity and smoothness. On the other hand, the many "tables of meteorological observations" are not nearly as exciting to read today as the New York City Telephone Directory. But to have excluded such valuable scientific information from this publication, especially after Frémont's long and painstaking

efforts in collecting it, would have been a serious omission.

W. EUGENE HOLLON  
University of Toledo

NANCY NICHOLS BARKER, translated and edited with an introduction by. *The French Legation in Texas*. Volume 1, *Recognition, Rupture, and Reconciliation*. With a foreword by JOHN CONNALLY. Austin: Texas State Historical Association. 1971. Pp. 357. \$12.00.

In the spring of 1838 at the request of the Texas agent to Britain and France, James Pinckney Henderson, the French Foreign Ministry began consideration of the recognition of the Republic of Texas. In September a young clerk in the French embassy in Washington, known as Alphonse DuBois de Saligny, was instructed to make a tour of inspection in Texas. He arrived in February 1839 and left in May for Europe, recommending strongly that France grant recognition to Texas. A treaty of recognition was signed on September 25, 1839, and the following month Saligny was named chargé d'affaires in Texas. He reached Houston in January 1840 and took up the duties of France's representative in Texas, which he was to manage intermittently until the annexation of Texas to the United States.

This volume is a record of his correspondence from May 1838 to August 1842. A projected second volume will cover the remaining period. The source of the letter is the archives of the French Foreign Ministry: nine bound volumes of political correspondence and one of commercial correspondence. Dr. Nancy Barker, the editor of this work, undertook the no doubt agonizing task of translating these from microfilm copies. A specialist in French history, she is unusually well equipped to explain the European nuances and overtones in the correspondence, and she has done a creditable job of handling the Texas angles, although here and there a few relatively unimportant weaknesses can be found. The two-volume work, when finished, will contain approximately half of the total correspondence. The editor has selected letters and passages from letters that seemed to be the most significant and has included brief abstracts of the omitted portions.

An even greater contribution than her anno-

tated translations, however, is her praiseworthy introduction, which traces the life of Saligny and puts him and the French legation in proper perspective. Until this work relatively little was known about the man who styled himself a count and stalked pompously on and off the stage of Texas history. Dr. Barker shows that he was born plain Jean Pierre Isidore Alphonse Dubois, that he was frequently untrustworthy, that he was something of a rascal, and that his letters and reports from Texas were unreliable.

It is this very lack of reliability that makes one wonder if this monumental work was really worth the doing. I would have much preferred to read a scholarly synthesis by Dr. Barker of Texas-French relations.

The whole effort to produce this and the volume to come appears to be some sort of special cause. The French ambassador personally presented the microfilm to the Austin Public Library; John Connally wrote a nice but needless forward; the volume is handsomely designed and beautifully printed and bound—at a production cost of probably twice the usual; and volume 1 was released with a grand fanfare but, naturally, without index or bibliography.

SEYMOUR V. CONNOR  
Texas Tech University

HENRY B. COMSTOCK. *The Iron Horse*. New York: Thomas Y. Crowell Company. 1971. Pp. 228. \$17.50.

A long century ago the sound of the locomotive whistle was one of the more exciting sounds in America. Not only were railroads then playing a vital role in our national development, but more specifically the steam locomotive was wooing youngsters from both farm and town toward a new-found interest in mechanics and the practical arts. *The Iron Horse* carries the history of the steam locomotive in our country from Colonel John Stevens's midget engine, built and run on his Hoboken, New Jersey, estate in 1825, to the 604-ton, 133-foot "Big Boy" built for the Union Pacific in the early months of World War II.

In the century and a third between the first American locomotives and the full acceptance of the diesel, about 180,000 steam locomotives were built in the United States. As the author

traces the early history of railroad motive power he presents much new biographical material about such early engine builders as Peter Cooper, Horatio Allen, John B. Jervis, Isaac Dripps, Matthias W. Baldwin, Henry R. Campbell, William Norris, and Ross Winans. Well before the Civil War locomotive builders had solved the basic mechanical problems of steam locomotion and had added such functional accessories as oil headlights, sand domes, bells, whistles, and cabs. As the growing transportation needs of the nation demanded ever larger locomotives and the introduction of such features as new wheel arrangements, "three-cylinder compounds," and the articulated Mallet, most railroad mechanical departments still were insisting on their own individualistic whims in engine design. Only during the days of federal control in World War I was there any degree of standardization in locomotive design and construction. The basic decision of early diesel manufacturers to standardize their product no doubt hastened the acceptance of this new form of motive power in the mid-twentieth century.

Drawing upon his long experience as a technical illustrator the author has enriched his text with more than two hundred drawings and illustrations. Much of the rich anecdotal material Comstock has included has not previously appeared in the standard secondary histories of the industry. A ten-page glossary of terms is helpful, but the bibliography is very brief and does not even include the valuable 1968 work on locomotives by John H. White, Jr. But the faults in this volume are few. Even though primarily written for the railroad buff *The Iron Horse* can be a valuable volume for any student interested in the technical and industrial history of the United States.

JOHN F. STOVER  
Purdue University

MORRIS F. TAYLOR. *First Mail West: Stagecoach Lines on the Santa Fe Trail*. Albuquerque: University of New Mexico Press. 1971. Pp. 253. \$10.00.

The history of the Santa Fe Trail before the Mexican War has long been familiar through the work of the contemporary historian Josiah Gregg and that of subsequent writers who have

drawn upon and supplemented his *Commerce of the Prairies* (1844). It is not generally realized, however, that the Santa Fe Trail continued to be of great importance until 1880 when the services of the Atchison, Topeka and Santa Fe Railroad rendered passenger, freight, and mail service over the old trail obsolete and unprofitable. Professor Taylor has filled a definite gap in knowledge by providing a history of the thirty-year period from 1850 to 1880 during which the Santa Fe Trail continued to be a vital link in the transportation system of the far southwest.

The history of stagecoach and mail service in these years is presented with the easy authority of one who knows his subject as the author unfolds the story of Waldo, Hall and Company, Hockaday and Hall, Hall and Porter, the Santa Fe Stage Company, and finally, Barlow, Sanderson and Company. The material for the work has been gleaned from a great variety of scattered contemporary and primary sources and has been blended with skill to form a connected account.

Among the points made by the author the following may be of interest. In a sense, the Santa Fe Trail may be viewed as a continuation of the National Road. A large number of pioneer Jewish merchants participated in the trade. After William Bent abandoned it in 1849 Bent's Old Fort was for many years used as a stagecoach station.

A good map and a few interesting illustrations are provided. The index and bibliography are excellent. The University of New Mexico Press has furnished a handsome cover and jacket, and both printing and paper are of a quality that matches the research and narrative skill of the author.

HARVEY L. CARTER  
Colorado College

H. CRAIG MINER. *The St. Louis-San Francisco Transcontinental Railroad: The Thirty-fifth Parallel Project, 1853-1890*. Lawrence: University Press of Kansas. 1972. Pp. viii, 236. \$8.50.

The purpose of the St. Louis-San Francisco Railroad was to establish a snow-free route to the coast that would have an advantage over other railroads in tapping the vast commerce that was expected to develop between San

Francisco and the Asian countries. The building of a railroad along the thirty-fifth parallel was a project that became involved in many financial, legal, and competitive problems. The project suffered from faulty administration, labor troubles, Indian disputes, and the disruption brought on by the Civil War. Although involved in many construction problems the Frisco eventually became a successful railroad and has become a good investment for the stockholders. The road had several changes in name—the Southwest Branch Railroad, the Southwest Pacific Railroad Company, the Atlantic and Pacific Railroad, the South Pacific Railroad Company, and the St. Louis and San Francisco Railway Company.

Writing the history of some railroads has been difficult because of a lack of records. Professor Miner, as a result of persistent research, found newspapers, government documents, diaries, and corporate records. Some of the material was located in old railroad warehouses and basement vaults of corporate office buildings. The book is illustrated with two maps and sixteen pictures of locomotives and persons connected with the railroad.

JOHN H. KRENKEL  
*Arizona State University*

NORMAN E. TUTOROW. *Leland Stanford: Man of Many Careers*. Menlo Park, Calif.: Pacific Coast Publishers. 1971. Pp. xiii, 317. \$9.95.

Leland Stanford, as the subtitle of this book suggests, had interests other than railroading. But it was in the business of transportation that he made his money; and, like many another nineteenth-century tycoon, he spent in a manner that both interested him and at the same time afforded him a claim either to philanthropy or the advancement of learning. These endeavors, in turn, earned for him sufficient prominence that his adopted State of California accorded him the traditional honor of a place in the U.S. Senate. There was even a brief "Stanford-for-President" boom as a final accolade.

In an effort to describe the usually neglected aspects of Stanford's career the author has compartmentalized Stanford's various enterprises, giving a good deal of attention to some of the lesser-known portions of it. For example, a chap-

ter is devoted to the magnate's role as a country squire: he invested a great deal of money in horseflesh, developed some prizewinning trotters, and gained a name as a breeder of superior animals. In his efforts to learn more about the mechanics of trotting Stanford spent considerable money in setting up a battery of cameras designed to record each motion of a moving horse. As a result he contributed to what would later be the motion picture industry.

Another of the railroad prince's efforts was to capture the title of Wine King of California, and into that enterprise he poured money with a reckless abandon. Some nine million dollars later the gentleman farmer was forced to recognize that money could not buy everything; the quality of his wine fell short of the anticipated goal. He then turned to brandy and in turning it out at a rate of 1,400 gallons a day could claim title to the world's largest distiller of grape brandy.

As a very rich man Stanford followed the path of his nineteenth-century fellows, traveling abroad in search of culture, dabbling in politics, and building mansions befitting of his position, but in founding a university as a memorial to his lost son he gained a peg or two on the others, many of whose names are today relatively obscure. In reviewing his subject's career the author candidly admits that the man "hovered on the edge of greatness, but never quite made it." Agreed, but one has to admit that in spending his money Stanford did a better job than a great many of his contemporaries. And in telling us about it, Professor Tutorow has made a contribution.

ROBERT G. ATHEARN  
*University of Colorado*

*History of the Supreme Court of the United States*. Volume 6, *Reconstruction and Reunion, 1864-88*. Part 1. By CHARLES FAIRMAN. (The Oliver Wendell Holmes Devise.) New York: Macmillan Company. 1971. Pp. xix, 1540. \$30.00.

This book deserves, indeed requires, a category more than a review. In its 1,500-plus pages Professor Fairman has created the equivalents of a half-dozen ordinary monographs; a useful collective biography of Chief Justice Salmon P. Chase and his brethren; several legal treatises; a substantial documents-case source book

blessed with rich headnotes; and, in the extensive footnoting, a treasury of bibliography, comment, criticism, lore, and wisdom. Yet this is only the first of two parts—i.e., volumes—allotted to Fairman for the Reconstruction period, in the eleven-volume Supreme Court history underwritten by the devise of the late Oliver Wendell Holmes, Jr.

I wish that Fairman had reached print a bit sooner. Literally on the day after I mailed off to a publisher my manuscript on Civil War and Reconstruction constitutional impacts, the review copy of Fairman's *Reconstruction and Reunion* came in. Available earlier, its information and judgments would have greatly eased my way.

This almost-immovable feast will guide and inform generations to come; any serious collection or library worth using will have a copy. Determined readers, blessed with good eyesight and sturdy bookrests, will exploit it as a standard by which to measure others' more modestly conceived efforts. Where else can one find appreciation of the 1867 Bankruptcy Act, or two hundred pages of minutely detailed coverage and analysis of mid-nineteenth-century municipal bond controversies, so long forgotten by other constitutionalists that our best present textbooks in that specialization ignore even the leading case, *Gelpcke v. Dubuque* (1863)? It is essential to understanding of Reconstruction alternatives to note, as Fairman has, that Lincoln's contemporaries were almost as vitally concerned about debt repudiations by Northern communities as about Southern state secessions and restorations; after Appomattox, more concerned. This duality of concerns was so deep among law men at least that the United States Supreme Court reporter saw fit especially to note in the preface to the 1863 *Reports* how, in holding *Dubuque* to payment, the national Court was preserving essential morality that Iowa had failed to nurture. As Reconstruction matters worked out, this tenacious dualism was to prevent Republicans from attending with full hearts and minds to Southern questions, as *Dubuque*-like Northern concerns gradually took precedence.

Similarly, it is basic to useful insights into the Supreme Court's Reconstruction history to understand legal-judicial procedures and the technicalities of pleadings, the mysterious sci-

ence by which ostensibly workaday private interests and relationships became transformed through litigation into public law. Professor Fairman, the distinguished biographer of Justice Miller and the worthy contender against Justice Black on the question of whether the Fourteenth Amendment carried national Bill of Rights constraints against states, is one of the few scholars who could have coped with the task. His *Reconstruction and Reunion* is a monumental achievement.

But, the question persists, achievement in what? Constitutional history? If so, then Professor Fairman's definition of the field is the widest-ranging since 1902, when Francis Thorpe, asking "What is a Constitutional History of the United States?" (*Annals of the American Academy of Political and Social Science*, 19 [1902]: 259-65), suggested that its sources included national and state constitutions, derivative statutes, legislative records, and, of course, the "mass of authoritative exposition by the courts." Untroubled by the mountains of printed data included in his free-swinging definition, Thorpe asserted that "the entire testimony bears back to principles of government, few in number and comprehensive in character." And, he concluded, "A constitutional history is the narrative of the apprehension and application of these principles by the American people" (p. 259).

Thorpe's *fin-de-siècle* confidence concerning basic constitutional principles failed to inspire derivative histories of those principles or of their primary expounder, the Supreme Court. During ensuing decades raiders from present-minded political science and law departments on campuses so transformed constitutional inquiries that by 1963 Professor Paul Murphy pleaded in this journal that it was "time to reclaim" the specialty for history and historians ("Time to Reclaim: The Current Challenge of American Constitutional History," 69 [1963-64]: 64-79).

Professor Fairman's *Reconstruction* volume will advance the ongoing reclamation if historians are not misled by its encyclopedic size and comprehensiveness into assuming that the book is constitutional history's final massive marker. Fairman labored to praise constitutional scholarship not to bury it, to augment understanding not to enlarge obsequies, and to loose constitu-

tional history in Clio's many-roomed house not to constrain it.

In some instances Fairman's freedom to include great data masses enriched the whole. Publishing economics may make this the last time that an author, impressed with the passionate quality of scholarship on a point under consideration, felt free to employ, fairly casually, more than a dozen pages for quotations of pertinent contemporary newspaper comment (pp. 343-55). In many others less inclusiveness and far tighter editorial control would have greatly improved the book's quality and utility. In search of rich lodes, one must push through many pages of inappropriate impediments. As examples, Fairman includes descriptions of the Court's seating arrangements; the floor plan of its chambers, statuary, and other decorations; robing-room situation; and members' reading and work habits. The filing systems the Court clerk and marshal developed receive attention. One can defend the unstinted detail Fairman employed on the basis that technical legal procedures are often the heart of substantive constitutionalism, ignored only at peril. But procedure is not trivia.

Fairman's *Reconstruction* sins only in minor terms. Often provocative, insightful, and thoughtful, it marches generally in step with the impressive recent reconsiderations of men and measures of Republicans during the Reconstruction era, yet fails to analyze adequately the aspirations, alternatives, and achievements of the framers and ratifiers of the Thirteenth through Fifteenth Amendments and derivative legislation.

This being so, I wonder why, considering Fairman's policy (p. xix) deliberately to keep references sparse, he troubled to cite Bentley's defective Freedmen's Bureau study and failed to note Kutler's more useful inquiries into Court numbers, *ex parte McCardle*, and Reconstruction politics? That Justice Miller should have understood Negroes because he was a Kentuckian (p. 124) is a quaintly antique suggestion. In light of Fairman's sensitivity to the national Court's bootstrapping role concerning Iowa's remissness in failing to check Dubuque, I wonder at his flat reaction to the high judges' equally adventurous jurisdictional stand concerning Missouri's test oath constitutional requirement (pp. 244-45). I regret the

curiously thin treatment Fairman gave to the Johnson impeachment (pp. 521-27); its constitutional and legal history rates far fuller attention. Fairman's decision was unwise to wait for pages 334-43 before offering lucid discussion of Reconstruction options and alternatives available in the 1860s to national policy makers in light of existing institutions, resources, and limitations—constitutional and otherwise. That sage analysis deserved front-of-the-book space.

But an imperial adventure deserves consideration on its terms not by a carper's tight horizons. I welcome, though I am troubled by, Fairman's *Reconstruction and Reunion* and wait eagerly its promised companion "part."

HAROLD M. HYMAN  
Rice University

ROBERT HIGGS. *The Transformation of the American Economy, 1865-1914*. (The Wiley Series in American Economic History.) New York: John Wiley and Sons. 1971. Pp. xv, 143. \$5.95.

In recent years economic historians, old and new, have improved in substantial ways our knowledge of the early nineteenth-century American economy. Most of this research has been guided by an interest in explaining the origins of rapid industrialization and of the growth of income that was one of the most attractive aspects of that complex historical process. When and why, scholars have repeatedly asked, did that process begin? So intent have they been on answering these questions that they have given little attention to later, equally important economic developments. As a result, we now know more about the workings of the economy in the twenty years before the Civil War than in the several decades that followed 1865. The appearance of Robert Higgs's study is therefore welcome, especially insofar as the volume is likely to arouse new interest in the modern American economy.

In this slim book the author gives us a series of discrete essays in search of a synthesis. After explaining his analytical framework, which is based on neoclassical economic theory (with a few special flourishes), Higgs attacks the problems of relating urbanization, health care, invention, and information exchange to the country's rapid growth. There is a brief digression on the antebellum years, a section in which the author seems to follow Stuart Bruch-

ey's treatment of the importance of property rights without, curiously, mentioning Bruchey. Higgs also explores the increased productivity and multiplying problems of the farmer, and he concludes with an examination of America's unequal distribution of wealth and welfare.

The book is well written and is clearly intended for historians, not economists. Any reasonably intelligent reader will encounter no problems in following the author's analyses. Indeed much of the book is elementary and familiar; historians will not be too surprised when they read about the end of the frontier and the rise of the city in the years 1865-1914. More novel are the author's comments on the sources of increased productivity and the economic significance of improved health care and of new means of transmitting information. These are subjects that will doubtless receive considerable attention in the future, and Higgs will be responsible for the direction this research takes.

For the present, however, his treatment of these and other related problems is mildly disappointing. All too often the author follows an elaborate explanation of hypothetical consequences with an admission that the evidence needed to test his ideas does not exist. In other cases the available data seem to indicate that the most important effects fell outside of the book's time period. Instead of this kind of particularistic probing, what we obviously need is a synthesis—something comparable to Douglass North's volume on the period 1790-1860; but all that Higgs gives us is a theory grounded in the assumption that America had a relatively efficient market economy and that static theory therefore provides the best means of analyzing change. Having admitted that the market worked rather well, however, we still need to know what the major determinants and long-run consequences of economic growth were. Numerous scholars have suggested that some dramatic changes took place in the economy around the end of the nineteenth century. Unfortunately Higgs has not addressed himself to the questions they have raised, and we can only hope that his book will be followed by another that will apply the author's substantial talents to this more challenging task.

LOUIS GALAMBOS

*Johns Hopkins University*

SHERRY H. OLSON. *The Depletion Myth: A History of Railroad Use of Timber*. Cambridge, Mass.: Harvard University Press. 1971. Pp. xvi, 228. \$9.00.

One hundred years ago and for many decades thereafter presumably knowledgeable persons predicted an imminent and inevitable "timber famine" in America. In one of her pungent sentences Mrs. Olson states that "the crisis never came to pass" (p. 2). The precise reasons why it did not come to pass have never been this clearly analyzed. The author, who teaches in the department of geography and environmental engineering at Johns Hopkins University, hews straight to the mark and clears away hoary misconceptions and confused thinking based on plain ignorance of the facts. She says of her analysis that "it differs critically from the classic fairy tale of American history and the traditional foresters' version of forest history" (p. 7). That it does.

Since the railroads once consumed from one-fifth to one-fourth of the nation's annual timber production, she presents data concerning railroad use of timber as a case history to show exactly what happened and why and how expert and popular opinions concerning timber depletion led to misdirected and costly public and private policies. She reviews the whole gamut of forest policy, sparing not even the revered Gifford Pinchot, whose advice concerning "good forestry" was too dogmatic and based on the European experience with silviculture and fire protection. Far more attention should have been given to wood utilization for, "Use alone can create value." Conservationists erred in confusing the growing of trees with the economic facts of supply and demand.

The incisive and valid conclusions reached are backed by impeccable analysis of abundant data, well documented. This diminutive but significant book contains eight illustrations, twenty-six figures (maps and graphs), and five tables. It offers a fresh approach to an important subject, and the findings can offer guidelines for anyone concerned with current problems of resource management, whether animal, vegetable, mineral, soil, or water. It is hoped that the book will become available in paperback form at a much reduced price.

FRED W. KOHLMAYER

*Illinois State University*



ALBRO MARTIN. *Enterprise Denied: Origins of the Decline of American Railroads, 1897-1917*. New York: Columbia University Press. 1971. Pp. xiv, 402. \$10.95.

Professor Martin believes that the physical and financial demolition of the American railroad system was begun in the years 1906-17 by "archaic Progressives" who were in turn manipulated by organized labor and industrial and agricultural shippers. He does not mince words: "What I have to relate is a story of the most intense frustration of the human spirit; of the brutal substitution of petty consistency for a sensible pragmatism; of the unconscionable elevation, by the government of a republic, of one set of interests over another and over the general welfare; and of a self-serving and all but cowardly refusal to face public duty." His heroes are the frustrated managers of such roads as the Pennsylvania, the Illinois Central, and the Santa Fe; his devils are the misguided initiators and inept administrators of public policy during the Roosevelt, Taft, and Wilson administrations.

The thesis of this book is clearly stated: Following the reorganizations of the 1890s, the fundamental need of American railroads was for investment capital to enable them to rebuild and modernize. Among those roads that accounted for "most of the tonnage and passengers, if not the total route mileage," an "advanced technology and an amazingly vital railroad management stood fully ready." But after 1906 public policy became increasingly unresponsive to the needs of these strong, soundly managed roads in that it fomented high labor costs and refused to grant general rate increases. The result was that the flow of investment funds available to the railroads did not keep pace with demands placed upon them. This combination of a cost-price squeeze and a lag in new investment caused profits to disappear after 1911 and paved the way for the eventual collapse of the the system of private management of the railroads. In this scenario the Hepburn Act of 1906 and the Mann-Elkins Act of 1910 appear not as milestones in the government regulation of economic activity but as the gravestones of responsible private enterprise.

The proof of this thesis rests upon two quantitative estimates and a qualitative appraisal of

the public policy options available at the time. Professor Martin begins with a comparison of the total annual output of freight and passenger services ("demands on the system") with the annual net new investment in railroads ("the absolute volume of national resources going to increase the capacity and efficiency of the system") in the base period 1905-07. He then computes, year by year for the period 1898-1915, what the annual net new investment would have been if it had been of the same magnitude in relation to the base period as was transportation output. These estimates lead him to the conclusion that the relationship between the trendlines of railroad transportation output and net annual new investment was "rational" in the 1898-1906 period and "irrational" thereafter. In short there was after 1906 a very large "annual and cumulative deficiency" in the flow of funds into the railroad system.

These calculations are justified in an eleven-page appendix, but it would be a mistake to consider them either the last word on the subject or the bedrock of Professor Martin's case. The major burden of his argument is found in his text, which seeks to prove that public policy makers understood these economic relationships and their implications, had alternatives available to them that would have benefited the railroads and the nation, but consciously chose and implemented policies that had opposite and detrimental effects.

Professor Martin has filed a brief seeking an indictment for conspiracy against the public officials of the Progressive era. He argues his plea with gusto and conviction. To this member of the audience his evidence still remains circumstantial and inferential. If one believes in such conspiracies, one will find Professor Martin's evidence admissible and his conclusions irrefutable. If one does not believe in such conspiracies, one must render a Scotch verdict at best. But one must admire the diligence and skill with which Professor Martin builds his case. He is certainly the most able advocate the railroads have had, then or since.

JAMES P. BAUGHMAN  
Harvard University

JOHN B. RAE. *The Road and the Car in American Life*. Cambridge, Mass.: MIT Press. 1971. Pp. xiv, 390. \$12.00.

In an age of often misguided criticism and rejection of industrial society, Professor Rae's study defends the automobile-highway system by looking at its history. On sturdy democratic grounds he rejects the idea of banning cars in Yellowstone even though they take two million tourists there each year with destructive effects. He takes a negative view of user taxes or of adopting highway revenues for other uses (a "breach of faith" with road-users and a form of discriminatory taxation banned by some states). He finds that new highways raise land values, though some people suffer (the depredations of "eminent domain" are not described). "Planned obsolescence" is "an overworked expression"; city congestion existed before the automobile; and the urban freeways take up less street space than conventional roads (despite exaggerated claims of how much of Los Angeles the car consumes). The car has brought a disastrous decay of public mass transport and has accelerated suburbanization. Professor Rae gives a needed argument in favor of the much-maligned suburbs of today. Above all, the gist of his argument is that the automobile-highway system exists because the market society chose it. People want it—here and in other nations.

Having a point of view helps the author to pull together this interesting and clearly written study of the interaction of the mass-produced car and the hard-surfaced, high-speed highway in recent history. Extending his study of 1965 (*The American Automobile*), his first eighty pages summarize familiar historical facts, while the bulk of the book concerns events since 1930. Rae touches briefly upon many topics, such as the role of highways in ancient empires, English and French road-building in the seventeenth and eighteenth centuries, turnpikes, and the eclipse of highways by railroads. Here readers will be reminded of Robert Fogel's more technical argument on the hidden costs of railroads; we see how railroads choked off facilities—good roads, inns, restaurants.

After 1900 people knew how to build roads well; they did not do so. The bicycle mania of the 1880s and 1890s got people out on the primitive dirt highways and created a road lobby (groups like the League of American Wheelmen, described already by Philip Mason). Farm pressure for local market roads and the auto-

mobile brought the real beginnings of state and national legislation. Rural Free Delivery (general by 1900) also helped. Professor Rae traces the history of public highway policy from the first federal act of 1902. Major developments after 1930 were the pioneering "parkways" built by the states, the turnpike revival of the 1950s, the California freeways, and the interstate highways built under the Federal-Aid Act of 1956, which will cover the nation with 41,000 miles of superhighway.

Professor Rae is at his best in economic description and explanation; he is less concerned with the social impact of the new system, though he suggests its dimensions: the life-style based on the car ("Drive-in America"); the elimination of rural-urban distinctions ("Rurban" America); the greater opportunities and rootlessness of motorized Americans. Though he describes the enrichment of rural life there is still more to be said about this "flexible, multiple-use vehicle," this private room on wheels, that has given Americans such cheap personal mobility. Its heritage has been ambiguous.

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BARBARA W. TUCHMAN. *Stilwell and the American Experience in China, 1911-45*. New York: Macmillan Company. 1971. Pp. xv, 621. \$10.00.

Barbara W. Tuchman has brought her considerable talent and audience to the subject of American-Chinese relations at an important time. This work, like her earlier books on the balance of terror in World War I European diplomacy, will have a wide impact on popular perception of present problems.

Despite, or because of, its broad compass, this mass-market historiography breaks certain rigorous research rules. Mrs. Tuchman does not branch out far from the papers of public and visible men. She has not delved deeply into scholarly, periodical literature nor explored exciting interdisciplinary suggestions about the professional, military aristocracy into which her subject fits. Finally, she is too trusting of secondary materials on many of her period's leaders and events.

Mrs. Tuchman is far too skillful at her genre to dwell on these problems inherent within it.

She culls so much from the sources she uses, paints such a vivid canvas, and fixes so well upon important conclusions. Further research, indeed, would probably not challenge the book's already well-quoted summation, "China was a problem for which there was no American solution."

In the context of these strengths and weaknesses in a book that will have a wide reading, it is important to examine its intent and ask basic interpretive questions about its significance. *Stilwell* is both a biography and a policy study. Its central figure is both a man and an archetype. As Theodore White's 1948 publication of *The Stilwell Papers* had already proven, General "Vinegar" Joe provided more than enough material, observation, participation, and disposition for reporter and biographer. He is a worthy match for the author's talents. She takes hold of the young Stilwell before 1900, his plebe year at West Point, and refuses to let go for nearly half a century through to the four-star general's receipt of his Combat Infantryman Badge a day before his death in October 1946.

Against this Stilwell setting Barbara Tuchman has chosen to trace the general outline of United States-Chinese relations in which the general played a major role as a military liaison officer in the 1940s. In the effort to capture the "American experience" in China in the light of one career, however responsive and representative, the author finds her most serious problems. No individual in either society, not even Stilwell, transcends effectively the period from the onset of Chinese revolution in 1911 to the end of Japanese war in 1945. Stilwell, the man, was, after all, just a bystander in the early years. *Stilwell*, the book, gets to the Sino-Japanese War (1937) by great leaps and bounds—27 of its 35 years in but 164 of its 531 textual pages. One must question whether the jump from Yuan Shih-K'ai in 1911 to Chiang Kai-shek in the 1940s, or from William Howard Taft to Franklin Delano Roosevelt, can be made with such dispatch. Given the book's concluding doubt about an American solution, it might be asked whether Chiang or Roosevelt or Stilwell, for all his idiosyncracies, were not acting out the last scenes of a drama foreshadowed in its less intense, earlier acts.

Even the hero's personal magnetism acts to

subvert rather than overcome the problems of generalization. Stilwell is so dynamic one almost forgets that there were other "attachés"—military and diplomatic, public and private, economic and religious—with other varieties of experience. Such characters pass through the broad, effective Tuchman panorama, but as passive participants in the Stilwell theater. They were, in reality, of course, the active agents of American material and culture. Given a historical and not biographical focus, Stilwell's experience is more a product of them than theirs is of him. The book's effort is a noble one, but it shows the need for more analysis of the component parts of American foreign policy. The shared assumptions and attitudes of all Americans in China gave United States policy its functional definition into which Stilwell's own role fit so well.

One critic concerned with the endless desert footage of a contemporary film classic commented he searched for more "Lawrence" and less "Arabia." The opposite is true here. We now have had sufficient Stilwell. We need to search about for more China, and indeed for more America if we are to go beyond biography to understand as well as describe the nature of "the American experience in China."

JERRY ISRAEL

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HAROLD SEYMOUR. *Baseball: The Golden Age*. New York: Oxford University Press. 1971. Pp. viii, 492. \$12.50.

The author of this book has a Cornell Ph.D., has taught college-level history for fifteen years, and displays many of the hallmarks of the serious academic scholar. He has put his book together in an orderly fashion (particularly in comparison with some sportswriters' books); and he discusses the sort of thing that is of interest to historians even if they happen not to be baseball buffs: organized baseball's commercial structure, its almost total authority over players, its cynical attitude toward a trusting public, and its reflection of the varied social and economic changes in the nation at large. Seymour handles all these themes skillfully, especially his analysis of the game's business operations, but he occasionally strains for

a pretentious academic posture that has the effect of making a mountain out of a pitcher's mound. He has directed this book to the general reader and has omitted footnotes; a bibliographical section at the end is generally helpful but leaves the reader uninformed as to the specific source for many of the author's most important statements.

Seymour is not only a scholar but a baseball nut: he was batboy for the Brooklyn Dodgers in the 1920s, played the game in high school and college, and has coached, umpired, and scouted. He was a youthful admirer, or at least observer, of the men he writes about here. Half memoir and half popular history, his book is vastly superior to the usual gee-whiz banalities of the daily sports page. Like most sports fans, Seymour is often nostalgic, especially about the goings-on at Ebbets Field, but he is never uncritical. He is that rare person, a batboy-turned-iconoclast, and his book is full of unsentimental revelations: baseball owners in the good old days were as greedy as they are now, Ty Cobb was psychotic, Babe Ruth was uncontrollably and childishly self-indulgent, the great 1927 Yankees achieved superiority by raiding the impecunious and approachable Red Sox for all their best players, the "Black Sox" of 1919 were indeed crooked but so were scores of other players who never got caught, and Judge Kenesaw Mountain Landis (the first baseball commissioner) was an arbitrary old martinet.

Seymour approaches his material with the raised eyebrow of the skeptic, not the open mouth of the credulous. "To call professional baseball a sport and the players and owners sportsmen," he writes, "is to use language so loosely as to divest it of meaning. Games staged for money are not played for fun." Baseball is really "a boy's game played by grown men for a living and run by promoters for a profit." But, he adds, it is after all "a cut above sticking barbs into bulls, or having two men batter each other in a ring until one drops unconscious."

Seymour has previously written *Baseball: The Early Years*, covering the late nineteenth century. A third volume on the recent history of the game is in preparation.

ROBERT L. BEISNER  
*American University*

CHARLES LARSEN. *The Good Fight: The Life and Times of Ben B. Lindsey*. Chicago: Quadrangle Books. 1972. Pp. xi, 307. \$10.00.

Ben Lindsey (1869-1943) was a rather simple man in basic intellect and character who had a highly complex career, being a juvenile court judge in Denver, a participant in Progressive politics, a voyager on the Ford peace ship, an advocate during the 1920s of changes in the institution of marriage, and a California Superior Court judge in the 1930s. An individual of great renown in his lifetime, Lindsey became unknown to the public and neglected by scholarship within a generation after his death. Charles Larsen's study, an authorized, but "not an official biography," attempts to reclaim Lindsey from undeserved obscurity and generally accomplishes its task, especially, for the post-1914 aspects of his career.

The earlier portions of the book are disappointing. The chapters on Lindsey's youth and the establishment and operation of the juvenile court offer little information or interpretation not in Lincoln Steffens's *Upbuilders* (1909) and the judge's own autobiographies. Larsen does not sufficiently place Lindsey's juvenile court work within a context of national trends, nor does Larsen discuss the weaknesses of this institution, a topic that has received much recent attention. Considerable space is given, however, to Lindsey's reflections on the 1912 presidential candidates. Yet, as the author admits, being judge of the Denver Juvenile Court was Lindsey's "chief role in life."

The book improves markedly as it proceeds and is particularly insightful on Lindsey's activities in the 1920s as a marriage and sex reformer. Larsen exaggerates, perhaps, in describing the judge as the "Prophet of the Jazz Age," or a "spokesman for . . . 'Flaming Youth,'" the more so since Larsen has demonstrated the moderateness of Lindsey's program. Here, as on the Ku Klux Klan skirmish, the 1929 disbarment, and the California comeback, there is more detail and analysis.

Larsen's laborious research is based on the immense, though inadequately organized, Lindsey manuscript collection in the Library of Congress. Larsen has also made good use of "the dwindling but helpful group" who knew the man. The overall perspective is favorable to Lindsey, but the biographer is aware of his

subject's limitations. The same perspective is applicable to the biography. It is a well-researched and well-written book that fills a gap in the bibliography of reform and will help restore Lindsey to importance, but it is a book flawed by its perfunctory treatment of the pre-World War I phase of his Denver career.

PETER GREGG SLATER  
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RICHARD B. HENDERSON. *Maury Maverick: A Political Biography*. Foreword by JOE B. FRANTZ. Austin: University of Texas Press. 1970. Pp. xxiii, 386. \$8.50.

"Two cowboys can ride herd a whole lot better than one," FDR once wrote to Maury Maverick in a sincere compliment to the aggressive, hard-driving little Texas congressman who had given the president's New Deal program a needed lift on more than one occasion. Maverick served only two terms in Congress, but he attracted more attention during his four years in the House of Representatives than many congressmen who have service records that span a score of years. But this was Maverick's style. As one of the leading members of the San Antonio Citizens League he helped oust a political machine described by one Texas politician as being capable of giving "Tammany Hall of New York lessons when it comes to holding crooked elections and counting candidates out who have been honestly elected." As an astonishingly liberal Southerner he organized a group of like-minded congressmen into a determined little voting bloc shortly after his arrival in Washington as a newly elected freshman congressman. After his defeat for re-election in 1938 Maverick won a term as mayor of San Antonio and did so much to modernize this beautiful but neglected city that John Gunther concluded that Maverick was "incontestably the best mayor San Antonio ever had." Even as head of one of the many wartime agencies, the Smaller War Plants Corporation, Maverick caught the nation's attention during World War II with his campaign against deadly bureaucratic jargon, coining the word "gobbledygook" to describe it.

Chronicling the colorful Maverick's political career has provided Henderson with a difficult challenge. Although asserting in his preface

that Maverick's family urged him to paint an objective picture of the outspoken Texan, warts and all, it is obvious that the biographer is much taken with his never-commonplace subject. Maverick's "volatile nature," as James A. Farley described it, had led Maverick to make some undeniably outrageous remarks, yet Henderson could almost always find excuses for the congressman's rough language, often employing such delicate phrases to describe it as "affectionate profanity" or "studied irreverence." But in truth Maverick was an attractive subject. An ardent New Dealer after President Roosevelt began to see that the days of his reform program were numbered, Maverick never weakened in his resolve and was willing to be a party maverick (his grandfather contributed the word to our lexicon) if necessary. Perhaps even more admirable was his devotion and effectiveness as a civil libertarian. Roger N. Baldwin, director of the American Civil Liberties Union, paid him the highest of tributes: "Among the handful of civil rights champions in Congress over the years, Maury stands out as the most devoted and the most skillful in achieving what he went after."

Henderson has written a compelling and well-documented monograph. He might have been more effective in relating Maverick with his times, thus achieving a smoother narrative, but the book is nevertheless a substantial contribution to the historiography of the Roosevelt-Truman era. An attractive volume with a section of photographs and a foreword written by Joe B. Frantz in his delightfully humorous style, Henderson's biography is also a fine reference book with a good bibliography and a functional index, always helpful to a historian in a hurry.

ROBERT W. LARSON  
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RICHARD M. FREELAND. *The Truman Doctrine and the Origins of McCarthyism: Foreign Policy, Domestic Politics, and Internal Security, 1946-1948*. New York: Alfred A. Knopf. 1972. Pp. xii, 419, xii. \$10.00.

This book, originally a doctoral dissertation under the direction of Gabriel Kolko, is an interesting and somewhat distinctive New-Left interpretation of the genesis of McCarthyism.

A scholarly polemic of a nature currently fashionable, it will, I suppose, be praised and damned in accord with already established views on the school of thought it represents. But the book is sufficiently provocative to merit serious consideration on its own and should be read by anyone interested in the cold war and American civil liberties.

Professor Freeland's thesis is that after the war the Truman administration became convinced that massive economic aid to Western Europe was necessary to develop American economic and strategic interests (largely the former) on the Continent. In order to build sympathy in Congress and throughout the nation for the Marshall Plan, administration officials mounted an anti-Communist propaganda offensive that produced a rigidly anti-Russian foreign policy and inadvertently paved the way for Joe McCarthy. In one year, from March 1947 to March 1948, the attitude of the American people toward the Soviet Union shifted dramatically; the public debate on the Marshall Plan was the "main battle" in the forging of a cold-war outlook that plunged us into the hysteria of the early 1950s and continued with sufficient force to commit us to Vietnam. Singled out for special attention are the Federal Employee Loyalty Program, the attorney general's list, efforts to deport "subversive" aliens, administration support of the House Committee on Un-American Activities, the release of various diplomatic documents that appeared to reveal Soviet hostility and imperialism, and the dispatch of the Freedom Train—a device that attempted, according to Freeland, to elevate the Truman Doctrine to the level of the Constitution.

Numerous efforts have been made, of course, to pin the blame for the cold war and McCarthyism on Truman, and this study suffers from some of the defects of its predecessors. It underestimates the role of Congress in the drive for internal security legislation, minimizes Truman's contributions to the defense of civil liberties, exaggerates the political uses of anti-Communism prior to Dewey's defeat, and, above all, makes little effort to describe those events at home and abroad that might have given Americans good reason to fear Communist aggression and subversion. Still, Freeland raises numerous questions about the Marshall

Plan and its advocates that need answers. And the extent of the author's research is to be applauded: he combed the Truman Library, the Library of Congress, and other archives for relevant materials; interviewed Clark Clifford; and read widely in the literature of the period under examination. Much of the evidence needed to prove anything conclusively about the events covered in this book remains beyond the reach of scholars, but Professor Freeland is to be congratulated for posing a challenging thesis that should stimulate historians for a long while.

THOMAS C. REEVES  
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GRAHAM T. ALLISON. *Essence of Decision: Explaining the Cuban Missile Crisis*. (Written under the auspices of the Faculty Seminar on Bureaucracy, Politics, and Policy of the Institute of Politics, John Fitzgerald Kennedy School of Government, Harvard University.) Boston: Little, Brown and Company. 1971. Pp. xii, 338. \$4.50.

LOUISE FITZSIMONS. *The Kennedy Doctrine*. New York: Random House. 1972. Pp. 275. \$7.95.

JAMES E. MCSHERRY. *Khrushchev and Kennedy in Retrospect*. Palo Alto, Calif.: Open-Door Press. 1971. Pp. 233. \$8.95.

Partisans of the tragically short Kennedy administration have often singled out the president's handling of the 1962 Cuban missile crisis as his finest hour in the field of foreign affairs. In the ten years following those thirteen days when the world's two most powerful nations "paused at the nuclear precipice," critics of the Kennedy mystique have raised serious questions about the necessity and wisdom of that policy. Only one of these books deals exclusively with the missile crisis, but all three, for quite different reasons, consider it the touchstone of Kennedy's foreign policy and a "major watershed in the Cold War."

Because of its analytical complexity Allison's book deserves a brief summary. Writing under the auspices of the Faculty Seminar on Bureaucracy, Politics, and Policy of the Institute of Politics in the John Fitzgerald Kennedy School of Government at Harvard, Allison describes the decision-making process during the Cuban missile crisis in the United States and the Soviet

Union by assigning three hypothetical analysts to interpret the event using three different conceptual models. First there is the classical or "rational actor" model, which is most often employed by diplomatic historians to explain foreign-policy decisions. It is based on calculating what rational, strategic choices would be made by key individuals for their nations without reference to any of the internal, personal, or bureaucratic machinations within their respective governments. Viewed from this conceptual frame of reference, the missile crisis, it is assumed, can best be explained in terms of value-maximizing choices like the following: the Soviets attempted to place defensive and offensive missiles in Cuba in order to obtain missile-power parity with the United States; the American government responded with a blockade as the logical middle course between doing nothing and invading Cuba; this in turn shifted the burden of further escalation back to the Soviets, who finally withdrew their missiles not because of the blockade itself, as most published accounts have claimed, but because it was coupled with the implicit threat of an air strike or land attack.

The second process model is one developed by organizational theorists and economists. It assumes that international politics is made up of organizational outputs that are invariably standardized and sluggish in times of crisis because they are based on routines (SOPs) for dealing with standard or normal day-to-day situations. From this vantage point, the actions of the United States and the Soviet Union do not have to be molded to fit a particular rationale. Therefore what started out to be either an attempt to bridge the missile gap or simply an attempt to defend Cuba (with the emplacement of defensive [SAMs] and offensive missiles [MRBMs]) was complicated and confounded by competition between the GRU, the KGB, the Air Defense Forces, and the Strategic Rocket Forces. Each agency did what it knew best to do without concern for internal consistency or adherence to the same security standards. Allison suggests it is possible that IRBMs, instead of only MRBMs, were installed by the Strategic Rocket Forces without Khrushchev's direct knowledge.

According to this second process model the United States chose to blockade Cuba out of

confusion arising over intelligence information because of competition between the CIA and the Air Force and because of a misunderstanding between civilian and military leaders over the effectiveness of a "surgical" versus a "massive" air strike against Cuba. Finally the Soviets withdrew their missiles, partially because of the confusion created by the few top Soviet officials who actually were aware of their emplacement and partially because actions by competing agencies created a contradictory situation in which the left hand of the USSR did not know what the right one was doing. So just as "Kennedy was beginning to wobble, Khrushchev folded" because of internal problems of communication and coordination.

The third process model describes the bargaining games played by top policy makers based on their unequal positions of power within governmental circles. According to this pluralist model foreign-policy decisions are the product of decentralized coordination (shared power) of the various pressures emanating from representatives of interest groups inside and outside of government. Using this conceptual approach Allison speculates that the Soviet Union placed its missiles in Cuba as the result of pulling and hauling among select members of the Presidium about which we do not as yet have enough public information for documentation. But there is already an abundance of this type of evidence for the United States in the published personal accounts of the meetings of the ExCom (Executive Committee of the National Security Council), which Kennedy created to deal with the crisis.

This third model questions whether the Soviet missiles in Cuba actually affected the nuclear balance of power and instead concentrates on the idea that, because Cuba was already a "political Achilles' heel" as a result of the Bay of Pigs fiasco, practically all of the influential forces represented in the ExCom pushed Kennedy in the direction of strong action. From the beginning, therefore, there was little chance of negotiating through straight diplomatic channels, especially in view of the argument that such normal diplomacy would allow time for the missiles to become operational. Thus, the blockade was really a "collage" decision, arising less out of organizational sluggishness and confusion and more out of in-

tense bargaining and pressure tactics within the ExCom. Likewise, the Soviets possibly decided to withdraw not because of the blockade or because of the subsequent ultimatum, but because of a tacit sense of partnership and mutual consideration between Kennedy and Khrushchev, which led them to establish direct lines of communication to avoid misunderstanding each other's intentions. This ultimately led to a private deal between them, which Robert Kennedy personally conveyed to Soviet Ambassador Anatoly Dobrynin (after delivering the official ultimatum): namely that American missiles in Turkey and Italy would be removed "within a short time after the crisis was over."

There is no doubt that Allison's multiconceptual approach adds new insights and speculative dimensions to our understanding of the complex process of decision making during foreign-policy crises which will prove invaluable for analyzing present and future ones. This study asks that diplomatic historians stop assuming that rational goals account for a nation's choice of action and begin to concentrate on the intragovernmental factors that determine any given foreign-policy outcome. However, it should be noted that the second and third models require information not readily, if ever, available about recent developments in foreign affairs, let alone past ones. Also, as far as the Cuban missile crisis is concerned, none of Allison's hypothetical analysts address themselves to the nagging question of the role that the forthcoming congressional elections may or may not have played either within the ExCom itself or among the much smaller group of the president's personal advisers. None of the process models, with the possible exception of the third one, seems to be designed to account for the ubiquitous ways in which "important interest groups" (technically outside of government, like various segments of the business community) influence diplomatic decisions.

Allison would have us believe that the formulation of foreign policy, at least in time of extreme crisis, is conducted by small *ad hoc* groups in both the Soviet Union and the United States, which attempt to cut themselves off from governmental bureaucracy and other influential pressure groups. Yet his evidence in the case studies for the second and third process models suggests how impossible this at-

tempt proves in practice, since "members of any organization, particularly career officials, come to believe that the health of their organization is vital to the national interest . . . [and] the President and senior players will almost always be concerned about domestic implications." There is no confusion, however, about the different lessons drawn from the three process models. While the first one leads to the conclusion that nuclear crises are manageable, the second and third ones clearly demonstrate how easy it would be for nations to stumble into a nuclear exchange because of the heretofore little understood risks in the decision-making process during diplomatic crises.

To Louise FitzSimons all the foreign-policy actions of the Kennedy administration add up not only to a defense of the international status quo, but also to an intensification of the cold war through the introduction of counterinsurgency tactics and nuclear brinkmanship. With particular reference to the emplacement of Soviet missiles in Cuba FitzSimons concludes that the crisis was as dangerous as it was unnecessary. These same charges were made by I. F. Stone and Ronald Steel several years ago and are based on the argument that Kennedy arbitrarily raised the incident to a high-level crisis because of his excessive concern for his own and his country's prestige (in the wake of the Bay of Pigs and his poor showing at Vienna with Khrushchev) and because of the November mid-term elections. Thus he bypassed traditional diplomatic contacts in favor of "eye-ball to eye-ball" confrontation. Like Stone and Steel, FitzSimons reaches this conclusion through logical deduction (Allison's model 1) rather than through documentary proof or detailed analysis of the decision-making process.

Generally speaking, FitzSimons too narrowly ascribes to the Kennedy administration alone an "untenable globalism" that led to "increased areas of conflict, to a heightening of the arms race, and to American concern with and involvement . . . in the affairs of almost every country of the world." This is in essence the Kennedy Doctrine. It is based on the assumption that since "we can . . . we should affect the course of events around the globe." While Kennedy indeed was a cold warrior it is questionable whether his counterrevolutionary



foreign policies were qualitatively (rather than simply quantitatively) different from all such presidential warriors since 1945. Richard J. Walton's book, *Cold War and Counterrevolution* (1972), is more balanced and better documented on this point.

Aside from exaggerations about how Kennedy exacerbated the cold war single-handedly, FitzSimons, a former foreign affairs officer for the Atomic Energy Commission and staff assistant in the Senate, generally makes judicious use of all the published secondary accounts of the Kennedy years plus congressional documents, the oral history collection at the JFK Library, and the one-volume Bantam edition of the Pentagon Papers. For obvious reasons this work does not accord equal attention to the more positive aspects of Kennedy's policies in Africa and Latin America. For other areas of the world, however, it provides a needed counterweight to the over-apologetic accounts of Kennedy's foreign policy by Sorensen, Schlesinger, Hilsman, and others, which have been the standard fare to date. She also rightly laments the fact that Kennedy used his charisma to stimulate cold war attitudes among Americans rather than to eradicate them.

In contrast to the works of Allison and FitzSimons, the McSherry book is an intellectual disaster. It is both the author's and publisher's intent to present "a serious, conservatively oriented book," despite the difficulties inherent in doing so because of the "growth of lock-step liberalism in the past thirty years." Unfortunately they have not succeeded. Based on inadequate research (primarily newspaper accounts and a handful of secondary sources) this book does not attempt the systematic analysis of the diplomatic relationship between Kennedy and Khrushchev that the title implies. Instead, the president surfaces only sporadically in what is really a superficial summary of Soviet policy from 1957 to 1964.

A former State Department intelligence specialist during the Eisenhower administration, McSherry drags out all of the cold war clichés from the domino theory to the fact that all Communist nations are equally untrustworthy and only understand force. Hence, approving of Kennedy's forceful handling of the Cuban missile crisis, McSherry views it simplistically as a personal attempt by Khrushchev "to

expose Uncle Sam as toothless and feeble" under the leadership of a young "tissue-paper president." The writing is disjointed and rambling, which makes the vague and poorly documented charges against Khrushchev and "mush-minded" American liberals all the less convincing. Contrary to his intent McSherry has not strengthened the scholarly reputation of the Far Right with this book.

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LYNDON BAINES JOHNSON. *The Vantage Point: Perspectives of the Presidency, 1963-1969*. New York: Holt, Rinehart and Winston. 1971. Pp. x, 636. \$15.00.

LADY BIRD JOHNSON. *A White House Diary*. New York: Holt, Rinehart and Winston. 1970. Pp. ix, 806. \$10.95.

Historians will long quarrel about the extent to which ghosts intrude between the authors of these two books and the published works, but such quibbling will be beside the point. Each book says what its author wants to have said, and each, beyond question, has the flavor, both linguistic and moral, of its author. The nearly simultaneous publication so soon after the event reveals a joint urgent need for apologia, but neither does this diminish the significance of the two books. Indeed the manner in which the apologiae are presented enhances the significance of these court reminiscences.

Mrs. Johnson (one might say "Ms." in deference to the South, but that would be mistaken as a nod to Women's Lib, whose principles seem to be pretty well rejected by Lady Bird Johnson's whole approach to her husband and family) recounts that on their arrival at the White House on December 7, 1963, "Mr. West was waiting at the door for us. . . . He has worked at the White House for twenty-three years and is now Head Chamberlain." Conversely, for January 20, 1969, Lyndon Johnson reflects: "the last morning I would wake up in this room, the last day I would live in this house. It was the day the mantle of the American Presidency would be shifted, in one of the greatest pageants of our democracy, from my shoulders to those of Richard Nixon. . . . Even in those final hours there could be no departure

from the routine. Presidents come and go, but the Presidency goes on without interruption."

Despite the intense interest in the politics of the Johnson regime, I suspect that the long-range historical interest of these two documents will lie far more in what they reveal about the nature of the American presidency than in anything they can tell us about the politics of the Great Society or the "war" in Vietnam. President Johnson refers to the "pageants" of the American democracy, but what he really seems to be saying is: "The king is dead; long live the king." The principal qualification, perhaps, is that being still alive, he can take several sharp pokes at the succeeding monarch for allegedly undercutting Johnson's Vietnam diplomacy during the extended lame-duck period.

We read in these records the account not just of a democratic presidency (in all its facets) but of an *imperial* democracy. It is impossible not to recall the strictures of Woodrow Wilson's favorite constitutional commentator, Walter Bagehot, who argued the desirability of separating the "dignified" from the "efficient" elements in a constitution. Wilson, for all his basic conservatism, never quite grasped this point, although he did see the point about the need to create "responsible government" by offering to resign upon defeat—and, perhaps not surprisingly, it was his second Southern successor, Johnson, who put the precept into practice. And Johnson put it into practice principally because Wilson, F. D. Roosevelt, and Harry Truman—his prime mentors in the presidential craft—had presided over the evolution of America from an expansionist to a defensive empire. Johnson, in a very real sense, suffered the consequences of presiding over a sated empire—an empire that had never suffered a serious military defeat (bar the controversial outcome of 1812–14), an empire that had enforced its sphere of influence in the Americas, made the Pacific "an American lake," and accepted from declining European empires the responsibility of keeping secure a world-wide market society. Defeat in these circumstances left no alternative to abdication.

For the real history of the Vietnam War one has to go to other sources. But for the impact of the war (and of crises in the Caribbean, the Middle East, and Central Europe) upon the

presidency these two books will remain primary sources. Mrs. Johnson's book, especially, records the interlocking burdens imposed upon the president by virtue of his being at once head of state and chief executive. Her account of the White House as court, despite her own remarkable sharing of the burdens and opportunities, leaves no doubt about the immense extra drain on one man's energies (and competence?) when he has to be both king and prime minister. And the "dignified" as opposed to the "efficient" aspect of his office was intensely absorbing for Lyndon Johnson.

A world-shocking tragedy shot Johnson into an office for which, in ordinary circumstances, he would have been as well prepared as had been most of his predecessors. But the circumstances cast him in the role of "populist" usurper. A prince had been murdered, and the horrified chorus of criticism seems, in retrospect, to have been more paranoiac than any paranoia as yet ascribed to American populists. It is little wonder that both of the Johnson books devote much space to their relationship with the Kennedys—which both books depict as one of warm mutual regard. Nor is it surprising that Lyndon Johnson (like the first Roosevelt) swore to carry out his assassinated predecessor's policies intact. In the domestic field, as Johnson records with loving attention, he accomplished this—with more success than would likely have been the case had the event in Dallas never occurred (although Johnson does not put it quite this way). In the field of foreign affairs, Johnson is even more careful to point to the unbroken thread of policy: "I would devote every hour of every day during the remainder of John Kennedy's unfulfilled term to achieving the goals he had set. That meant seeing things through in Vietnam as well as coping with the many other international and domestic problems he had faced. I made this promise not out of blind loyalty but because I was convinced that the broad lines of his policy, in Southeast Asia and elsewhere, had been right. They were consistent with the goals the United States had been trying to accomplish in the world since 1945."

The circumstances of the succession account for much, as does the nature of the office itself. Yet the background of the Johnsons themselves and the felt need to "justify" the South also

bulk large. Lyndon Johnson was deeply concerned with this latter problem (as was his wife, whose favorite politician seems to be Harry Truman). LBJ puts it this way: "The burden of national unity rests heaviest on one man, the President. . . . One reason the country could not rally behind a Southern President, I was convinced, was that the metropolitan press of the Eastern seaboard would never permit it. My experience in office had confirmed this reaction. I was not thinking just of the derisive articles about my style, my clothes, my manner, my accent, and my family—although I admit I received enough of that kind of treatment in my first few months as President to last a lifetime. I was also thinking of a more deep-seated and far-reaching attitude—a disdain for the South that seems to be woven into the fabric of Northern experience."

LBJ certainly brought with him to the White House much that shocked the Eastern establishment. But he also brought a Southern political know-how that in sheer managerial skill should have put that establishment to shame. What the cold-war liberal sophisticates of the East seemed not to recognize, and what is made very clear in *The Vantage Point*, is that the Southern populist had bought their own goals. What he brought to the job was a power of simplification that the Easterners found distasteful and even vulgar. In following through the logic of their own policies the usurping president revealed the limits of power and the need to call upon an essentially illiberal patriotism to justify contradictions between foreign and domestic actions. "Those who created division," writes Johnson, "who opposed decisions, and who made it more difficult to accomplish the job need to reflect on the consequences of their actions." Ultimately he refuted the doubters by very simple questions: "I wonder what the reaction would have been in the America of 1969 if Soviet cosmonauts had planted their red flag on the moon."

KENNETH MCNAUGHT  
University of Toronto

HERBERT L. MATTHEWS. *A World in Revolution: A Newspaperman's Memoir*. New York: Charles Scribner's Sons. 1971. Pp. 462. \$12.50.

Herbert Matthews was one of those fascinating

men who made history while writing it. He repeatedly took positions and reported events that engendered strong reactions by governments and the public. Accused of being pro-fascist for his reports on the Italo-Ethiopian War, attacked by conservatives and Catholics for his reports of Loyalist activities during the Spanish Civil War, and widely condemned as having helped hoodwink the American government into believing Castro was not a Communist, Matthews has written an impressionistic memoir of his own career as a reporter for the *New York Times*—a career that clearly places him within the mainstream of American liberalism.

The book consists of a series of interesting and well-written chapters, each dealing with a country he covered as a reporter. Though he bases his comments largely on his own experiences he does not limit himself to just the time he spent there. Rather, he perceptively uses his past experiences as a means of commenting broadly on each nation and the world at large. His recollection of the ignorance of Hindu and Muslim peasants about the activities of Gandhi and the Muslim League during World War II becomes an opportunity to comment on nationalism as a middle-class emotion and concept. Accusations of bias in his reporting on the Spanish Civil War and the Italo-Ethiopian War bring on a discussion of how a good reporter must combine bias and factual honesty. A brief and relatively quiet tour in Italy from 1939 until 1941 provides the backdrop to an incisive and enlightening interpretation of Italian politics from Mussolini to Togliatti. Through all of this a number of themes are dominant. Matthews is clearly disillusioned with ideology. Communism (he sees all post-Lenin Communists as counterrevolutionaries), fascism, capitalism, socialism—they all seem irrelevant in the face of rampant nationalism. (Spaniards fought each other with little thought given to ideology.) His archenemy is authoritarianism, regardless of the disguise. And his faith is in the young radicals who may not be right but must create their own world in which to live.

The book also provides only a limited amount of useful primary source material, for Matthews has written nine other books in which most of his reminiscences of the three great crises he covered—Abyssinia, the Spanish

Civil War, and Castro—are fully recorded. He does relate the details of a fascinating interview between Ernest Bevin and Arthur Sulzberger on the subject of Truman's recognition of Israel, and there are also occasional quotes and short stories that catch the historian's eye; for example, when Matthews quotes (by hearsay) Robert Murphy in 1944 as saying that communism in Italy was no problem since the Communists "are now part of the democratic community" (p. 137). The real scholarly value of this book, however, is for historians of American journalism. Matthews states at the beginning that he is happy to report at last on his two great struggles (the Spanish Civil War and Castro) with "his" newspaper with complete candor. He defends his "honest bias" and accuses the paper of failing to live up to its own credo when it suppressed his reports on Castro. Yet this is clearly criticism from a member of the family. Equally intriguing is the insight this provides into the relationship between the reporter, who sees only part of the whole but knows what he sees, and the publisher-editor who looks at the whole but sees nothing for himself. During the Spanish Civil War the *New York Times* incensed Matthews when the editors chose to give equal weight to reports on the Loyalists from Matthews at the front and to reports on the Rebels based on official press releases. Then during the early years of Castro in Cuba the paper took the other position and "muzzled" Matthews, in spite of (or because of?) his exclusive access to Castro and other Cuban government leaders. There are other curious revelations, particularly Matthews's offer in 1962 to Ambassador Thomas Mann to report back any items of interest that came up during his projected talks with the Cuban leaders; that is, he would check back with the CIA!

There is no doubt that much of this book is an attempt at self-justification by Matthews, particularly regarding his relationship to Castro, and on that subject Matthews's arguments are persuasive. The popular notion of Matthews single-handedly influencing the State Department's image of Castro is asinine on the face of it. The book is also an earnest plea for the value and validity of newspaper reporting. He quotes Arthur Schlesinger, Jr.'s comment before the American Historical Association to the effect that, after having served in the White

House, he would no longer take newspaper and magazine articles seriously. Matthews archly points out that if one "cannot distinguish good journalism from bad, or trustworthy newspapermen from phonies, he should not try to write contemporary history" (p. 8). But for Herbert Matthews separating the honest from the dishonest in the stories he covered is simple, for he knows what he saw. For historians it is not so easy, for like publishers and editors, we must look at all sides and always through the eyes of others.

WARREN F. KIMBALL  
Rutgers University,  
Newark

ROBIN W. WINKS. *The Blacks in Canada: A History*. New Haven: Yale University Press. 1971. Pp. xvii, 546. \$15.00.

This is the first comprehensive history of Canadian Negroes—from the arrival of the first African slave in New France in 1628 to the scattered stirrings of black protest activity in the late 1960s. It is an extraordinary achievement, impressive as much for its wide-ranging breadth as for its scholarly depth and thoughtful insights. It is far more than black history, moreover, for Professor Winks deals importantly with Canadian, Canadian-American, and French and British imperial history as well. The black presence in Canada has never been as significant as in the United States, from which many Negroes originally came, for blacks have never amounted to more than five per cent of the population of any province and only about two per cent of the nation as a whole. Scattered from the Maritimes to Vancouver, the product of several distinct waves of immigration widely separated in time and area, for the most part poorly educated and overwhelmingly lower-class throughout their history, isolated both from the dominant bicultural white society and from each other, Canadian Negroes have had even less success than their black brothers in the United States in gaining an acceptable and accepted place in Canadian life. Too few in number to have more than local visibility, too scattered to have an effective press or national leaders and organizations, the blacks have largely been ignored by white Canadians, who smugly and er-

roneously assumed that racial problems and prejudice existed only south of their border. In Winks, Canada's blacks have found a sympathetic and indefatigable chronicler, who over more than a decade mastered an enormous range of chiefly unpublished sources scattered throughout Canada and the United States as well as in the United Kingdom, France, the West Indies, and even West Africa. Since only about half of his research materials were incorporated into this book, Winks has deposited his notes, correspondence, and other papers, including early and more extensively documented drafts of the manuscript, in the Schomburg Collection of the New York Public Library for use by other scholars. Although he modestly points out that because of its pioneering character this cannot be regarded as a definitive study, it is unlikely to be replaced soon. Indeed, it may become a Canadian equivalent to Gunnar Myrdal's epochal *An American Dilemma*, by calling attention to the long and as yet unrealized struggle of Canadian Negroes to become Negro Canadians.

E. DAVID CRONON  
University of Wisconsin,  
Madison

FREDRICK B. PIKE. *Hispanismo, 1898-1936: Spanish Conservatives and Liberals and Their Relations with Spanish America*. (International Studies of the Committee on International Relations, University of Notre Dame.) Notre Dame: University of Notre Dame Press. 1971. Pp. xx, 486. \$15.00.

The year 1898 brought to an end Spain's overseas empire and stimulated Spanish intellectuals and statesmen to probe the causes of national decadence and to reappraise their country's place in world affairs. This year of disaster and despair for Spain has been chosen by Fredrick B. Pike as the point of departure for an examination of the theory and practice of Pan-Hispanism. In scope this work is much broader than would appear from the narrow definition implied by the title. Not only does the author deal with material going back to the 1870s, but he explores in considerable detail intellectual, social, political, and religious problems of Spain during the past century.

Focusing upon the bitter debate between conservative and liberal Pan-Hispanists, the au-

thor depicts an international movement that was more significant for rhetoric than for practical achievements. Liberals and conservatives, while differing on religious and social issues, agreed on the need to preserve a hierarchically and organically structured society. Democracy and materialism, in their view of the world, were the archenemies of the Hispanic *raza*. If Latin Americans wished to defend themselves from the twin threats of Yankee imperialism and social revolution, in the opinion of *hispanistas*, they were urged to cultivate the traditional values of Spanish spirituality.

Although the ranks of Pan-Hispanic advocacy included such prestigious figures as Rafael Altamira, Ramón Mendéndez Pidal, Miguel Unamuno, Antonio Maura, and Ramiro de Maeztu, the movement never enjoyed more than limited success. From time to time important Spanish Americans like Rubén Darío, José Enrique Rodó, Marcelo T. Alvear, and Manuel Gálvez showed interest in *hispanismo*, but the establishment of close political, economic, and cultural ties between Spain and her former colonies eluded the advocates of Hispanic cordiality.

Pike puts his finger on the basic shortcomings of Spanish leadership. An abrasive paternalism toward America alienated potential friends of Spain. Unseemly quarrels among conservatives and liberals reduced effectiveness. Lack of a sustained governmental commitment and Spanish weakness as a world power handicapped *hispanoamericanistas* in their efforts to compete with the United States and European powers in Latin America. The gradual drift of *hispanista* ideology to the political right limited the movement's appeal almost entirely to the most conservative elements in the New World.

The documentation offered by Professor Pike is extensive, varied, and convincing. Historical interpretations are perceptive and judicious throughout. An attempt to show connections between twentieth-century *hispanismo* and the Pan-Hispanic movement before 1870 would have given greater historical depth to this study, but this is a minor criticism that subtracts little from the overall excellence of Mr. Pike's scholarship.

MARK J. VAN AKEN  
California State University,  
Hayward

DAVID GREEN. *The Containment of Latin America: A History of the Myths and Realities of the Good Neighbor Policy*. Chicago: Quadrangle Books. 1971. Pp. xiii, 370. \$10.00.

This is a troubling book, though not entirely for the reasons the author intended. Part of the difficulty may be that David Green attempted to analyze several somewhat different, if related topics and did not altogether succeed with any of them. The preface, for instance, promises a revisionist analysis of "the myths and realities of the Good Neighbor Policy," but it is regrettable that the author tells us little new about this promising topic.

Green also discusses recent United States foreign economic policy and economic relations with Latin America. There is worthwhile material in chapter three on the Inter-American and Export-Import banks, chapter 4 (perhaps the best in the book) on the economic impact of the war, and parts of chapters 7 and 8. Elsewhere the author fails to provide a systematic economic analysis, despite his underlying and unproven assumptions about United States economic motives.

Green's arresting major contention is that the United States feared and persistently attempted to thwart Latin American nationalistic movements. He holds the New Deal's "relationship to private interests" responsible for the ultimate "tragedy," though at one point he concedes that the intentions of Presidents Roosevelt and Truman were benevolent. Otherwise Green implies insistently that even the motives were wrong. Thus he asserts that the United States consciously "retained effective control" over Cuba after 1933 and that Secretary Hull was primarily interested in securing markets.

Here Green largely ignores the effect of the depression on Washington's policies and misrepresents Hull's ultimate goals. Later he reads economic expansionism into Roosevelt's remark that "Dr. Win-the-War" had to replace "Dr. New Deal." Again Green claims that the United States subsequently neglected Latin America because of a shift "by mid-1944" of "economic resources into Western Europe for anti-Russian activities." Truman accordingly appears as a man of views "crude in the extreme" who preferred "confrontation," while Bernard Baruch (whose influence Green exag-

gerates), Nelson Rockefeller, and Arthur Vandenberg figure variously among the cast of villains. With even scantier evidence Green disposes of President Kennedy as a conscious heir to an antinationalist policy and an advocate of "containing" Latin America through the Alliance for Progress.

Insufficient research partly explains some of Green's errors, as in his misleading account of wartime Argentina. But the book also suffers from indiscriminating idealization of nationalism and from an unfortunate attempt to prove too much. Green, however, has properly identified an important opportunity for historical analysis and criticism of policy. This challenge remains for scholars.

PAUL S. HOLBO  
*University of Oregon*

BRIAN R. HAMNETT. *Politics and Trade in Southern Mexico, 1750-1821*. (Cambridge Latin American Studies, Number 12.) New York: Cambridge University Press. 1971. Pp. viii, 214. \$12.50.

This important study concerns the attempt to control the lucrative cochineal trade of Oaxaca. It not only provides considerable information about the economic and political aspects of the eighteenth-century Bourbon reform program and its implementation but also gives a thorough discussion of the dye trade, which was, after bullion, the most important product exported from New Spain in the colonial era.

The red-dye cochineal was controlled by merchants in Mexico City, who, working closely with firms in Spain and through the local *alcaldes mayores*, exploited Indian labor, financed the cultivation and manufacture of the dye, and marketed the product in Europe. Cochineal brought such handsome profits to the merchants and production boomed to such an extent that the metropolitan government, ever desirous of more revenue, attempted to smash their monopoly and to tax the production by implementing administrative reforms and free trade. Hamnett explores in detail the many facets of the struggle to control the cochineal industry: the reform-minded administrators versus the traditionalist bureaucrats; the Mexico City merchants versus the Veracruz

Consulado; the Creole interests of the independence era versus the peninsulars; and the Church hierarchy in Oaxaca, which first supported the reform government and the intendancy system, thinking the Indians would be less exploited, and later threw its support to those elements within the government that wished to see the trade continued under the control of the Mexico City merchants.

All the while Oaxacan cochineal production was falling drastically, from a high of one and one-half million pounds in 1774 to 300,000 pounds in 1821. The drop was caused in part by government action, the insecurity of commerce between Spain and her possessions during the colonial wars, the chaotic economic conditions resulting from the struggle for independence, and ultimately the competition presented by other areas into which cochineal cultivation was introduced.

Hamnett has based his study largely on archival materials in Spain and Mexico. Lacking only in some analysis of the grass-roots implications, particularly within the Indian society, of this politico-economic struggle the work is otherwise thorough and excellent.

CHARLES R. BERRY

Wright State University

MOISÉS GONZÁLEZ NAVARRO. *Raza y tierra: La guerra de castas y el henequén*. (Centro de Estudios Históricos, New Series, Number 10.) [México, D. F.:] Colegio de México. 1970. Pp. x, 392.

Previous works by this outstanding Mexican historian have been noted for methodical and objective use of primary sources and first-hand statistical data, for example, *Estadísticas sociales del Porfiriato* (1956), *El Porfiriato. La vida social* (Volume 4 of *Historia moderna de México* [1959]), or *La colonización en México* (1960). One can now add another title to this list of basic reference works for modern Mexican history. *Raza y tierra* is based on archival searches in Mexico City, Merida, La Habana, Madrid, in the Foreign Office in London, and on an impressive number of printed reports, contemporary accounts, and secondary works. In the author's usual style—a kind of military ordering of all relevant information—the text is tightly packed with factual footnoted information and then reinforced with statistical ta-

bles and a rear-guard appendix of supporting documents, all serving to give an impression of overwhelming evidence.

*Raza y tierra* is actually four interrelated studies in Yucatecan history. The first one traces the unfolding of the tragic Caste War that burst into flames in 1848 and finally sputtered out in the early twentieth century. This part of the book serves as an important supplement to Nelson Reed's *The Caste War of Yucatan* (1964). The second study represents rare archival research on the Yucatecan slave traffic to Cuba, 1848–61. British abolitionist policy inhibited this business, but, according to González Navarro, it was the triumph of Juárez and, more important, the rise of labor-hungry henequen plantations that actually put an end to it. The third part is concerned with labor conditions and labor legislation in Yucatan. Here the author demonstrates that in spite of reforms that modified Indian peonage obligations, Mexican *gachupines* continued the Spanish colonial tradition of hacienda managers acting with police powers to pursue runaways, vagrants, and debtors and to administer corporal punishment. An old Spanish proverb still described the system: "Los indios no oyen sino por las nalgas." In essence there is agreement with the account of Indian servitude given by John K. Turner in his sensational exposé *Barbarous Mexico* (1911). The final section provides a statistically detailed account of henequen production and agrarian reform in Yucatan from the revolution of 1910 to the present, with emphasis on monoculture, collectivized *ejidos*, and peasant proprietorship.

Documentary attachments, a full bibliography, and an index by name and subject add the finishing touches to this scholarly production. Any shortcomings in a work by González Navarro must be taken with its virtues. The writer lets the facts, wet or dry, speak for themselves and only rarely splices in his own interpretive opinions, impressions, or comments.

ARTHUR F. CORWIN

University of Connecticut

RALPH DELLA CAVA. *Miracle at Joazeiro*. (Institute of Latin American Studies, Columbia University.) New York: Columbia University Press. 1970. Pp. xiv, 324. \$9.95.

There is no greater test of the historian's craft than the need to sift through conflicting and highly partisan secondary sources. To be the first investigator who also consults unpublished documents is an even greater opportunity. Ralph della Cava has met the challenge admirably in this painstakingly researched monograph.

*Miracle at Joazeiro* reconstructs the story of an extraordinary popular religious movement in an interior town of the impoverished northeastern Brazilian state of Ceará between 1889 and 1934. The central figure was Padre Cícero, a devout and remarkably long-lived priest, twice excommunicated for failing to obey the ecclesiastical authorities who had declared fraudulent the "miracles" alleged to have occurred at the humble cleric's mass. Despite condemnation by the "Romanizing" Church, Padre Cícero rapidly became a saint to the thousands of destitute pilgrims who flocked to Joazeiro. In time the venerated priest became the most powerful political figure in the northeast and a fierce promoter of his adopted city of Joazeiro. Not surprisingly, he acquired many enemies, both among the clergy and outside observers who insisted on seeing only "fanaticism" in a movement whose deep social roots della Cava mentions briefly. The author has purposely avoided emphasizing the colorful folkloric aspects, despite obvious sympathy for the underprivileged masses for whom the movement meant most. Instead he concentrates on the difficult task of reconstructing the complex political history of Padre Cícero's relations with the "Romanizing" clergy of the contemporary Brazilian church, the state and national political elites, and José Marrocos and Floro Bartholomeu, the ambitious laymen who played such a key role in building the legendary priest's secular power.

The story is set clearly in the context of regional and national change, both political and economic. The author convincingly demonstrates how the "dualistic" hypothesis of a "civilized coast" and a "primitive interior" is refuted by the case of Joazeiro, which showed close connections with a regional economy and a national political system. Equally important, *Miracle at Joazeiro* offers fascinating insights into the structure and use of power in a community of the Brazilian northeast dur-

ing the Old Republic (1889-1930). One finishes this book with the conviction that it is the most objective and carefully documented political history of the movement that could now be written. Its analytical clarity and restrained style were made possible by the consultation of unpublished documents and oral testimony available only to a patient researcher willing to settle down in an interior city and listen.

THOMAS E. SKIDMORE  
University of Wisconsin,  
Madison

ADLAI F. ARNOLD. *Foundations of an Agricultural Policy in Paraguay*. (Praeger Special Studies in International Economics and Development.) New York: Praeger Publishers. 1971. Pp. xx, 294. \$17.50.

An attempt to establish bases upon which an agricultural policy may be developed for Paraguay is certain to encounter monumental difficulties. To explain these obstacles, the author provides a short geographical description of the country, a brief summary of political history, and a disappointing history of land tenure. These three introductory chapters fail to provide the background needed for understanding contemporary conditions. If the reader will turn first to Joseph Pincus, *The Economy of Paraguay*, published in this series in 1968, the gap can be filled, and Arnold's study will serve as an excellent elaboration of agricultural problems touched upon briefly by Pincus.

Justification for the book is found in the last four chapters. An examination of the minifundia-latifundia problem and the work of the Institute of Rural Welfare (IBR) suggests that there is no immediate prospect that the IBR can fulfill its mission. A chapter on "Paraguayan Plans for Economic Development and their Implementation" describes the objectives of three plans launched in 1965, 1967, and 1969. The last of these plans could result in great benefits if accompanied by what would be miraculous changes in Paraguayan bureaucracy. The problems have been identified, the solutions have been plotted; but implementation has failed miserably, except in a few cases such as road building.

The heart of the volume is chapter 6, "An



Approach to Agricultural Development." Here the author correctly endorses the agricultural infrastructure approach that emphasizes transportation, storage, many government services, and extensive contributions from the private sector. The concluding chapter attributes Paraguay's backwardness primarily to its "turbulent history" in which there was almost no road building and property rights were concentrated in very few hands. Most readers with a firm grasp of Paraguayan history will agree with the conclusion that "a strong central government may be the only alternative to anarchy and chaos" (p. 160).

One should emphasize that this is not a history of Paraguayan agriculture but an elaboration of what Paraguay must do in order to develop its agricultural resources. The author, who served in Paraguay with USAID from 1965 to 1970, has a thorough understanding of problems faced by agrarian reformers. His analysis of Paraguay's principal agrarian laws and the plan for 1969-73 show that Paraguayans know what to do; but it also raises the question of whether they will do it. The author is understandably pessimistic.

HARRIS GAYLORD WARREN  
*Santa Fe, New Mexico*

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

In its two central points Henry F. May's review of my *An Unsettled People* (*AHR*, 77 [1972]: 578-81) rather oversteps reasonable bounds. He appears to wish that I had written a different category of book, and—much worse because less obvious—he complains that the one I did write fails to prove a thesis that is, however, no part of it.

The book is frankly a synthetic effort; why should it not include "textbook topics" or cite "standard secondary works"? (It draws on primary sources, too, and could have cited more of them, but Professor May misreads my introductory remarks on sources and citations.) The critical question here surely is not whether the data, or even parts of the argument, are al-

ready familiar—indeed, the more familiar the better, if the sense made of them is fresh and valid. If a reviewer claims to find little more in a synthesis of social history than "the old miscellany," he might at least be expected to give a few examples of its failure to integrate conventional topics (Professor May merely names a few of the latter). But I may misconstrue his point. Elsewhere in the review he quite effectively summarizes, with overall approval, the synthesis of leading topics, both novel and familiar. Perhaps he would have preferred a monograph, such as he urges me to write, or else a historiographical disquisition on other people's theories, models, and controversies, though he does not specify what "new ideas, information, and events" one should consider. But there is more than one way to ask "one's own questions" of history, and I hope the book will suggest some to monograph writers and followers of trends.

Professor May's other stumbling block is his recollection, flattering but unfortunate, of a conservative social hypothesis that I proposed in 1960 (*AHR*, 65 [1959-60]: 495-514). As he observes (quoting that article at much greater length than the book), it included a three-phase cycle running from colonial stability through nineteenth-century instability, anxiety, and reaction and back once again to relative stability in the twentieth century. The article may have been an "arresting skeleton," but the book written in the intervening eleven years does not, I think, represent arrested development; it explicitly reverses the last part of the 1960 scheme. The first two parts, fleshed out, remain generally acceptable to Professor May. But when he comes to the last phase, he complains not only that the proof which he so long

though skeptically awaited is missing, but that the book actually proves "again and again" that, just as he suspected, American society has *not* perceptibly regained stability. (He also chides me for having failed to re-examine my hypothesis!) But the book is not the article, and sufficient to each are the errors thereof.

The most casual reader might well discount, on Professor May's own evidence, the first of his objections. But as it would take a reading of the book itself to reveal that his second line of criticism misrepresents its revised but still radically conservative argument, I trust that the review will dissuade no one from examining it.

ROWLAND BERTHOFF  
*Washington University*

PROFESSOR MAY REPLIES:

I hope the same.

HENRY F. MAY  
*University of California,  
Berkeley*

TO THE EDITOR:

I hope you will allow me to correct an egregious lapse in my review essay on Stuart parliamentary history (*AHR*, 77 [1972]: 705-14). The sentence in question read: "Bernard Bailyn has recently stimulated a major re-examination of the American Revolution by suggesting that Locke really did matter to the colonists"—the very reverse of Bailyn's thesis. What I should have written is that *political ideas* really did matter, not Locke, and I must apologize to Professor Bailyn for misrepresenting his views.

THEODORE K. RABB  
*Princeton University*

## Other Books Received

Books listed were received by the *AHR* between May 1 and July 15, 1972. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

ALEXANDER, EDWARD PORTER (ed., with an introd.). *The Journal of John Fontaine: An Irish Huguenot Son in Spain and Virginia, 1710-1719*. Williamsburg: Colonial Williamsburg Foundation; distrib. by the University Press of Virginia. 1972. Pp. xii, 190. \$4.95.

ALVES, RUBEM A. *Tomorrow's Child: Imagination, Creativity, and the Rebirth of Culture*. New York: Harper and Row. 1972. Pp. 210. \$6.95.

ANDERSON, M. S. *The Ascendancy of Europe: Aspects of European History, 1815-1914*. [London:] Longman. 1972. Pp. xi, 332. £1.75.

BARLETTA, EDVIGE ALEANDRI (ed.). *La depositaria del concilio di Trento*. Vol. 1, *Il registro di Antonio Manelli, 1545-1549*. Ministero dell'Interno, Pubblicazioni degli archivi di stato. Fonti e sussidi, No. 1. Archivio di stato di Roma. Rome: the Ministero. 1970. Pp. xi, 435. L. 5,500.

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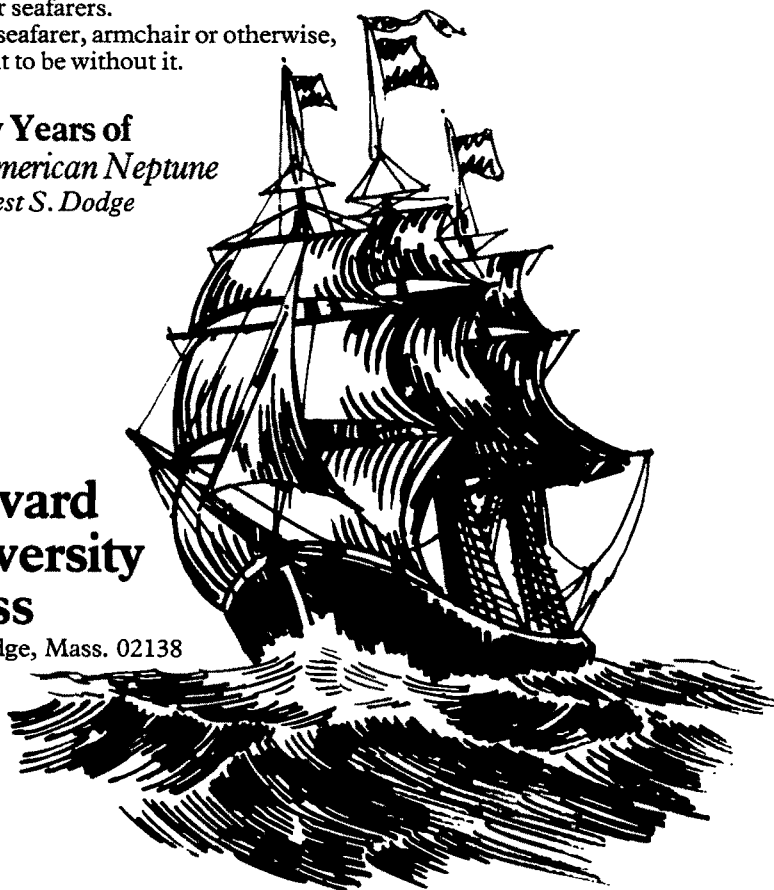
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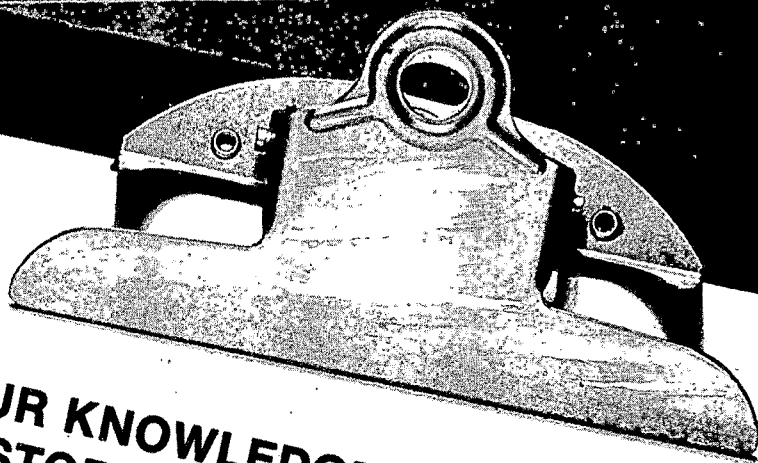
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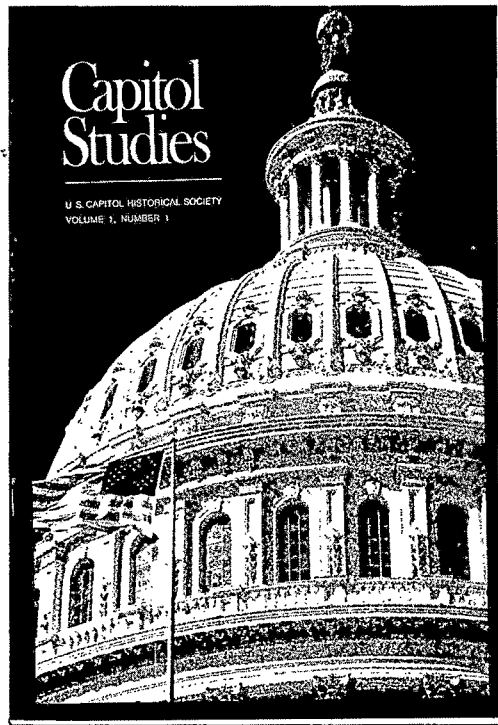
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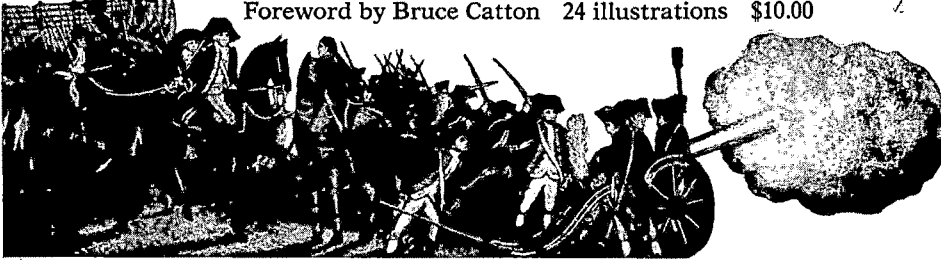
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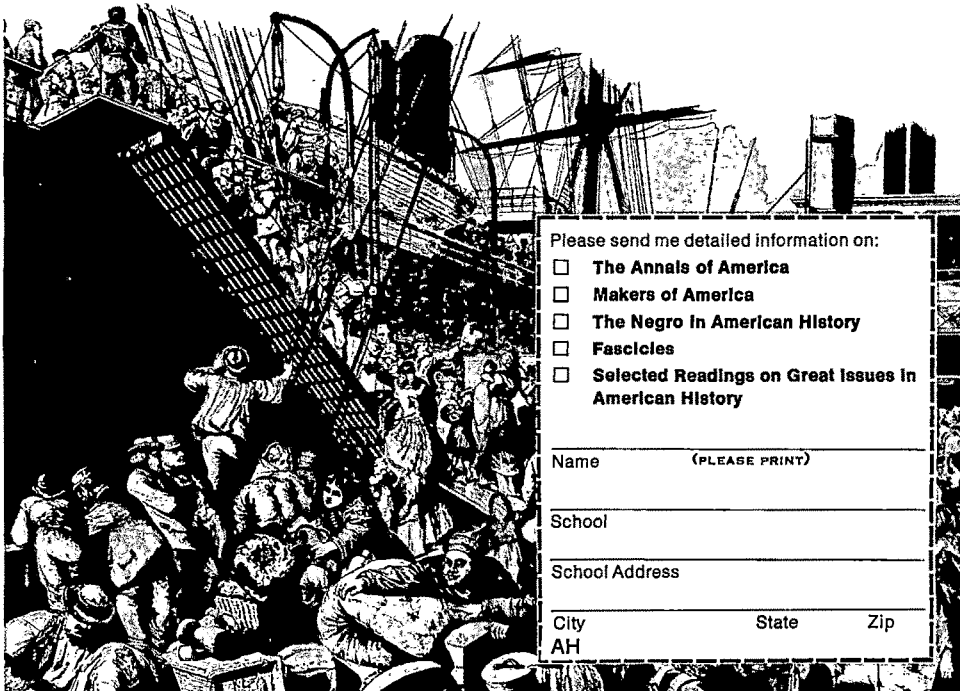
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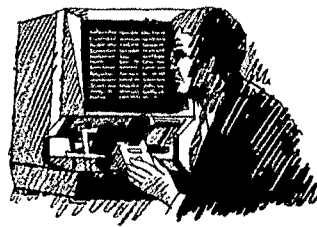
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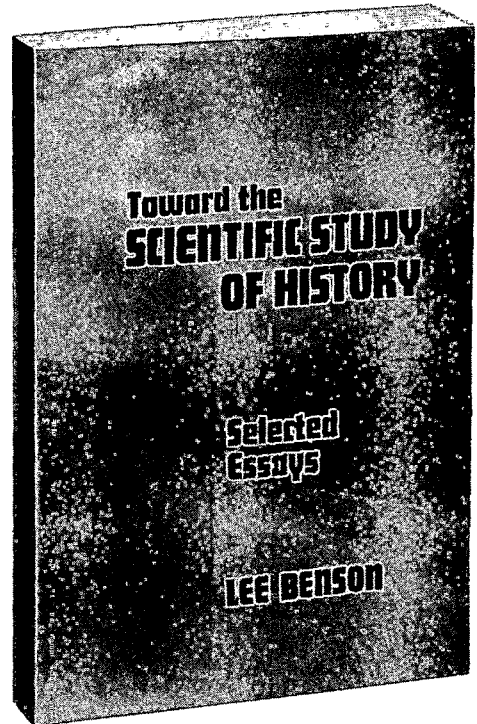
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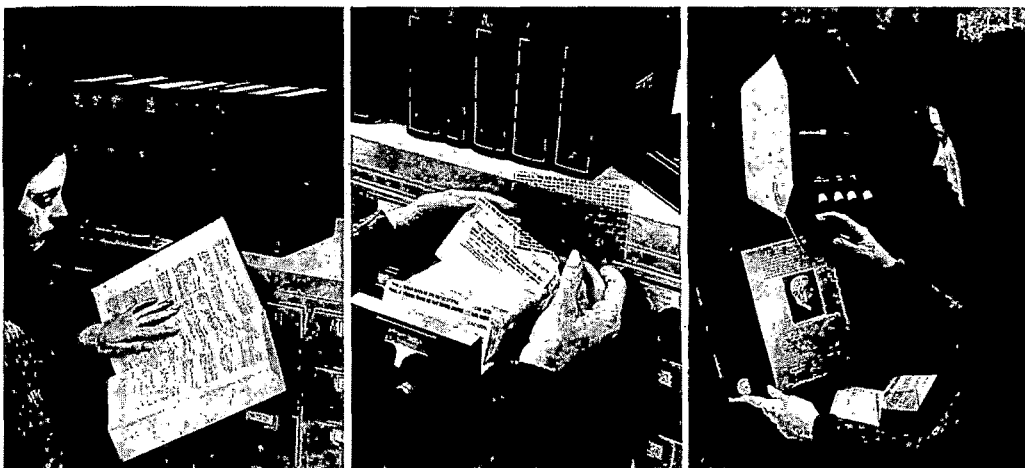


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